



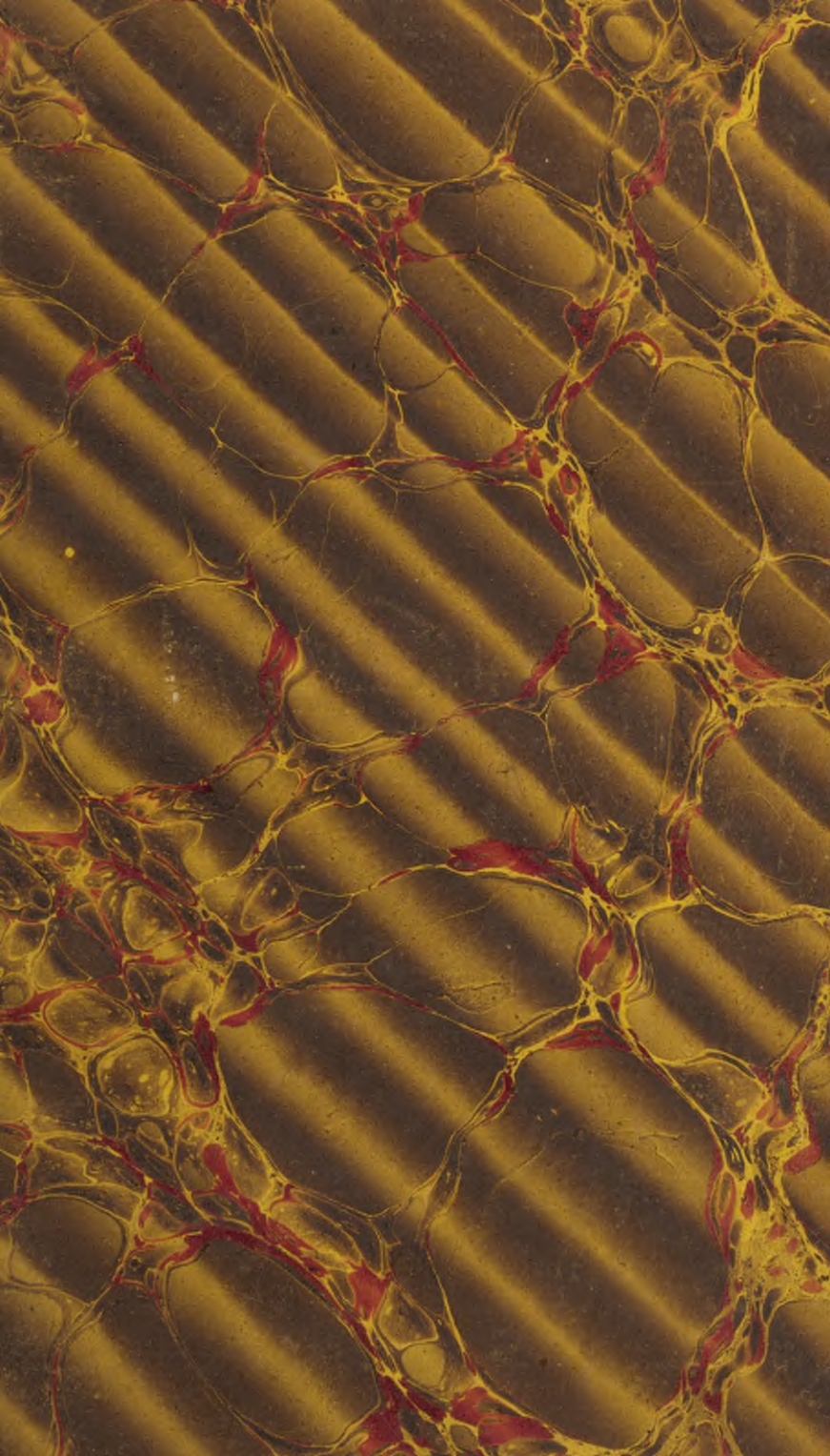
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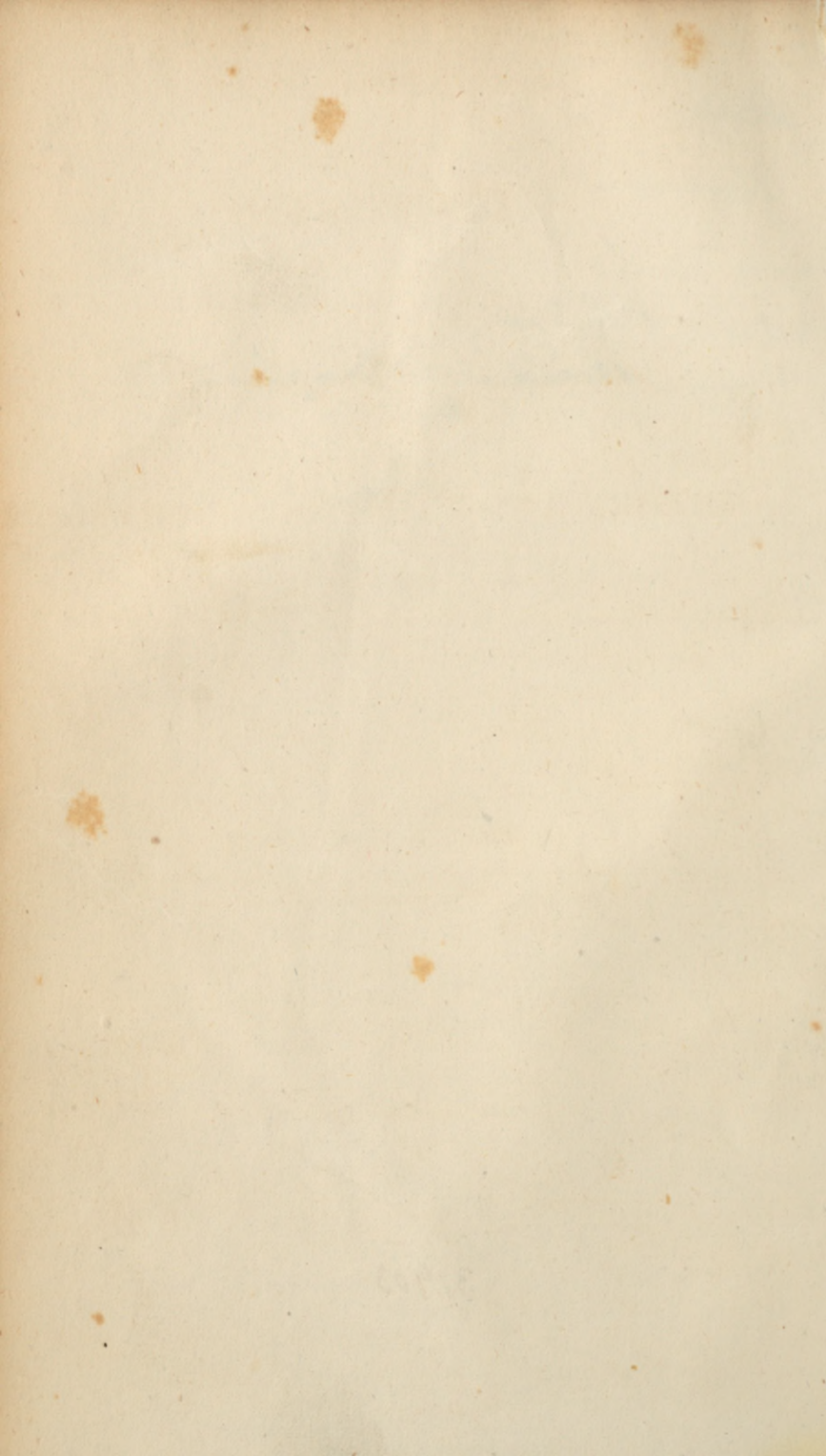
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LECTURES

ON THE

THEORY AND PRACTICE OF PHYSIC.

BY WILLIAM STOKES, M. D.

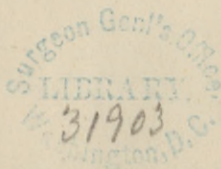
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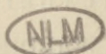
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NOTICE.

The present is the first edition of the embodied lectures of Dr. Stokes. They were originally published in the "London Medical and Surgical Journal," and were necessarily dispersed over several numbers. Detached lectures have been occasionally reprinted in the different medical periodicals of this country; and their excellence has been so manifest, that the particular numbers of the British periodical in which they first appeared have been imported solely on their account. The editor of the "American Medical Library" has consequently thought that he should be rendering a service to his professional brethren by issuing them in the present form. He is not aware of any work in which the topics treated of are displayed in a more attractive, and at the same time a more instructive, manner. The author, from his situation, has every opportunity for confirming or disproving his theoretical considerations by the test of practice; he is well informed on every thing that has been done, and is doing, in various countries; and he possesses, withal, powers of discrimination and exposition with which few are favoured. On all these accounts, his lectures will be found peculiarly valuable.

ROBLEY DUNGLISON.

Philadelphia, June 15th, 1837.

CONTENTS.

LECTURE I.

General observations. 1

LECTURE II.

General remarks on local diseases—Fixed rules for the guidance of students—Great importance of diagnosis—Existence of pure fever rare—Doctrine of the Humoralists and of the Brownists—Pathology of the digestive system. 13

LECTURE III.

Pathology and treatment of diseases of the digestive system—Different forms of gastritis—Pathology of this disease imperfectly understood by the ancients—Gastritis and enteritis not always found in connection—Phenomena characterising acute gastritis—Symptoms and sympathetic relations—Diagnosis—Gastritis simulating other diseases. 22

LECTURE IV.

Gastritis—No one symptom decidedly indicative of the particular condition of any organ—Sympathetic irritation liable to terminate in organic disease—Sympathetic relations as connected with the viscera of the thorax—Treatment of simple acute gastritis—Antiphlogistic remedies—Purgative medicines injurious—Enemas and injections—Use of ice beneficial—Effervescing medicines hurtful. 31

LECTURE V.

Pathology and treatment of gastritis—Application of blisters—Emetics can be seldom used in acute gastritis—Hæmatemesis and delirium tremens complicated with gastritis—Treatment of these affections—Dyspepsia, or chronic gastritis—Hypochondriasis—Termination of chronic gastritis. 40

LECTURE VI.

Treatment of chronic gastritis—Frequent excitement of the vascular system necessary to the performance of the functions of the stomach—Local bleeding—Regimen—Counter-irritation over the stomach—Treatment of Broussais—Use of vegetable tonics—Oxide of bismuth—Acetate of morphia. 48

LECTURE VII.

Friction with croton oil—Attention to diet during convalescence—Organic disease of the stomach—Principles of treatment—Diet and attention to the bowels—Duodenitis—Inflammation of the jejunum—Ileitis, complication and nature of—Dothinenteritis—Ulceration of the mucous membrane—Symptoms and diagnosis of ileitis. 56

LECTURE VIII.

Diseases of the small intestines—Symptoms of ileitis—Occurrence of diarrhœa with fever symptomatic of this form of inflammation—Frequency and symptoms of the disease in children—Tabes mesenterica, treatment of. 64

LECTURE IX.

- Treatment of ileitis—Advantage of leeching—Stimuli sometimes beneficial—
 Infantile remittent fever—Inflammation of the mucous membrane—Enteritis
 with diarrhœa—Effects of opium in inflammation of serous and mucous mem-
 branes—Pathology and treatment of diarrhœa and dysentery—Perforation of the
 intestine—Diseases of the large intestine. 72

LECTURE X.

- Diseases of the large intestines—Treatment of diarrhœa—Apyrexial period of
 diarrhœa—Danger in suddenly arresting the discharge—Purging in phthisis—
 Dysentery—Epidemic dysentery. 81

LECTURE XI.

- Sporadic dysentery—Nature of this disease—Treatment; mercurial, stimulating,
 antiphlogistic—Recommendation of Dr. Elliotson—Success of Dr. O'Beirne in
 the use of tobacco injections—Tympanitis, or meteorism—Windy colic, remedies
 for the cure of. 89

LECTURE XII.

- Pathology of jaundice—Its co-existence with a flow of bile—Case of aneurism of
 the hepatic artery—The disease, independent of mechanical construction—
 Colouring of the various parts—Effects on the milk, and humours of the eye—
 Jaundice with preservation of health—Icterus infantum. 97

LECTURE XIII.

- Jaundice from gastro-duodenitis—Researches of Broussais and Marsh on—Jaun-
 dice without hepatic inflammation—Nervous symptoms—Treatment—Yellow
 fever—Its occurrence in this country—Predominance of gastric irritation in
 warm climates—Typhus icterodes—Jaundice from biliary calculi—Different
 situations in which biliary calculi may be found. 105

LECTURE XIV.

- Diagnosis of jaundice from biliary calculi—Proof of the passage of the calculus—
 Indications of treatment—Rupture of the gall-bladder after the use of emetics—
 Spasmodic jaundice—Treatment of spasmodic jaundice—Discharges of fatty
 matter—Researches of Drs. Bright and Elliotson—Connection with malignant
 disease examined—Source of fatty matter. 114

LECTURE XV.

- Acute and chronic hepatitis—Pathological differences—Effect of climate—General
 and local symptoms—Character of fever—Pain of shoulder—Use of pleximeter
 —Complication with jaundice—Resolution—Abscess—Various openings of the
 latter—Cicatrisation. 123

LECTURE XVI.

- Diagnosis of the rupture of hepatic abscess—Pulmonary openings—Case of
 double opening—Puncture of the gall-bladder—Gangrene of the liver—Its con-
 nection with hepatic apoplexy—Diagnosis of distended gall-bladder—Its causes
 —Inflammation of the parietes over the liver—Sympathy of the integuments. 131

LECTURE XVII.

- Aneurism of the hepatic artery—Distension of the liver with bile—Treatment of
 hepatitis—Employment of mercury—Symptoms of suppuration—Dr. Graves's
 operation for giving exit to matter in hepatic abscess—Rupture into the perito-
 neum—Chronic hepatitis—Complication with disease of the heart—Embryonary
 state of the liver. 140

LECTURE XVIII.

Treatment of chronic hepatitis—Neuralgia of the liver succeeding hepatitis—Connection of hepatic with gastro-intestinal disease—Modes of transmission of disease from the mucous surface of the liver—Phlebitis of the vena porta—Obstruction of this vein—Case of pulmonary, hepatic, and intestinal fistulæ—Hepatic neuralgia. 150

LECTURE XIX.

Gastritis, with delirium tremens—Varieties of intestinal worms—Organisation and origin of—Occurrence in the fœtuses of various animals—Formation—Pathology of perforation of the intestines by—Worms in tumours and abscesses. 160

LECTURE XX.

Symptoms of intestinal worms—Sympathetic irritations of—Affections of the nervous and respiratory symptoms—Various diseases mistaken for worms—Exciting causes of worms—Farinaceous and milk diet—Verminous fever—Treatment of worms—Specific and mechanical purgatives; calomel, turpentine, &c. &c.—Remedies for each species of worms—Preventive measures. 169

LECTURE XXI.

Painters' colic—Effect of metallic poisons on the nervous system—Symptoms of painters' colic—Pathology of neuroses—Action of lead on the system—Abdominal and cerebral symptoms—Species of painters' colic—Dr. Thompson's researches on lead—Effects of in animals—Effects of on the generative system. 177

LECTURE XXII.

Pathology of painters' colic—Researches on the state of the nervous and digestive systems—Treatment—Use of narcotics, purgatives, tobacco, &c. &c.—Treatment of paralysis from lead—Efficacy of strychnine and brucine—Colic from copper—Poisonous effects of mercury—Remarkable case—Affection of the respiratory muscles. 186

LECTURE XXIII.

Diseases of the nervous system—Pathology of, unknown—Molecular change in the nervous centres—Difficulties of distinguishing arachnitis from encephalitis—General and partial cerebritis—Symptomatology of—Diagnosis of—Preservation of intellect in—Production of general symptoms by local lesion. 195

LECTURE XXIV.

Encephalitis, diagnosis of—Preservation of function with organic disease—Vicarious action of parts—Importance of pathology to phrenology—Use of pathology to phrenologists—Arachnitis at the base of the brain—Symptoms of—Influence of age over the intellectual faculties—Opinions of Bouillaud, Serres, and Foville—Influence of the optic thalami and corpus striatum on the motions of the extremities—Diagnosis of disease of the cerebellum—Connection with the generative system—Remarkable cases of. 204

LECTURE XXV.

Symptoms of encephalitis—Conclusions as to contraction and paralysis—Remarkable cases of encephalitis—Abscesses in the brain—Sympathetic affections—Enteritis simulating cerebritis—Prognosis in cerebritis—Remote neuralgia a symptom. 215

LECTURE XXVI.

Encephalitis—Treatment of in the adult—Importance of energetic means—Dangerous effects of opening the temporal artery or jugular vein—Copious blood-letting from the arm—Difficulty of producing syncope—Employment of cold—

Good effects from purgatives—Encephalitis caused by piles—Treatment—Beneficial effects of blisters—Mercury—Dangerous effects of emetics—Dessault's treatment—Use of opium—Violent counter-irritation of coma—Application of boiling water—Treatment of partial encephalitis. 224

LECTURE XXVII.

Analysis of symptoms of cerebritis—Inconstancy of pain—Arachnitis, pain of—Intermittent pain—Headache—Phenomena of the eye—State of the pupils—Various affections of the functions of vision—Researches of Parent and Martinet—Relief by convulsions—Brain considered as a secreting organ—Dangerous effects of opium; delirium—Phenomena of organic life—Vomiting in Hydrocephalus—Sympathies of the digestive and respiratory systems—Treatment of hydrocephalus—Of internal remedies—Cancrum oris, treatment of. 342

LECTURE XXVIII.

Apoplexy—Cerebritis and meningitis—Definition of apoplexy—Simple or nervous apoplexy without disorganisation—Complicated with other diseases—Congestive or serous apoplexy—Dr. Abercrombie's opinions—Apoplexy with extravasation—Sites of extravasation—Absorption of clot—Apoplexy in children. 246

LECTURE XXIX.

Apoplectic effusions—Curative process adopted by nature—Connection of temperaments with disposition to apoplexy—Researches of Rochoux—Periods of life most subject to apoplexy—Principles of diagnosis—Varieties of apoplexy—Connection of symptoms with pathological appearances—Rostan's division of—Different symptoms of—Double effusions—Rupture into ventricles—Hemiplegia—Value of the suddenness of paralysis as a diagnostic examined—Symptoms of apoplectic effusions. 254

LECTURE XXX.

Apoplexy from ramollissement of the brain—Supervention of apoplexy on encephalitis—Inflammation round the clot—Variety of paralysis consequent on apoplexy—Paralysis croissée—Different forms of paralysis—Origin—Phenomena of face and tongue—Paralysis of the tongue—Treatment of apoplexy—Blood-letting—Purgatives—Lotions, beneficial effects of—Emetics, dangerous effects of—Use of revulsives and stimulants—Treatment of paralysis—Efficacy of strichnine—Its modus operandi—Brucine, its proposed employment. 261

LECTURE XXXI.

Local treatment of paralysis—Flesh-brush, shower-bath, &c.—Application of moxa—Cases in which it is useful—Professor M'Namara's plan—Acupuncture with galvanism—Electro-puncturation—Method of applying—Powerful action of a small battery—Mr. Hamilton's observations—Value of galvanism and electricity—Use of, in paralysis of the muscles of the face—Paralysis from disease of the arterial system—Case of, by Dr. Graves—Diagnosis of this affection—Pathology of Pott's gangrene—Dupuytren's mode of treatment. 271

LECTURE XXXII.

Paralysis from arterial disease—Singular cases of, by Rostan—Diagnosis of paralysis from arterial obstruction—Magnetism, use and action of—Effect of magnetism in disease—Result of trials in the Meath Hospital—Paraplegia—Mechanical hyperæmia—Occurrence without disease of the cord or vertebræ—Cases by Mr. Stanley—Effects on urine by division of the spinal cord—Ammoniacal urine—Caries of the vertebræ—Diagnosis of paralysis with disease of the kidney—Prognosis in paraplegia. 279

LECTURE XXXIII.

Sudden paralysis from abscess of the brain—Curious case of paralysis without effusion—Previous symptoms of—Demonstration of the cellular tissue of the brain—Compressibility of the brain—Inaccuracy of the opinions of Drs.

Abercrombie and Clutterbuck—Pathological states—Arachnitis without delirium—Traumatic apoplexy—Case of paralysis of the portio dura—Peculiar appearance of the affected side of the face—Use of the electro-puncturation—Bad effects from—Mechanical support of paralysed parts—Neuroses, active and passive—General pathology of—Principles of diagnosis—Case of neuralgic liver—Neurosis from moral causes. 290

LECTURE XXXIV.

Principles of treatment of neuralgic affections—Connection with organic disease—Neuralgia of the liver—Treatment—Hemicrania—Treatment—Use of iron, quinine, and opium—Endermic method of using opium—Tic douloureux—Opinions of Sir C. Bell—Remarkable case related by—Inflammation of frontal sinuses—Violent symptoms—Mr. Crampton's treatment—Affections of the fifth and seventh nerves in cases of cerebral disease—Neuralgia of the side—Researches of Lombard and Brande on the effect of nitrate of silver—Injury to the skin. 300

LECTURE XXXV.

Scrofula, former opinions on—White and red capillaries—Division of the system into red and white tissues—Vascularity of the white tissues—Dr. Graves's views of the lymphatics—Analogies of lymphatics and veins—Meckel, Cruikshank, and Magendie's opinions on—Relations of the circulating and nervous systems—Vitality of serous membranes—Reproductive power of white parts—White blood and white tissues more prevalent in women than men—White tissues more liable to cancer, &c.—Analogy of the white parts with cold-blooded animals—Increased sensibility of white tissues—True nature of the scrofulous diathesis—Reference to arrest of development—Explanation of its phenomena. 311

LECTURE XXXVI.

Fever—General considerations on—Erroneous modes of investigation—Importance of the labours of French pathologists—Complication of fever with local disease—Primary and secondary fevers—Relation of, to local changes—Tendency to spontaneous termination—Principles of treatment—Errors of Brown and Broussais—Researches of MM. Gaspard and Magendie—Their pathological conclusions—Importance of the knowledge of secondary lesions—Effect in preventing crisis—Treatment—Humoralism and solidism. 323

LECTURE XXXVII.

Intermittent fever—Definition and character of—Phenomena of the paroxysm—Cold stage—Internal congestions—Pathology of—Hot stage—Ague not a simple fever—Affections of various viscera—Theory of Broussais—Effects of bark, quinine, &c.—Modus operandi of. 333

LECTURE XXXVIII.

Intermittent fever—Symptoms—Occasional irregularity of the paroxysms—Convulsive motions of the fœtus in a pregnant woman during ague—Exciting causes of ague—Treatment—Complication with other diseases—Importance of careful investigation—Visceral lesion, how far contra-indicating the use of bark—Bark almost a specific in ague—Large doses of quinine in ague—Rapidity of its operations in some cases—Fowler's solution of arsenic—Prussian blue—Its advantages. 341

LECTURE XXXIX.

Use of quinine—Disease not a simple increase or decrease of vitality—Bark a specific in ague—To be given in the period of apyrexia—Large doses at considerable intervals—Arsenic followed by dyspepsia—Mercury, its effects in ague—Treatment during the paroxysms—Dover's powders, heat, laudanum, carbonate of ammonia—Pressure on large arteries to arrest the cold stage—Used in a case of hydrophobia with temporary relief—Gastric intermittent—Endermic mode of using quinine—Bleeding in the cold stage—Generally with safety and advantage—Supervention of other diseases. 350

LECTURE XL.

Continued fever—Varieties of fever infinite—Typhus fever—Symptoms of typhus—Petechiæ, sign of typhoid character—State of the tongue various—Progress of the disease—Typhus produced by injection of putrid substances into the veins—Hemorrhage from the intestines, &c.—Opinions on fever—Prognosis—Phenomena arising from each system—Jaundice an unfavourable sign. 359

LECTURE XLI.

Nervous symptoms in typhus—Uncertainty of development—Opinions of Dr. Clutterbuck—Unfrequency of lesions of the brain in typhus—Occurrence of all nervous symptoms, independent of any appreciable symptoms of the brain—Nature and treatment of headache in fever—Delirium, researches of Louis on—Its treatment in early and advanced stages of fever—Pathological state of the brain in delirium—Use of wine and opium—Dr. Graves's remarks on—Nature of adynamia—Principles of treatment of the local inflammations in fever—Errors of the school of Broussais on this point—Use of stimulants at certain stages. 367

LECTURE XLII.

Opium in fever—Dr. Latham's opinion on—Symptoms for the exhibition of opium—Affection of the sensorium in fever—Adynamia, consequence of fever—Direct adynamia—Indirect adynamia—Treatment of—Stimulants in fever—Dr. Grant's notions of fever—Symptoms of typhus—Catarrh of fever—Opinions of Andral, Louis, and Laennec—Bronchitis with fever—Increase of râles on decrease of disease—Affection of the gastro-intestinal mucous surface—Symptoms of pneumonia and bronchitis—Pneumonia of fever—Symptomatic affections of the respiratory system in fever—Sympathy between the left lung and stomach—Phthisis, consequent of fever. 377

LECTURE XLIII.

Peripneumonie des agonizans of Laennec—Congestion of the lungs from position—Avenbrugger's opinions on—Precaution of Boyer—Importance of position in typhus—Treatment of catarrh and pneumonia in typhus—Principles of treatment—Management of excessive secretion—Employment of emetics—Use of sulphate of quinine and opium in injections—Typhoid pneumonia—Gastric symptoms in fever—Broussais' physiological theory—Brown's sthenia and asthenia—Remarks of the physiological school—Different treatments of fever. 387

LECTURE XLIV.

Different lesions in typhus fever—Absence of gastro-enteric symptoms—Cases by Bouillaud, Andral, Louis, &c.—Andral's arrangement of fevers—Louis's opinion on the anatomical character of fever—Analogy of typhus with small-pox—Absence of pain in enteritis—Means for diagnosis—Variety of disease in serous tissues from typhus—Treatment of the digestive symptoms of typhus—Hiccup—Tympanitis in fever, treatment of—General treatment of fever—Conclusion. 398

LECTURES

ON THE

THEORY AND PRACTICE OF MEDICINE.

LECTURE I.

GENERAL OBSERVATIONS.

Gentlemen:—You may have often heard that the approaches to science are rugged and uninteresting, and some of you have perhaps experienced the truth of the remark. Hence the custom of delivering an introductory lecture, in order to lay before the young mind, when first entering on each path of knowledge, the objects, the results, the attained good, and the hoped-for glory of the pursuit. These are to be displayed with clearness and with truth, yet it is obvious that much of the effect of such a lecture must depend on the nature of the subject and the judgment of the speaker; and it is well when the exalted nature of the one is attainable by the capabilities of the other. Such a lecture, then, should be an earnest lesson on the objects, the pleasures, and the advantages of that science, of which the course is destined to treat; its history, its true mode of study, its interests, actual state, and future prospects, may all form legitimate subjects, and when thus rightly viewed, an introductory lecture, so far from being a mere ornamental appendage, may become a most important part of the course.

With these views let us approach our subject, the theory and practice of medicine. Let us contemplate that study and that profession, which, venerable by all antiquity, yet in itself is "ever new." Even in its infancy, when the world was in darkness, was medicine a glorious science when compared with its cotemporaries, and its first professors were ennobled and exalted by its influence. As their mantles descended through a long line of illustrious successors, we see medicine progressively expanding, and even when the night of barbarism hung gloomily over the earth, we see its genius triumphing over the surrounding darkness, and shining in the east as a beacon to the shipwrecked mind of man; and I

trust that I shall be able to prove to you, that, in our own time, when the human mind has made such astonishing advances, medicine has kept pace with her sister sciences, and it is a gratifying reflection to think, that, among the most distinguished promoters of the collateral sciences, physicians have ever held a commanding rank, thus proving themselves foremost in knowledge, as they have ever been in philanthropy, in private and public charity, and in all good will to man.

It is scarcely necessary to allude to the title of this course of lectures, further than to remark, that, however different they may be in name, it is yet impossible to draw the line of distinction between the theory and the practice of medicine. If medicine were merely the knowledge of a number of empirical remedies for particular symptoms, given without our enquiring into their mode of action, or any acquaintance with the dependence of one function, or one viscus, on another, of any knowledge, in short, of physiology in the healthy or diseased state, then we might have a practice of medicine independent of what is called its theory. But medicine now holds a higher place, and much of its improvement is traceable to our advances in physiological and pathological science. Thus to treat, or teach, the treatment of a disease, we must know the healthy function of the organ, or organs, the history of development, the influence of other organic systems, the changes produced by disease, and, as far as possible, the action of all external or internal agents on the viscera. But this is the theory of medicine.

For example, let us suppose that we are called either to treat or to teach the treatment of a case of enlarged liver. Let me here remark, that in selecting this case I do not wish you to suppose that I am one of what might be called the hepatic school of medicine, in which the existence of almost every organ, except the liver, seems to be forgotten, and of which the creed seems to be, that there is but one viscus, the liver, one source of disease, biliary derangement, and one cure, mercury; a creed which, though not enforced and defended by the sword, has lost perhaps as much of human life as others whose history is written in letters of blood. But no one can doubt the importance of the organ, and I have taken it to illustrate the connection between the theory and the practice of medicine.

You detect an enlarged liver; you are called to cure the disease:—

1st. You must be aware of the healthy state of the organ, and of its healthy functions, as shown by the volume, sensibility, influence on digestion, and the healthy state of the secretion. You must know all these, as it is by the departure from these conditions that you recognise this disease at all.—*But this is the theory of medicine.*

2dly, You must know the history of its development, because there is a period of life when the natural state of the liver is in a greatly enlarged condition, and this may continue even to adult

life, and produce an enlarged liver, not the result of disease but the arrest of development, and the question will arise as to whether the case before you is an example of this, or of recent and actual disease. The whole treatment turns on this.—*Yet this is the theory of medicine.*

3dly. You must know the influence of other organic systems. An enlarged liver may be produced mechanically by obstructions in the lungs or in the heart; it may be produced from the sympathetic irritation of a duodenitis, or be the result of original disease in its own structure. All these circumstances must be known and taken into account. If it be merely obstruction in the venæ cavæ hepaticæ the ordinary treatment will not answer; if there be duodenitis we must modify our treatment, and so on. We must know these things; we must know how to recognise these diseases before we can prescribe or practise successfully. All this is that part of the theory of medicine called pathology, or the physiology of the diseased body.

4thly. You must know the effects of disease on the liver itself. Some of these are removable by art, others are totally incurable. You must know these in order to determine on the probability of their existence.

5thly and lastly. You must know the influence of remedial agents on the liver and the adjacent organs. You must be familiar with the effects of stimulation of the mucous surfaces of the stomach and duodenum. Then, indeed, and not till then, will you be qualified to treat the case with judgment and success. The same remarks, I need scarcely add, will be found applicable to the diseases of each viscus in the body.

The objects of medicine, gentlemen, are twofold; first, to cure disease, no matter where seated or how produced; and secondly, to relieve bodily suffering in cases where a cure is impossible. Its great end is to prolong life, and to diminish the bodily evils which result from the infirmities of our nature and other circumstances. Some of you may ask, where then is the distinction between medicine and surgery? In truth, there is no distinction in reality, and there should be none in theory. The human constitution is one;—there is no division of it into a medical and surgical domain; the same laws and the same principles of treatment apply to the cure of a fractured bone and the cicatrisation of an internal ulcer. Unlike the corporations of medicine and surgery, the supposed purely medical and purely surgical parts of the body live in excellent harmony. Here, then, there is no division, no jealousy, no separation of interests.

I am by no means prepared to deny that advantages may arise from a practitioner devoting himself to this or that branch of his profession; but, *if he seeks for eminence*, he will first educate himself *generally*. Let him attain extended views of pathological medicine; let him make himself master of the actual state of the science, and then he will find that there is not a single fact or law with which he has become acquainted that will not have its bear-

ing on his particular pursuit. It is in the education of medical men that the ruinous effects of the division of the professions of medicine and surgery are most perceived: and I feel convinced that, of the two, *the surgical student is the greater sufferer*, because his views of pathology are injured. All the great laws in pathology are drawn from the consideration of visceral disease; yet the attention of the surgical student is diverted from this, and directed to what, I will say, can never elevate him in the ranks of science. He is taught anatomy, and what is called surgical disease, but he is kept ignorant, by this wretched system, of the great part of his profession, until he comes to practise, when, if he has a mind fitted for observation, he will find, that for one dislocation there will be hundreds of visceral diseases; and he will discover what was concealed from him during his pupilage, *that many, many more die of what are called medical than surgical diseases*. During the late war, more men in the British navy died of fever than of all other causes—including the sword. But, I rejoice to say, in Dublin the exclusive system of education is fast wearing away, and one of the many excellences of our national school of medicine is the instruction in general pathology. There are few schools of medicine where now a more enlarged and liberal spirit of education exists.

In the study of your profession, gentlemen, let me warn you not to allow yourselves to be misled by the idea that surgery and medicine are different in their nature. The mere surgeon, or the mere physician only knows half of his profession. Reckless of human life, he may practise the healing art as a trade, but he never can know it as a science. But, as there are infinitely more cases of what are termed medical than surgical disease, it is plain, that the surgeon, ignorant of medicine, will far exceed the physician ignorant of surgery, in the extent of his malpractice. I have long observed the ruinous system which has been pursued by teachers, as connected with this subject. The pupil was taught to consider, that if he was a skilful anatomist, if he understood the routine surgery of an hospital, and had carefully studied certain works on surgery, and some obsolete books of pathology, he was thereby prepared, in the language of the schools, to go forth to teach and practise the art and mystery of medicine in general. Now, all this was wrong. You may be profound anatomists and be bad surgeons, and worse physicians; you may have by heart the writings of Pott and Dessault, of Hunter and Thomson, and be totally incapable of treating a simple or complicated fever, or a case of visceral disease. But it is not necessary to say more. Society demands that the old system of a division in education should be abolished; and, ere long, I even trust to see a fusion of the profession, when much of the present evils must cease, when medical men shall have a common centre, from which they will receive a common impulse; when their efforts shall be solely directed to the increase of medical science, and the political and moral exaltation of their profession; and last, yet not least, when the ingenuous pupil shall not be led

astray; when he shall not be told by one teacher to despise this, and by another to neglect that part of his profession; but, having the *whole* of the noble science of medicine thrown open to him, his mind, unwarped by prejudice, unfettered by fear, shall be permitted to take that right view of his pursuit, that alone can lead him, and assuredly will lead him, to the honours and success which truth bestows on all its votaries.

I have said, that the exclusive system of education had singularly diminished in Dublin. Indeed, our national school has earned great reputation for general pathology; and, from a long and cordial intercourse with the class of Dublin, I will affirm, that there are few places where we can see such zeal, talent, and thirst for knowledge among the students. As an Irishman, addressing my own countrymen, let me congratulate you on the fame the Dublin School of Medicine and Surgery has now acquired, and is every day acquiring; and when the strength of Irish talent, aided by the proper working of our unrivaled institutions, is brought into play, may we not anticipate a still more glorious result? This reflection has often cheered me, that within the last few years there has been a greater stimulus infused into the science and literature of this country. Amid the ungenial influences of political excitement, and the animosities of party, how gladly should we contemplate the advance of what will prove an honour to our national character, and an advantage to mankind. It is like the growth of the coral into rocks and fertile islands, though surrounded by the strife and waste of waters. Our scientific societies have multiplied; our periodical literature, the want of which furnished so fruitful a theme for cavil, has been extended so as to afford a wholesome and vigorous supply in the varied departments of literature and science; and our monthly and quarterly publications are taking their proper place among the ranks of British journals. When we turn to works of a more permanent kind, we also see cause for satisfaction. Many most important works in anatomy, surgical pathology, physiological medicine, and midwifery, have lately issued from the Irish press; and the Irish contributions to the Cyclopædia of Practical Medicine are allowed on all hands to give to that work no mean portion of its value.

There are few more wholesome exercises for the mind, few so necessary and so useful as the comparison of the actual state of any science with its advance and character at a former period; and it is in this, most chiefly, that the value of what is called the history of medicine consists. We study it then, not as a matter of antiquarian research, of learned curiosity, but as the picture of the human mind, now on the right path, now misled by error, yet still struggling onward; as the record of a dear-bought experience, and a beacon to warn us of the rocks and shoals that beset its future progress unto truth. To analyse the actual state of medical science, to show you all that has been done within a little time, to display all old pretensions to the character of a true and thrice noble science, would far exhaust my capabilities and your patience.

Let it suffice to contemplate the improvement considered generally, and the means by which that improvement has been attained.

It is an error too generally received, that medicine owes all its advances to the researches of modern times. Far be it from me to undervalue these, but I believe that the opinion I have alluded to is wrong, and is perhaps kept alive by our own vanity; for by a specious deception we often take to ourselves the honours and distinctions of the time we live in. The truth is, that medicine, like many other of the sister sciences, has been long steadily advancing, and the flippant every day remarks that the *inductive system* (that is, the observation of facts and the embodying of those conclusions that legitimately flow from them) has been only introduced into medicine in our time; and that our predecessors in medicine put theory first and fact second in their medical philosophy, are "*as false as dicers' oaths.*" Have the authors and teachers who are so fond of decrying the medicine of a former day, at a time when they are (perhaps innocently) making use of its facts and observations—have they read the writings of the father of medicine? Have they studied that "*aureum opus,*" so well called from its lustre, its purity, and its surpassing value? Was Avicenna a mere theorist? Did Morgagni observe no facts, nor truly record them, even at the expense of his medical reputation? Is there no induction in Baglivi? Was Haller unacquainted with the method of experiment and induction? Or is the discoverer of the circulation of the blood, the good, the great, the injured, but the immortal Harvey, forgotten? Where do they place Boerhaave? and shall the name of Sydenham go down with his ashes to oblivion?

The true state of the case is, that medicine, in its present advanced state, only represents the improvement in other branches of human knowledge, all of which are so intimately linked together, that, although their extremes be far removed, there is a point where all are reciprocally cause and effect; so that if we take any one of them, it is easy to show its intimate bearings with, and importance to, all the rest. We have been long advancing in medicine; and though I admit most fully the vast strides which have been made, still I must here declare my firm conviction, that the study of the older authors is too much neglected, and that in them you will find a treasury of knowledge, much of which you may think to be the production of modern times.

If the writings of the ancient authors only contained a small portion of the information with which they abound, it would be a sufficient stimulus to their study; to reflect that it is in them, in the medical writings of the ancients, that the germs of the inductive philosophy are first to be found. It is, then, in the old regions of medicine that we find the fountains of that mighty river, which, for two thousand years, has fertilised the earth, and made man its lord. Had the progress of man not been retarded by the ignorance which is the child and servant of barbaric despotism, an earlier Newton might have enlightened the earth, an earlier Laplace have measured the heavens, or a Cuvier declared the glories of a past and present

creation. The mind of man would have burst its chains, and ages ago have formed that holy alliance with knowledge and her first-born, liberty, which now is its safeguard and its glory. I repeat it, in the writings of Hippocrates you will find the principles of the inductive philosophy. A physician showed Bacon the road to immortality.

We find that there is in the mind of man a tendency to reverse the true mode of reasoning, and to seek for a principle before it has observed facts, and this was the cause of the retardation of medicine, as well as of all other sciences. Hence the various schools, from Pythagoras to Cullen or Brown, in our day. But a slow, though sure, revolution was long going forward; and I believe that Cullen and Brown were even *behind* the actual state of medicine in their time. Physicians turned disgusted from the war of words and doubt, to seek in tangible objects the certainty which these only can produce; in a word, they began to follow the Baconian system more generally. They reverted to the instructions of Hippocrates, and from that period our modern improvement may date. They turned their attention to the examination of those changes which disease produces on the human body, and connected these with the symptoms observed during life. And what has been the result of this?

1st. The accumulation of an enormous number of facts, relative to the changes of organs produced by disease.

2d. The connection of a vast number of these changes with particular symptoms, and hence the advance in diagnosis.

3d. The establishment of the true value of symptomatology, and the verification of that all-important fact, that opposite states and organs may produce similar symptoms.

4th. The knowledge of the vast class of latent diseases; in other words, diseases which exist without influencing the phenomena of animal life, or, in some cases, the phenomena of both animal and organic life. Diseases, either without symptoms at all, or only with such as previously were not supposed capable of leading to their detection. You know that the phenomena of life are divided into two classes, viz. those of organic or vegetable life, such as *nutrition, circulation, absorption, respiration, secretion*. While those of animal life, or the life of relation (*so called from its being the source of our connection with surrounding bodies*), are the senses, the phenomena of mind, and muscular motion. The one life seems more under the influence of the ganglionic, and the other under that of the cerebro-spinal system of nerves.

As some of the junior part of the class may not have accurate ideas as to the meaning of symptoms, I may state that disease is recognised by signs and symptoms.

By signs, we mean those mechanical alterations, produced by disease, in the conditions of parts, which are recognisable to the external senses of *touch, sight, and hearing*; *changes in appearance, volume, shape, resistance, peculiarities of feel*, and the production of *sounds*. We may make a diagnosis by signs alone.

Take, for example, a case of tympanitis. The abdomen is prominent, enlarged, circular, elastic, and sounding like a drum when struck. Thus we learn that the belly is distended by air.

Now, *symptoms* are totally different; they consist in certain changes produced in *functions*; and these functional changes are to be considered in a threefold manner:—

1st. Changes in the functions of the part itself.

2d. Changes in the phenomena of organic life.

3d. Changes in the phenomena of animal life.

Let us take, for example, a case of inflammation of the stomach. We have, first, changes in its own functions—morbid sensibility, vomiting, thirst, anorexia. In the next place, we have changes in the functions of organic life—fever, from the action on the circulating system; hurried respiration, and cough, and hiccup, from the action on the respiratory system; jaundice, from its action on the biliary system; suppression of the secretion of the skin, kidneys, &c. All these, you observe, are lesions of the functions of organic life.

But we may have other symptoms; prostration, headache, delirium, convulsions; these are lesions of the life of relation, or animal life.

Now, in many cases, we have to combine these sources of knowledge to form a correct diagnosis. Take, for example, a case of hepatitis.

The patient has had pains in the hepatic region, fever, jaundice, hurried breathing, tenderness. After some time he has a tumour; the side dilated; the hypochondrium dull on percussion. Well, the signs point out an enlargement of the liver; the symptoms, that the cause of that enlargement was an acute hepatitis.

In general, we may state, that signs only declare the actually existing mechanical condition, while symptoms, either present or past, point out the cause of the change, whatever it may be. Both must be studied together; but you will learn more from symptoms without signs, than from signs without symptoms. But to return to the results of the improved method of investigation.

Great light was thrown on *fever* in general; and it is, I believe, quite true, that all the advances which we have made in the knowledge of fever, are due to the prosecution of pathological anatomy. Almost all of what we may call our general knowledge of fever, is due to Hippocrates; but anatomy has revealed its effects, its complications; and the all-important fact, that the cause of its fatality is often local inflammation. This knowledge, however, is not so new as is taught by some modern systematists. Galen (*De Affect. Intern. c. xli.*) taught, that in continual fevers bleeding and cold drinks were the powerful remedies. Sydenham declares that the ignorance of the inflammations in malignant fevers, has been more fatal to the human race than the invention of gunpowder. Baglivi, that malignant fevers often depend on a visceral inflammation, and Van Swieten knew the frequency of intestinal ulcerations in typhus.

Among the direct results of pathological anatomy, it is shown that *disease is seldom confined to one organ, or even one system*, and thus it has utterly shaken the nosological system of Cullen and his predecessors, which, you know, consisted in classifying disease by symptoms, which were supposed to point out a certain and single disease. For example, the nosologists class *phthisis* as an affection of the lung; but pathological anatomy has shown, that in many cases it is the result of a disease invading many organs and systems, and that the pulmonary disease is but a link in the chain of morbid actions. Pathological anatomy, also, has demonstrated the inflammatory nature of a vast number of diseases, and has thus given us a key to treatment, to prevention, and to palliation, when the disease is incurable.

The last grand result of pathological anatomy is the discovery that a vast number of affections, supposed to be merely lesions of function, are more or less connected also with alteration of structure. Thus many of the dyspepsias of the nosologists are proved to be examples of gastritis, or of other organic diseases; cases of asthma turn out to be chronic inflammation with emphysema; the palpitations may depend on organic disease which has sprung from a carditis, and so on. I need not now dilate on the vast importance of such facts to practical medicine.

But let us now come to an all-important enquiry. Is pathological anatomy to be considered as the basis of medicine? or is it, even when combined with clinical observation, the foundation of all medical knowledge? This enquiry, you will at once perceive, involves the question as to whether Hippocrates and his followers have done any thing for the science, or whether medicine is wholly new, an infant, and consequently a weak and imperfect science. Are we to despise the works of the ancients, to be ignorant of them, and to allow medicine to be in its infancy? In fact, if we review the history of medicine from the Hippocratic era to the absurdities of Hahnemann, we find that there have been two orders of men, one constituting what we may term the school founders, who made a theory, and sought to square facts to meet that theory; these have only brought disgrace on medicine. The other class consists of the Hippocratic observers; that is, of men who sought for facts, who collected and pondered on these facts, in other words, who were Baconian philosophers. It is the labour of these that has really advanced medicine. Asclepiades, who lived in the first century of the Christian era, declared that the medicine of Hippocrates was a *cold meditation* of death. The celebrated Thessalus, who lived under Nero, in writing to the emperor, makes use of the following words:—

“I have founded a new sect, which is the only true one. I have been forced to this, because none of the physicians who have preceded me have discovered *any thing useful*, either for the preservation of health, or for the cure of diseases, and because Hippocrates himself has put forward many dangerous maxims.”

And what was this new doctrine? That nature in each case

pointed out to the patient what was most fit for him, and that hence he should be diligently supplied with every thing that he fancied.

We have next Paracelsus. He commenced his course of lectures at Basle, in the year 1526, by publicly burning the writings of Galen and Avicenna, and assured his auditors that a single hair of his head contained more knowledge than Hippocrates and his successors. He taught the cabalistic medicine, the intimate connection between the planets and the viscera; he was a vitalist, but embodied his vitalism under the shape of a demon, who resided within the system, and which he called Archæus. Diagnosis was to repose on the examinations of the stars, and not on symptoms. He invented the doctrine of tartar, which is the cause of all diseases, of accumulation, obstruction, and concretion; "and I call it tartar," says he, "because it contains the oil, the spirit, and the salt, which burn the patient as hell does."

Hahnemann, the founder of the homœopathic doctrine, may be quoted next as an example of these school founders; and he, like his predecessors, expresses himself with all that arrogance, which ignorance, when it pretends to learning, invariably assumes. Speaking of the Hippocratic medicine, he says—

"Since this art only consists in a gross imitation of a dangerous and insufficient process, it must be admitted that the true medicine was not discovered until by me. It is the infallible oracle of the art of curing; it is the sole mode of really curing disease, because it reposes on an eternal and infallible law of nature."

And what is this mode and doctrine? We have it in four propositions, and it is hard to say which of them is most revolting to common sense. We are told that it is absurd to seek for the cause of symptoms in order to remove them; that we must cure diseases by the exhibition of substances which would otherwise produce them; that the dose is to be inconceivably small; and that there are three original diseases from which spring all the maladies which afflict mankind—syphilis, sycosis, and the itch. These are the fruitful causes of all diseases, epidemic, sporadic, idiopathic, and symptomatic. Like his predecessor in quackery and deceit, he, too, has in his syphilis, sycosis, and itch, the oil, the spirit, and the salt, which burn the patient as hell does. Like Paracelsus, too, he maintains the curability of diseases, and is a disciple of the animal magnetism.

Let us next see how Broussais announced his doctrine to an admiring world.

"After so many vacillations in its march, medicine at length follows the only path which can conduct it to truth—the observation of the relations of man, with external modifications and the relations of the organs of man, one to the other." This is the physiological method, because it cannot be followed without studying life.

I am more anxious to draw your attention to this doctrine, as Broussais may be considered as the source of the anatomical

school, which, of late, was so completely the fashion—if I may use such a term; and it is a striking instance of the danger that attends the idea of our having made a discovery, to see a man like Broussais, than whom few have really added so much to medicine, falling into the same fault of arrogance and contempt towards his predecessors.

At this moment, the medical world, particularly on the continent, are divided into two great sects. One may be called that of the *pathologico-anatomists*, the other the *Hippocratists*. The first declares that diseases are *primitively local* in all cases; that the symptoms—say in a case of fever—are only the *results of a sympathetic irritation from some local disease*, which is to be attacked *with vigour*; that pathological anatomy is to be the foundation of all practice; that there is nothing approaching to a *specific in medicine*; and that *nature makes little or no attempt to cure*. Their favourite maxim is that saying of Bichat's—“*What is observation, if we are ignorant of the seat of disease?*”

This is the sentiment of an anatomist, but not of a physician; and we must regret that it once escaped the author of the “*Researches on Life and Death*,” a book of such interest and such beauty, as to captivate even the non-medical reader, and make the very name of Bichat be hallowed in our memory. Many are the diseases of which we know not the seat; yet in which observation—Hippocratic observation—is of the greatest utility.

We know not the seat of fever, let the followers of Broussais say what they may to the contrary; yet is observation of symptoms of no avail in fever? Are the effects of contagion, the history and nature of epidemics, the termination by crisis, the results of treatment, of symptoms as connected with prognosis—is the observation of these useless or unnecessary? Sydenham knew not the seat of variola; yet he declared the true principles of its treatment. There are very many diseases on which pathological anatomy throws but a negative light—if I may use such a term—particularly affections of the fluids, and the neuroses.

So much for the doctrine of the anatomical school. I beg of you not to misunderstand me as undervaluing pathological anatomy; I only wish to show you its true value. I believe there could hardly be adduced a single fact in pathological anatomy that has not its distinct bearing on practical medicine. And it is true that the diseases whose treatment is best understood are those whose pathological nature are best known. Even in fever, the actual nature of which has not been revealed, great advantage has been derived from anatomical researches; for all the advance in our knowledge of this Protean disease consists in ascertaining the number, nature, and seat, of the local inflammations which accompany or rise in the course, and complicate the disease.

Let us, lastly, revert to the opinion of the Hippocratists. They admit that *vast advantage* has arisen from pathological anatomy; but they see that its light is limited within certain bounds. They

believe that great advantage is to be derived from the careful study of symptoms, even in cases whose pathological nature is not revealed by the knife. They believe that there are many diseases whose local origin cannot be demonstrated; for instance, *fever*. They deny that pathological anatomy is always to be our guide; but admit a rational empiricism, and the use of remedies which may be called specifics; and, lastly, they hold that nature, in many cases, makes an attempt to cure; and that the physician, in the words of Hippocrates, is to be *the minister and interpreter of nature*, rather than her master.

Let us, then, combine the precepts of the founder of medicine with the lights of modern science. Let us take *observation*, and that observation rendered fruitful by study, for our guide; and let the observation equally embrace the phenomena of the living as well as the dead. Let us be Hippocratists in the dissecting room as well as at the bed-side. By comparing the practice of these two schools, we get more accurate ideas as to their doctrine. The anatomists, holding that all diseases are local, direct their whole attention to the discovery of the lesion, and its connection with symptoms. This, with their doctrine that almost all diseases are inflammatory, leads them to a strict general and local antiphlogistic treatment. Fever is to them symptomatic, and the supposed source is to be vigorously attacked in the commencement. *Diathesis, the nature of the epidemic, and the powers of nature* to effect a cure, are comparatively neglected. They inhibit purgatives for fear of increasing the local inflammation, and lose many patients for want of a timely support of the powers of life.

They deny specificism in disease as well as in medicine, and are sorely puzzled to explain the extraordinary powers of bark, and mercury, and sulphur, and iodine. They despise the experience of the past.

The true Hippocratist, on the other hand, believing that we have not yet arrived at the knowledge of the local origin of all diseases, and particularly fevers, grounds his practice accordingly. He draws his experience from the recorded knowledge of the past, and his own unbiased observation. When he recognises a local inflammation, he meets it with judgment, taking into account the habit, diathesis, epidemic, constitution, and tendency to crisis. He trusts much to nature, and watches her operations, particularly in fever. He is not afraid of moderate evacuations; the phantom of a local inflammation does not always haunt him; and even where he recognises its existence, that does not prevent him from using a stimulating and supporting treatment, if the general state of the patient requires it. He treats particular diseases by particular remedies, the utility of which has been proved by experience—such as syphilis, scrofula, intermittent fever, and so on. He uses the expectant medicine, which is not inactive treatment, but founded on the observations of the powers of nature—“*Natura morborum medicatrix*,” but he never loses the opportunity of

doing good, when such presents itself, remembering the first aphorism of his great master :—

“*Occasio præceps.*”

I have great hopes for medicine, for I see men's minds turning to the true path ; and I trust that all whom I now address will deem themselves as labourers in the great work. Think what a noble science you profess ! the only one relating to earth-born things, which, while it ennobles the mind of man, yet softens and expands his heart ; whose source is all science, whose end is good to man. Above all things, follow truth ; nature can never deceive—see that you be her faithful interpreter. The great evil is, that there has as yet been adopted no means by which the experience of the past can be brought fully to bear on the actual teaching and practice of medicine. Too often has the physician to create his own instruments. But when all the scattered facts of medicine are collected, whether they be the observations on the living or the dead body, as old as history, or as young as to-day ; when these votive tablets are hung up in the temple of truth, and their facts verified, compared, and classified, then, and not till then, will you see medicine in all her glory :

LECTURE II.

General remarks on local diseases—Fixed rules for the guidance of students—Great importance of diagnosis—Existence of pure fever rare—Doctrine of the Humoralists and of the Brownists—Pathology of the digestive system.

I commence the course by entering at once on the subject of particular diseases. I am aware that the common practice is to occupy the early part of a course on the theory and practice of medicine with preliminary discussions on general pathological subjects. To this I have strong objections. Every man who assumes to himself the office of teacher, no matter what the fact may be, should presume that his auditors are ignorant of the subject he is about to teach ; if he does not he must be unjust to his class. Some of the class must be ignorant of the information he wishes to convey, and he should take it for granted that all are so. To commence with the consideration of general disease would argue that the whole class was acquainted with the subject in all its bearings, and capable of understanding its principles without any previous illustration. I think this is beginning at the wrong end. My plan is first to teach the facts, and then the general principles and conclusions to which these facts lead. It is of the deepest importance in the study of medicine to be able to form a collection of laws or fixed principles. In your professional career, nothing will give you so much satisfaction as having in

your minds a number of established facts and fixed rules to bear on every case which comes under your cognisance. We commonly hear of the uncertainty of medicine and the instability of its practice, it is said to have as many phases as the moon, and as many changes as the tide; but, after all, I think this expression is more general among those who know little than among those who know much. Those who have successfully laboured in treasuring up a store of deep and extensive knowledge are firmly convinced, that, though some cases are involved in doubt and obscurity, the general certainty of medicine is at present increased far beyond what it was in former times. No man, except one in full and extensive practice, earned by industry and capacity, can be aware of the vast improvements of modern practical medicine, and of the number of lives which are saved by the judicious treatment which the rapidly progressive improvement of medical science has introduced. Medicine is much more certain now than it was in past times. There are two reasons for this, one of which is, that at the present period diagnosis, the guide and master-key to sound treatment, is more certain. Here, gentlemen, is a great source of certainty in the practice of medicine. You will find, in the course of a few years, that the old saying of "doctors differ," will become less frequently applicable, because, as the education and acquirements of medical men become more extended, diagnosis will be reduced to fixed rules, and difference of opinion will be very seldom observed. A vast number of local diseases, formerly wrapt in obscurity, are now detected with the most unerring certainty, and this certainty of diagnosis must bear on fixed principles of treatment and similarity of practice. Another vast source of increased certainty is the fact, now extensively established, that the element of a great number of diseases is the same. This is an important law, because the deduction from it is, that the principles of treatment are the same in these cases. The principles of treatment in a case of hydrocephalus and in a case of vomiting from gastritis may be, and often are, completely identical, because, in many cases, both are reducible to a common action. In the one case we have to deal with inflammatory action in the stomach, in the other we have to treat an inflammation of the membranes of the brain. The principle in both cases is to deplete the suffering organ and to diminish or remove every thing that keeps up irritation. Pathological anatomy, too, has effected a vast deal for medicine by the improvements in diagnosis which it has introduced, and by reducing to one class a vast number of affections formerly supposed to be unanalogous and distinct.

Before I commence entering on the consideration of the pathology and treatment of diseases of the digestive system, it is necessary that I should mention another peculiarity of the mode of teaching the theory and practice of medicine adopted in this school. The ordinary way of lecturing medicine in the schools is this: the teacher begins by going over, at great length, the whole subject of fevers, and then proceeds to the consideration of the signs, symp-

toms, and treatment of local diseases. We reverse this mode here; we begin by teaching the pathology and treatment of local diseases, or affections of particular organs, and having studied these with care and attention, we then proceed to the consideration of fevers. In point of fact, we are thoroughly impressed with the truth of this splendid conclusion in medicine, that local diseases may be considered, as it were, the alphabet of fevers, and that to have a distinct and accurate conception of the whole subject of fever, it is essentially necessary that we should be acquainted with all kinds of local disease. To commence with a class which the teacher presumes, or should presume, to be ignorant of the phenomena of local diseases, unacquainted with the rules on which their diagnosis depends, and unacquainted with the principles which should regulate their treatment—to begin with such a class by entering at once on the subject of fever, would, in my opinion, be extremely wrong. You will read in books and hear teachers speak of bilious fevers, of nervous fevers, of catarrhal fevers, of gastric fevers, and of simple fevers. These expressions are founded on the fact of the complication or noncomplication of fever with local disease in various parts of the system. If simple fever was the rule, and its complication the exception, then, indeed, there would be some reason for pursuing the ordinary track of medical instruction, and we might commence by teaching the subject of fever, independent of local inflammation. But the truth is, that fever, in the *simple form, is the exception, and its complication the rule*, and that to have a correct idea of fever, in the general acceptance of the term, we must previously possess an intimate knowledge of the affections of particular organs. The progress of medicine has established, by the most unquestionable evidence, that simple fever is a matter of extremely rare occurrence, so rare, in fact, that you might pass through the practice of a fever hospital for years without meeting with a single case which you could say was, through its whole course, a case of pure, essential fever. Sooner or later its character is changed, and the complication with visceral disease comes on; you may take this with you as a well-proved fact. You will have, at some period, a complication with local disease in the head, or local disease in the chest, or in the belly, or in the circulating system, or perhaps all the great viscera in the body will be simultaneously affected. My experience on this point, after having attended the fever wards of the Meath Hospital many years, is this, that among all the cases which were admitted under such circumstances, there were very few indeed in which I could not say that the patient had something more than fever. Many were admitted who presented no indication of disease in the head, chest, or digestive tube; all that could be said of them, at the period of their admission, was, that they had fever; but my experience of them is, that, in a vast majority, there was, during their progress, unequivocal evidence of the supervention of visceral disease. I do not go as far as the disciples of Broussais have gone, nor do I mean to say that all fevers are

symptomatic ; all I assert is that, at some period, most fevers are complicated with local disease. I admit that there is a vast number of symptomatic fevers, but I believe there are two which are essentially simple, typhus and intermittent. The progress of medicine has shown that these may exist in the simple form, and that their complications may be secondary ; this I believe to be the fact, but the almost invariable liability to complication is a point of the highest importance. We scarcely ever see typhus accompanied by symptoms of local disease ; and, with respect to intermittent, in ninety-nine cases out of a hundred, visceral disease of the head, or chest, or belly, may, and will, supervene.

Another great fact bearing on this subject, and which pathological anatomy has established beyond the possibility of a doubt, is, that in the great majority of cases having a fatal termination, death is caused by disease of some particular organ or organs. The old notion of the cause of death was, that the patient died of debility or exhaustion. In cholera, in tetanus, in hydrophobia, we cannot, to be sure, demonstrate any appreciable lesion of structure, and we may say, if we like, that the patient died of debility ; but this does not hold good in cases of fever, for on dissection you will generally find disease sufficient to account for death, even though there had been no fever at all. From these circumstances it follows that in the management of fevers, the attention of the physician must be directed to the local affections, or, at all events, that to understand fever well and treat it successfully, he must be acquainted with the nature and treatment of every form of visceral disease. It will be sufficient for me to call your attention to this fact, *that there is not a single acute local disease which may not occur during the progress of a fever.* This is a broad and general proposition. If you look to the nervous system you will find, in patients who have died of fever, traces of lesion in almost every part of it, inflammation or congestion in the cerebrum, in the cerebellum, and in the spinal cord. If you go to the respiratory system, you will see all kinds of shades, and varieties of inflammatory action, thickening and ulceration of the bronchial membrane, hepatisation, congestion, and destruction of the parenchymatous tissue, effusions of lymph, serum, or pus, into the pleural cavities. As you proceed in your examination you will discover new lesions ; you may see the whole lung filled with lately formed tubercular matter, you will meet with the destructive ravages of phthisis. You will find the pulmonary tissue converted into a dark and fetid mass by gangrene. You may see carditis, hypertrophy, inflammation of the external or internal coverings of the heart, inflammation of the lining membrane of the arteries, phlebitis, (a common occurrence in typhus fever,) and passing on to the lymphatic system, you will often find evident traces of inflammation in its glands and vessels, an occurrence which I shall be able to demonstrate to you when treating on the subject of gastric fever. If we go to the digestive system we find that disease has here taken a wider range ; congestions and ulcerations of the stomach and intestines,

morbid states of the liver, congestion and inflammation of the spleen or kidneys, evidence the fatal extent of local inflammation. I think I might safely challenge any one to point out any one single organ which may not become diseased during the progress of a typhus fever. I do not wish you to suppose that typhus is a symptomatic affection. I think we may define it, in general terms, as a diseased state of the whole system, in which various local diseases arise, modify the character of the original complaint, give it an additional intensity, and are generally the cause of death. Go round the wards of an hospital during the prevalence of an epidemic fever, examine every patient in succession, and bring this principle to the test. You will see one labouring under the morbid excitement of high delirium; his face injected, his eyes sparkling, his carotids throbbing with intensity. Come next day, and you will find him in a state of profound coma, perfectly insensible to every thing around him:—two or three days afterwards he is dead. You follow his body to the dissecting room, and open his brain; unequivocal marks of excessive congestion, inflammation of the substance of the brain, or of its membranes, sufficiently indicate the cause of the fatal termination. Here is a case of inflammation of the brain; you find another with cold skin, his face of a dirty hue, faintly tinged with red, his breathing quick and hurried, and the spitting-vessel by his bedside filled with adhesive mucus tinged with blood; you percuss his chest and find dulness over the whole surface of one lung; you apply the stethoscope and discover intense bronchitis, hepatisation, or suppurative pneumonia. Farther on you see another in a state of deep prostration, with a sunken countenance, constant hiccup, and low delirium. Take down his bed-clothes, and you find his belly swelled, tympanitic, and tender on pressure; then his tongue, lips, and gums, are parched and encrusted with dusky sordes; his thirst is insatiable; he vomits, and has an emaciating diarrhoea. After death you find traces of an extensive and fatal gastro-enteritis; in others you will find exemplified the very climax of inflammation, and all the three great cavities are simultaneously affected.

But these, you will say, are cases in which the complications are evident, and where an ordinary knowledge of the phenomena of local disease will be quite a sufficient guide. Well, here is another case. You will meet with instances of fever without any apparent local symptoms, where the patient lies in what you would consider a quiet state, and free from danger: nothing seems to be the matter with him, except that he is very weak; he perhaps does not sleep at night, and his tongue is a little foul; he complains, in fact, of nothing but weakness and some thirst, and you think his fever is going on very well. Some morning or other, on coming to the hospital, you are astonished to see the change which has been wrought in him since the day before; his countenance is altered, his pulse can hardly be felt, and life is fast ebbing away. You ask the nurse about him, and she tells you that, during the night he suddenly complained of violent pain in his belly. On

examining him, you find distinct evidence of intense peritonitis, and, after death, dissection reveals the existence of a perforating ulcer of the intestines, of which there was apparently no sign during life, except fever and the unexpected occurrence of peritonitis. The frequency of the complication of local disease with fever, its insidious latency, and the fact, that death, in the majority of fever cases, is caused by visceral inflammations, all clearly point out the necessity of being intimately acquainted with every modification of local disease before you proceed to the study of fevers.

I commence with the digestive system. I am anxious to do this for several reasons, but for none more than this—that to the improvements made in the pathology of the digestive system we owe much of the rapid advancement of modern practical medicine. Before our time the pathology of the digestive system was very little known, and if not quite a *terra incognita* in medicine, there existed respecting it a great deal of misconception. The schools were deeply tinctured with the doctrines of the Humoralists and the Brownists; and this had the effect of giving rise to irrational theories and false notions of the true state of the system in disease. The humoral pathologists, who sought for disease in an alteration of the fluids alone, neglected the study of visceral lesions; and when they turned their attention to the digestive system, they only considered it, its secretions, and not its actual condition or the state of its sympathies. The liver, with them, was an organ of the highest importance, and the secretion of bile claimed a vast share of their attention. To it they gave a paramount influence, and to an alteration in its quantity and quality they attributed most of the changes which occur, not only in the digestive tube, but also in the whole system; and hence the great object of their practice was to attempt to restore its healthy condition, convinced that if this were once accomplished every thing would go on favourably. From this, too, arose the purgative plan of treatment in various forms of intestinal disease, a plan too often rashly pursued, even where there was unequivocal proof of inflammation in the digestive tube. Their sole purpose was to evacuate sordes, to produce a flow of healthy bile, and to eliminate depraved secretions; and they did this without possessing any knowledge of local inflammation, or of the effects of disease of the digestive system on other organs. The followers of Brown, on the other hand, only admitted disease of the digestive system in a state of intense, manifest violence, as, for instance, ileus or violent enteritis; but, in the great majority of cases, they did not recognise intestinal inflammations, because their prominent symptom was prostration, or, to use their own terms, an asthenic condition of the whole system. They saw nothing but prostration; they prescribed for nothing but debility; they gave wine instead of iced water; ordered bark instead of local depletion. They exasperated the disease by stimulants; and then, thinking they had not gone far enough, they heightened the stimulant and doubled the debility.

Another cause of the low state of pathology in former times was

the general neglect of dissection. The fact is, that in fever there were no post mortem examinations made, until very lately. Morgagni, who did so much for pathological anatomy on almost every other subject, did little for fever, because he was afraid to dissect the bodies of persons who had died of a contagious disease. This was the idea which prevailed among the older pathologists; and hence this source of knowledge was avoided, and for many successive centuries the state of the viscera in fever was a matter of speculation, doubt, and uncertainty. Even at the present day it is only done by the ardent pathologist, who cares not about filth and stench, and who had rather encounter the miasm of contagion than remain in the mists of error. Nothing is more common, I regret to say, even at the present time, than this:—A person says he has dissected cases of fever, and when asked whether he had examined the intestinal canal, he says that the intestines appeared healthy, but he did not make any particular inspection of them; he only opened the belly, and, finding no trace of inflammation in the peritoneum, he went no farther. Now nothing can be more useless than such an examination. If we compare the information afforded by an inspection of the serous membranes of the three great cavities, we shall find that the least is given by an examination of that of the abdomen. Disease of the substance of the brain is rare without affections of its investing membrane, disease of the substance of the lung is exceedingly rare without the occurrence of disease of the pleura, but you may have most extensive and fatal disease of the intestinal canal, without the slightest lesion of the peritoneum. In this point, therefore, it differs from the pleura, and from the arachnoid membrane. The fact of the rarity of disease of the peritoneum in cases of disease affecting the parts beneath, was noticed by Dr. Graves and myself, in our report of the Meath Hospital, and also by Mr. Annesley, in his account of the diseases of India. You will see cases of hepatic abscess, which present a distinct tumour externally, and where you can detect a perceptible fluctuation; and yet, if you examine these cases after death, you may not find any adhesions of the peritoneum, even in the situation of the abscess. You will find the mucous and muscular coats of the colon extensively destroyed, you will see the stomach all but perforated, you will meet with cases where the whole ileum is one extensive sheet of ulcerations, with no disease in the adjacent peritoneum.

In entering on the consideration of diseases of the digestive system, we shall begin first with the mucous expansion of the stomach and intestines, and then proceed to the affections of the solid viscera connected with them. The mucous surface of the stomach and intestines is of enormous extent and extraordinary sensibility, possessed of innumerable and powerful sympathies; its influence is felt by almost every organ in the body, formed for receiving and elaborating every thing destined for nutrition; its conditions, both in health and disease, are entitled to the deepest and most attentive consideration. To facilitate the study of its affections, and for the sake of some practical arrangement, we shall divide its diseases

into five classes, beginning with the œsophagus, or that portion of the digestive tube which is above the diaphragm, and then proceeding to the stomach, duodenum, ileum, colon, and rectum. But, in order to give you a clear idea of diseases of the intestinal canal, I shall commence with diseases of the stomach; because, if you consider the whole range of animal life, you will find that its functions are the most important, the stomach constituting, as it were, the source and fountain of life, which is nutrition, and giving by its existence a character to all the upper classes of animals. No organ possesses such remarkable sympathies as the stomach, whether we look upon them as sympathies of organic or of animal life, none possesses such remarkable power and influence in modifying the condition of every part of the system. But, putting aside physiological reasons, let us come to practical matters. The success of almost every form of medical treatment, all the advantages to be derived from the administration of internal medicine, depend upon the stomach; in fact, in whatever point of view we consider it, we must look upon a knowledge of the state of the stomach as the great key to sound and successful practice.

It is a most useful reflection to consider the extraordinary frequency of disease in some portion of the digestive tube. It is now admitted by every person possessed of experience in the causes of mortality, that more human beings die with acute or chronic diseases of the digestive tube than with diseases of any other part of the system. This has been established by numerous investigations, and is admitted by the best pathologists; and, indeed, I think it can be easily accounted for, when we call to mind how many persons die of some form of fever or other, when we look to the ravages of remittent and yellow fever, to the hundreds of thousands who annually perish by the various classes of fevers. Now, in almost every one of these cases, disease of the digestive system forms one of the most prominent pathological characters. Recollect, besides, all that die of dysentery, whether sporadic or simple, and here is inflammation of the colon; see, too, how many die with diarrhœa—here, too, there is intestinal disease; remember how many die of the malignant intermittent of the West Indies, in which unequivocal proofs of disease of the stomach and intestines have been found. Observe what a close connection there is between tubes mesenterica, and inflammation of the mucous membrane and surface of the intestines; think what a vast number of persons fall victims to the harassing effects of constipation and dyspepsia; and recollect that there is a host of diseases in which the train of morbid phenomena commences in the digestive system, and then exhibits itself by functional alteration or organic disease of other parts.

We recognise the presence of disease in the digestive tube, first, by the local phenomena and the lesion of the digestive function, and next, by the sympathetic relations of other parts, by the sympathies of the respiratory system, of the circulation of the skin, and of the nervous system. I shall enumerate the local phenomena and functional lesions: vomiting, anorexia, thirst, jaundice, pain,

tenderness on pressure, tympanitis, changes in the character and quality of the discharges, constipation. Here are a set of functional lesions and local phenomena; let us now consider the sympathetic relations; these are fever, heat of skin, suppression of the cutaneous secretion, suppression of the secretion of urine, morbid states of the tongue and pulse, pains in the chest and cough, hurried breathing, and palpitations of the heart. In the next place, we may have prostration of strength, delirium, coma, convulsions, tetanic spasms, and other symptoms of functional disease of the brain; these are all sympathies of relation. Now, in the first place, I have to remark, that there is a great deal of variety in the combination of these symptoms. On what does this depend? on a variety of circumstances; sometimes on the intensity or extent of the inflammation: sometimes on the situation of the disease; sometimes on the complication of the affection; sometimes on the various modes and degrees of susceptibility of the individual. All these causes tend to produce a great variety in the disease, and an extensive modification of the sympathetic relations. For instance, in some cases inflammation of the stomach and intestines is so slight that the patient is not prevented from going about and pursuing his ordinary avocations; in others, on the contrary, the patients are struck down at once by the violence of the disease, and are carried off by the fever which accompanies it before the inflammation is completely developed. It varies also according to situation; there is a difference between gastritis and dysentery: in the former we have an inactive state of the great intestine, and consequent constipation; in the latter, the colon is thrown into violent action, and there are frequent dejections. Disease of the duodenum is attended with a very remarkable peculiarity, being very frequently complicated with jaundice; here is a modification produced by situation. Again, inflammation of the ileum is attended with a very curious peculiarity, namely, the absence of pain. The patient states, that he feels unwell, he has obscure symptoms of intestinal disease, but it is neither dysentery nor gastritis; you investigate it with care and find that the ileum is in a state of inflammation. Yet the patient does not complain of any pain, and this is another peculiarity depending on situation.

But in considering the differences which depend upon intensity, extent, and situation of disease of the intestinal canal, we must not omit those which depend upon tissue. If disease be confined to the mucous membrane of the intestines alone, we may have an extremely diffused and extensive inflammation, sufficient to destroy life, without any pain being complained of by the patient; it is a painless though fatal disease. Recollect this, extensive and fatal inflammation without pain. In former times the ideas of pain and inflammation were inseparable. Thanks to the light which pathology has shed upon modern medical science we are now acquainted with this seeming anomaly, and can conceive the existence of extensive disease of mucous surfaces unaccompanied by pain. But let the inflammation seize on the muscular tissue, the character of

the disease is instantly changed, and the pain is dreadful. Here is a case in which difference of tissue is to be taken into consideration.

The phenomena and sympathetic relations of intestinal disease may vary also according to its complication, and here we come to investigate one of the most beautiful laws of the human economy, namely, that the more complicated a disease is the more latent will be any local lesion. This is a point that should never be forgotten. For instance, enteritis by itself is much more easily recognised than when complicated with pneumonia, or with irritation of the brain, and gastritis is but too often completely masked by being combined with irritation of the bronchial mucous membrane. Lastly, we have the varieties which depend on different degrees of susceptibility. In one person we may have only slight cerebral irritation, in another high excitement, in a third delirium and extraordinary convulsions. The variety, then, in the modifications of disease, and the combination of sympathies, is very great, and is referable to the extent and the intensity of the inflammation, difference of situation, complication of disease, difference of tissue, and different degrees of susceptibility. I shall give examples of these at my next lecture, and then proceed to the pathology and treatment of gastritis.

LECTURE III.

Pathology and treatment of diseases of the digestive system—Different forms of gastritis—Pathology of this disease imperfectly understood by the ancients—Gastritis and enteritis not always found in connection—Phenomena characterising acute gastritis—Symptoms and sympathetic relations—Diagnosis—Gastritis simulating other diseases.

The consideration of the pathology and treatment of diseases of the digestive system shall occupy our attention to-day. I shall commence with the study of gastritis, and to this subject I would entreat your undivided attention; not that I have any thing very new to communicate, but because I believe that many of the statements, which are connected with this disease, will be found to rest on the basis of fact and truth, many of them will be found useful in your future practice, and this subject, I fear, is not sufficiently considered in the schools of medicine of this and the sister countries.

The older authors describe gastritis as occurring under two different forms, one of which they termed *phlegmonous*, and the other *erysipelatous*. The advanced students know the meaning of these terms, and that they are admitted as significant of different modifications of the inflammatory process, but to those who are not advanced I shall state that it is very difficult to give an accurate idea of these terms, so far as they are applicable to cases of internal

disease. But we may attempt a general definition by saying, that phlegmonous inflammation occurs in a good constitution, and under favourable circumstances, that it is an inflammation of a bold and distinct character, requiring and admitting of depletion, and, like that on the external parts, terminating in healthy suppuration, or adhesion. Erysipelatous inflammation is (described to be) a disease of a different kind, occurring in bad and debilitated constitutions, and under such circumstances that the same treatment, employed in the phlegmonous form, is more or less inadmissible; and when stimulants are necessary, if not in the commencement, at least at a very early period of the disease. It is quite impossible to found any system of pathology on this division into phlegmonous and erysipelatous; we are, however, sometimes obliged to make use of it for want of a better. The terms themselves are highly calculated to mislead. *Healthy inflammation*, which is all but a contradiction in terms, may occur in a debilitated constitution, and *erysipelatous* in a strong one. The latter of these, too, is particularly erroneous, as we now know that erysipelas may occur under opposite circumstances. In the one case, requiring the lancet and leeches, and purgation; in the other, demanding a stimulant and tonic treatment. In speaking of gastritis I do not intend to adopt this division, because it would be likely to embarrass you, and, in truth, it is unnecessary, as there is no difference in the (principles of) treatment, whatever may be the form of this inflammation. The proper way to consider gastritis is to look upon it as a disease, presenting, on the one hand, symptoms of extreme violence and urgent danger; on the other, feebly shadowed out by the phenomena of ordinary and slight indigestion. Between these there are many shades and numberless gradations. The phlegmonous gastritis of the old authors implied a violent and extensive inflammation, in which all the coats of the stomach were implicated; but, in treating of the subject of gastritis in these lectures, I shall only allude to inflammation of the mucous membrane and glandular apparatus of the stomach. The other tissues are sometimes engaged, but the mucous membrane, constituting the most important of these tissues, and forming an exquisitely delicate vasculo-nervous expansion, is, in the great majority of cases, the principal seat of inflammation, and to this I would direct your particular attention.

The true pathology of gastritis was but very imperfectly understood by the ancients. They knew enteritis and gastritis as intense inflammations of the coats of the stomach and intestinal canal, accompanied by violent pain and fever, but they had no conception of their various shades and modifications. For a knowledge of the true nature of gastritis, and of its numerous varieties, we are indebted to modern pathology, and it is the boast of pathological anatomy to say, that in this instance its labours have shed a broad and vivid light on a class of diseases previously involved in deep obscurity.

It has been stated, that it is impossible to separate the symptoms of gastritis from those which characterise enteritis, and the reason

given for this is, that the two affections frequently co-exist. This is a proposition of vast importance. It is said, that in cases where you have gastritis, the chances are that there is more or less of enteritis; but according to this doctrine, if a man has gastritis the probability is that he has inflammation of some other portion of the intestinal canal. Broussais, in the 138th proposition, makes the following observations:—"Inflammation of the stomach, or, as it is called, gastritis, is never found except in conjunction with disease of the small intestine. It is better, therefore, to give it the name of gastro-enteritis; and even in those cases, in which we have enteritis, we have gastritis as the irritative." Now if this proposition is true, it is one of very great importance, and entitled to a large share of our attention, in studying the phenomena and treatment of inflammation affecting the digestive tube. Pathology, however, has proved that these inflammations are not always found in connection. Andral gives many cases, in which disease existed separately in one or other portion of the intestinal canal; when it was found in the stomach and not in the duodenum or ileum, and when it was found in the ileum, but not in the duodenum or stomach. I myself have seen many examples of gastritis without disease of any other part of the digestive tube, and disease of various parts of the digestive tube without the co-existence of gastric inflammation. But I believe the proposition is generally true, particularly in cases of fever, in which you have secondary inflammation of the digestive tube during the course of the disease. When inflammation attacks the intestinal mucous surface during the progress of a fever, you will, in most cases, have these two diseases combined; the patient generally presenting symptoms of gastritis, and, at the same time, symptoms of enteritis affecting the lower third of the ileum.

Let us now proceed to investigate the phenomena which characterise acute gastritis. Here I must remark, that, as an idiopathic disease, acute gastritis is extremely rare. This is a very curious circumstance. When we compare the stomach with other viscera, we shall find that one of the most remarkable differences between it and other organs is, that it is much less liable to be attacked by violent inflammation, as an idiopathic affection. This is an interesting fact. So rare, indeed, is the violent form of gastritis, that our knowledge of the symptoms which indicate intense gastric inflammation is principally drawn from the study of cases of acute gastritis caused by swallowing corrosive poisons. We very seldom meet with an inflammation of the stomach, presenting those decided characters so frequently witnessed in similar affections of other organs. We may attempt to explain this fact, by considering what the functions of the stomach are, and by recollecting that it is the organ of the body, whose functions require that it should be most frequently in a state of great vascular excitement. Every one is aware that the vascularity of the stomach is amazingly increased during the act of digestion; but it is to be remembered that this is a physiological and not a pathological condition. If the stomach

were as liable to inflammation as other organs, it could no longer carry on its functions with safety; every meal would prove a stimulus sufficient to excite inflammation—every digestion would be followed by gastritis. Nature has provided against such accidents.

Let us take a brief review of the symptoms of acute gastritis:—intolerable thirst, desire for cold and acidulated drinks, constant nausea and vomiting, pain and burning sensation of heat about the stomach, and fever—these are the symptoms of a violent gastritis. It has been stated, that in gastritis the fever is at first inflammatory and afterwards typhoid. If authors mean by this, that the patient rapidly falls into a low typhoid state, the observation is true. There is no form of inflammation, except that which accompanies severe peritonitis, in which the typhoid state comes on so rapidly. Inflammations of the digestive tube differ, in general, from similar affections of other organs, chiefly in this—prostration rapidly supersedes excitement. A patient labouring under inflammation of the brain will exhibit, for a long time, decided symptoms of high excitement, and of what has been termed the *phlogistic diathesis*; acute pneumonia and inflammatory affections of other parts will go on for days, without prostration, and require the use of the lancet; but gastritis is a disease in which the inflammatory symptoms, as they are called, last but for a very short time. In violent cases the irritation of the stomach is excessive, and every thing is rejected. I have seen cold water thrown up almost immediately; I have seen effervescing draughts rejected the moment they were swallowed, and make the patient evidently worse. The epigastric region and the left hypochondrium are exquisitely tender on pressure, and the tenderness differs from that of peritonitis in this, that it is almost always localised. The patient screams with agony when you touch the epigastrium, but will bear pressure freely on the lower part of the abdomen.

Now, with respect to the sympathetic relations of gastritis, I have to remark, that they are very numerous. First, as to respiration—it is extremely quick and hurried; the heart, also, is violently excited; and hence gastritis has sometimes been mistaken for pneumonia and pericarditis. Sometimes we have bronchitic cough; the patient is restless, gets no sleep, and is extremely uneasy; his skin is hot, his bowels confined, his pulse rapid and small. In the second stage, he is beginning to sink, his features become contracted, his skin cold and pale, his extremities sunk below the natural temperature; he now bears pressure; the vomiting is changed for regurgitation of every thing he swallows; low delirium supervenes, and he dies.

It is of the greatest importance to attend to the sympathetic relations of gastritis, for this reason, that in many cases the local symptoms are all but wanting, and the disease is only to be known by its sympathetic relations. Before I enter on this subject I shall make one or two remarks on some symptoms which have not been attended to by many practitioners. One of these is an incapability

of swallowing, sometimes so great that all ingesta, whether fluid or solid, are rejected. This will sometimes arise from spasmodic stricture of the œsophagus or cardiac orifice of the stomach; and, as there has been no other cause revealed, by dissection, in several cases in which this symptom was present, we must admit this as one of the causes of the dysphagia, which, on some occasions, attends gastritis. This symptom is most commonly accompanied by tightness and oppression about the præcordia. The patient, feeling a load or weight, as he expresses it, in this situation, thinks it would be relieved by vomiting, and begs his medical attendant to give him an emetic, which is sometimes administered, and produces very bad effects. There is only one case in which an emetic can be given in gastritis, and that is, where indigestible or irritating substances in the stomach give rise to irritation, and when we cannot expect a favourable termination until we effect their removal.

There is another most disagreeable and distressing symptom, generally occurring in cases in which there is inflammation about the cardiac orifice of the stomach—I mean hiccup. Hiccup is a most harassing symptom; it does not allow the patient a moment's rest; in his brief and uneasy slumbers he is conscious of it, and is constantly awakened by it. Now, this is also one of the results of gastritis, with inflammation about the cardiac orifice. I say this, because I have seen it in many cases, in which there was distinct evidence of inflammation about the cardiac orifice of the stomach; and, in three instances, I have verified it by dissection. I do not mean to say that every case of hiccup is indicative of disease of the cardiac orifice, but I believe it is a very frequent accompaniment. The case of a celebrated professor of languages was a remarkable example. A short time previous to his death, he came from Liverpool in one of the steam packets. He was always subject to sea sickness; but on this occasion he was extremely ill, and vomited during the entire passage or sea voyage. He complained of his stomach for some time, and then got hiccup, which resisted every kind of treatment, and continued without any abatement up to the time of his death. On opening the stomach, this organ was found in a state of intense inflammation, particularly about the cardiac orifice. You can see the stomach (of which a very good preparation has been made by Dr. Houston) in the museum of the College of Surgeons. There was another very remarkable case in the Meath Hospital. A patient was admitted who had laboured under acute pneumonia, for which he was treated with tartar emetic, and the symptoms rapidly declined, but vomiting and hiccup came on, and the latter symptom continued until death. We opened the body eighteen hours after his demise, and found the lung quite healthy; but the stomach, and the cardiac orifice in particular, were, as in the case I have just mentioned, in a state of intense inflammation. When hiccup is the result of inflammation of the cardiac orifice, you will also frequently observe that the patient complains of pain in the lower part of the chest, along the course

of the diaphragm. These are some of the relations of gastritis, their connection with which is proved by their being relieved by draughts of cold water, leeching, and every other means calculated to remove inflammation of the stomach.

We come now to consider the state of the tongue. A vast deal of error and misconception prevails among British practitioners on this subject. Nothing is more common, than from the condition of the tongue to form an opinion as to the state of the alimentary canal. For instance, whether it is in a state of inflammation, whether there are sordes present or not, and whether it requires this or that medicine. All this is behind the actual state of medicine, and it is melancholy to think, what a vast quantity of mischief is done by those practitioners who take the tongue as the index of an inflammatory or non-inflammatory condition of the intestinal canal. The schools of Abernethy and Broussais are wrong in stating that the tongue will point out the state of the digestive tube. The connection between the state of the tongue and that of the stomach, has been lately made the subject of extensive clinical investigation by M. Andral; listen to his sentiments on this point. From the experience of a vast number of cases, he declares "that there is no constant relation between the state of the tongue and that of the stomach." In the next place he states, "that there is no modification of the one corresponding with any special modification of the other." "Thirdly, the stomach may be found in a certain state after death, with various conditions of the tongue during life." "Fourthly, we may have a diseased stomach with a healthful condition of the tongue, and diseased appearance of the tongue with a healthful state of the stomach." These are facts of the greatest importance. Let us now refer to Louis. In giving an account of the gastritis which accompanies fever, he states that in many of the worst cases the appearance of the tongue was natural, in fact, that there was not the slightest relation between the tongue and the stomach. It is fair, however, to observe here, that both these pathologists drew their information only from cases of gastritis, occurring in fever. But it has also been frequently observed, that even in idiopathic cases there is a want of correspondence between the condition of the tongue and stomach, and we have seen several instances of this in the Meath Hospital. I believe we should be wrong in taking the tongue alone as our guide in the treatment of intestinal derangement, whether existing in the stomach or any other portion of the tube, and this I state as the conclusion which I have drawn from my own experience, in gastric and enteric inflammation. Yet how many will you see taking the tongue as the unerring index of various conditions of the digestive tube? hundreds and thousands. It is unquestionably true, that in certain cases of gastritis, particular morbid appearances, as redness, dryness, pointing, and a tremulous state of the tongue, are observed, but what I wish to impress on you is, that it is *necessary that these phenomena should coincide with other symptoms*. I do not wish

you to believe, that the inspection of the tongue, or the knowledge derived from its appearance, is useless, particularly in cases of fever: the state of the tongue is never to be overlooked, but you should understand on what principle it is to be examined. You should examine the tongue not so much as a guide to the knowledge of local disease, but *as an index of the condition of the general system*. For instance, if, during the course of a fever, the appearance of this organ changes and becomes more favourable, it is a sign that the whole disease has taken a favourable turn, and vice versâ. This is the proper way to look at the tongue in fever, not as reflecting any particular state of the intestinal canal, but as being indicative of some modification of the whole economy.

Let us now consider the sympathetic relations of the nervous and respiratory systems in gastritis. This is a very curious and interesting point in the study of gastric disease. I may mention here, that these relations are subject to considerable variety, and differ according to the peculiar predisposition of the individual. If a person of nervous habit gets gastritis, he will be very liable to have sympathetic affections of some part of the nervous system; but if he is a person with unsound lungs, the irritation will be transferred to the respiratory apparatus. Can we define these irritations? I believe the best definition we can give of them is, that they are affections of some organ, which are the result of sympathy; and that they are at first functional, but afterwards become organic. A person of nervous habit, labouring under gastritis, will frequently have his head sympathetically affected; he will complain of headache, more or less intense; toss about and get no sleep; still he has no actual disease of the brain. But let the cerebral irritation go on, let the pain and uneasiness and watchfulness continue, and he will finally get arachnitis. So, too, with respect to the lung; the patient has hurried breathing and cough, without any of the stethoscopic signs of pulmonary disease; but if these symptoms continue for any length of time, or if the irritation be severe, he will get pneumonia or bronchitis. Observe the importance of this law with reference to treatment, because it shows you that you cannot always expect to remove sympathetic affections by attacking the original source of disease; for if functional derangement, produced by sympathetic irritation, has gone so far as to become organic, you must direct your attention to parts which have been secondarily engaged, as well as to those which are primarily affected. Every one is aware of the effects of particular states of the stomach on the brain, and of the influence which the brain exercises over the stomach. Most individuals know, that by grief or strong mental emotion the appetite is completely removed; and that after a surfeit, or from taking bad and indigestible food, a person will get sick headache. If this happens every day under ordinary circumstances, and where the original affection is so slight that it does not interfere with the usual avocations of the patient, you can readily conceive how intense the sympathetic irritations may be in a case of violent gastritis. The headache is

frequently intense, the patient is extremely restless, there is considerable intolerance of light, delirium, tetanic spasms, and other symptoms characteristic of inflammation of the brain. There are numerous cases on record in which these symptoms were particularly noticed, and it was supposed that the brain was in a state of inflammation, but on dissection there was no disease found, except in the mucous membrane of the stomach. There are many cases, too, in which medical men, not aware of the extent of these relations, looked upon the disease as a pure cerebral affection, and directed their whole attention to the brain. They certainly succeeded in modifying the apparent disease, but as they took no steps to remove its cause, the patients generally sunk from an unsuspected gastritis. There is one important law with respect to inflammation of the stomach, which perhaps may be fairly applied to all inflammatory affections of the digestive tube. When inflammation of the stomach or any other portion of the intestinal canal has continued for some time, and when the disease has attained a certain degree of violence, the local symptoms may subside, and the gastritis or enteritis will be represented by disease of some other organ, by symptoms of an affection of the brain or its investments, or by symptoms of disease of the lining membrane or parenchymatous tissue of the lung. I shall endeavour to explain this. Here is a case taken from the *Clinique Médicale* of Andrel.

“A middle-aged man, four days before his entrance into the hospital, was seized with bilious vomiting, epigastric pain, and fever. (Here is a certain case of gastritis.) In about twenty-four hours after the invasion of these symptoms, he first perceived a difficulty in depressing the lower jaw, and a violent trismus was established, which continued for the two following days; at the end of this time he entered the hospital in the following state:—trismus, the head drawn backwards and forcibly retained in this position by the muscles which are inserted into the occipital region; rigidity of all the extremities; abdomen hard as a board; intellect perfect. Notwithstanding the trismus, the patient could articulate with sufficient distinctness to give the above account of his case. *From the time when the first tetanic symptoms appeared the vomiting and epigastric pain ceased.* He died on the evening of his admission. On dissection no appreciable alteration of structure was found in the brain or spinal marrow; the meninges of the brain were very slightly vascular, but those of the spinal marrow pale. The whole surface of the stomach presented an intense red colour, which was at first concealed by a thick layer of mucosities. The remainder of the digestive tube was perfectly healthy, and the thoracic organs were natural.” This may be called a case of tetanus; and it is a curious fact, that when the tetanic spasms came on, the vomiting and other symptoms of gastritis subsided. Now this is what I wish to direct your attention to. A man dies with symptoms of an affection of the brain, the head is opened after death, there is no trace of cerebral disease found, but the whole surface of the stomach is discovered to be in a state of intense inflammation. That

the stomach was inflamed is proved by the vomiting and epigastric pain which existed during life, as well as by the vascularity which was revealed by dissection; and there can be no doubt that this condition was the result of an intense inflammation, as there was no other cause to produce it.

Last year, a patient was admitted into the Meath Hospital, labouring under violent maniacal excitement, his eyes blood-shot, and his aspect ferocious. He had thirst, a dry fissured tongue, a quick weak pulse, and constipated bowels. There was no epigastric tenderness, no vomiting, in fact, none of the prominent symptoms of gastritis complained of. On the third day the belly was slightly tender and tympanitic. The cerebral symptoms increased so as to require the use of the strait waistcoat, and continued with violence until a short time before death, which occurred on the eighth day. On dissection there was no appearance of inflammation found in the brain or its membranes, but there was a vast extent of disease in the digestive tube. The splenic extremity of the stomach presented several patches of vascularity, and its mucous coat was softened; the lower half of the ileum, the cæcum, and part of the ascending colon, were in a state of intense inflammation, and dotted all over with numberless ulcerations.

You observe of what importance the knowledge of these facts will be to you in practice, and how much it should become the object of your study, since you will thereby be able to make the diagnosis of gastritis from the sympathetic relations, though the usual symptoms are more or less absent. Even in cases of this kind, in which the symptoms have subsided on the appearance of these sympathetic irritations, the judicious practitioner will not be diverted from directing his attention to the source of the original mischief; nor will he, because the local symptoms have disappeared, conclude that the disease has therefore been removed from the stomach. Many examples of this *apparent* transition of disease are to be seen in cases of children, in which an inflammation of the upper part of the digestive tube frequently simulates hydrocephalus, and where the headache, delirium, and intolerance of light, are completely removed by the application of leeches to the epigastrium. I have seen this occur many times, and would entreat your particular attention to it. I believe many children are lost from the want of correct notions on this subject on the part of their medical attendants. The phenomena present in such cases are certainly those which characterise hydrocephalus; but you should always investigate them with care, and ascertain whether the disease has commenced with symptoms of inflammation of the mucous membrane, of the stomach, or bowels; and if you find that it has originated in this way, and that the cerebral symptoms have not gone too far, direct your treatment in the first place to the digestive tube. It is extraordinary how rapidly all the symptoms of apparent cerebral disease subside under this plan of treatment. I must mention here to you a very remarkable case of enteritis, which simulated local disease of the substance of the

brain. A girl who had received an injury was admitted into the Meath Hospital; she was treated with purgative medicines, and was "*discharged cured!*" In a few days afterwards she was re-admitted with pain in the head, and *violent spasmodic contractions of the fore-arm, by which the fingers were bent so forcibly that the nails were driven into the hand.* There was no thirst, vomiting, or abdominal tenderness. She died a few days after her admission; and on dissection the brain was found perfectly healthy, the viscera of the thorax were in the normal state, the stomach presented nothing remarkable, but the ileum was almost one sheet of deep and recent ulcers. The result of this case is important, also, in another point of view. You know that spasmodic contractions of the upper extremity are believed by certain pathologists to point out an inflammatory softening of the optic thalamus, and its prolongations. Here we had the symptom, at all events, without the corresponding lesion.

I shall reserve the subject of sympathetic irritations of the respiratory system until Wednesday, when I expect to be able to finish the pathology and treatment of gastritis.

LECTURE IV.

Gastritis—No one symptom decidedly indicative of the particular condition of any organ—Sympathetic irritation liable to terminate in organic disease—Sympathetic relations as connected with the viscera of the thorax—Treatment of simple acute gastritis—Antiphlogistic remedies—Purgative medicines injurious—Enemas and injections—Use of ice beneficial—Effervescing medicines hurtful.

You recollect that at our last meeting I endeavoured to lay before you some of the general facts connected with the pathology of gastritis, and showed you that the statement made by Broussais, that inflammation of the mucous membrane of the stomach is always accompanied by a similar affection of some part of the intestines, has not been confirmed by the investigations of more recent observers; but, on the contrary, that their experience goes to disprove, in various instances, the validity of this assertion. But, when I say that this statement has been disproved, it is only as taken in the general and extended sense. The fact of their frequent coexistence has been proved; the statement that they are always associated has been found incorrect. Another thing connected with this, which has been also established by repeated observation, is, that the cases in which they are commonly combined are those in which a secondary affection of the mucous surface of the digestive tube comes on during the course of a fever; so that, if in fever a gastritis supervenes, you will commonly have enteritis; or if the fever be complicated with enteric inflammation, the mucous surface of the stomach will partake in the diseased action.

I have described some of the more prominent symptoms of gastritis, and directed your attention not only to the ordinary symptoms, as mentioned in books, but also to others which have either been passed over, or slightly noticed, by authors; as, for instance, dysphagia, oppression and sense of constriction about the præcordia, globus, pains relieved by cold and acid drinks, &c., and that obstinate hiccup, which, in cases where there is reason to suspect gastritis, marks inflammation of the cardiac orifice of the stomach. I stated that hiccup alone does not prove the existence of inflammation of the cardiac orifice of the stomach, unless where symptoms, indicative of gastric inflammation, prevail at the same time. I laid before you the actual state of the case with respect to the value and certainty of diagnosis, as derived from an inspection of the tongue; and showed you that no reliance can be placed on it, since it has been proved that we have the most opposite conditions of the digestive tubes, accompanied by a similar condition of the tongue; and that there is no peculiar modification of the one, corresponding exactly and constantly with any peculiar modification of the other. The conclusion to be deduced from these facts is, that in the treatment of inflammatory affections of the digestive tube, we are not authorised, and would frequently err, in taking the tongue alone as our guide in practice; and you may lay down this as a rule, and an important one:—if we look through the whole range of the history of medicine, we shall scarcely be able to point out any symptom which, taken singly, is decidedly indicative of any one particular condition of an organ. You will find that this proposition is not only extensive in its scope and relations, but also of extreme value in its application. You will commonly hear persons saying, this is such a disease, for this symptom is present, and that is such a disease, for such a symptom is extremely well marked. But there is no single symptom which points out, with certainty, any peculiar condition; and to arrive at a just and well-grounded diagnosis, you must always take the whole group of existing phenomena, connect the lights which they collectively throw upon the case, and then make a cautious decision. It may be objected to this that there are particular signs; as, for instance, the stethoscopic, which point out distinctly particular states of organs. It is said that *gargouillement* is decidedly indicative of a phthisical cavity, that *ægophony* points out a particular stage of pleuritic effusion, and that *metallic tinkling* is an unequivocal proof of pneumothorax. This, however, is not the fact; even in these cases you are not authorised to depend on any sign or symptom *taken alone*. If you ground your decision on any individual sign, you will very often fail in arriving at the truth.

I showed you that the sympathetic irritation of gastritis varied according to the peculiar character of the disease, and the habit and degree of susceptibility of the patient; that, generally speaking, the more intense the disease is, the more numerous are its irritations; but that, in all cases, they are considerably modified by predisposition (I use this term for want of a better), the sympa-

thetic irritation being reflected on the lungs in cases where these organs are naturally unsound, and on the brain, where the patients have a tendency to disease of that organ. I endeavoured, also, to impress on you the fact, that these irritations are at first functional; but when long continued, or marked by extreme severity, they are very apt to terminate in organic disease. I illustrated this point by several examples; I shall give a few more of this kind before I enter on the treatment of gastritis.

If a patient labouring under acute gastritis has a bad cough, if respiration be very much hurried, and the distress of the chest great, and that these symptoms are overlooked or neglected, you will find that the cough, which was at first only a result of functional disease, will at last point out an organic affection of the lung. Again; let a patient, labouring under gastritis, have severe headache, restlessness, and irritation; suffer these symptoms to go on and increase in violence, and the great probability is, that they will terminate in arachnitis. The obvious deduction from these facts is, that when a sympathetic irritation has existed for some time in a state of considerable intensity, it is very probable that there is more or less of organic derangement produced, and we are not to expect to be able to remove it by merely attacking the original seat of the disease.

The last great rule which I endeavoured to impress upon you was, that where these sympathetic irritations, these affections of the nervous, respiratory, and circulating systems, were extremely well marked, the ordinary local symptoms were more or less wanting, but that this does not by any means imply the subsidence of the original disease. This is a most important law in pathology.

In my last lecture, I entered into a detail of the sympathetic irritations connected with the brain and other parts of the nervous system; to-day we shall consider the sympathetic relations, as connected with the viscera of the thorax. If you look to the cases of acute gastritis, mentioned in works on toxicology, you will find that in cases of gastritis, produced by swallowing corrosive poisons, the patient has often frequent hard cough, the breathing is at first hurried, then becomes protracted and laborious, and that death is generally ushered in by tracheal rattle. The same symptoms are observed in cases of acute idiopathic gastritis; hurried breathing, extraordinary hard and almost laryngeal cough, sometimes occurring in paroxysms, sometimes constant. For the first few days it is, generally speaking, dry; as it progresses, there is more or less expectoration. At first, it is the result of sympathy; there is as yet no organic affection of the respiratory system, and the disease is purely functional; still it is of importance, and entitled to your particular attention, because, in consequence of the apparent identity of the symptoms, it is often mistaken for disease of the substance of the lung, or its mucous lining. The existence of a gastritis is frequently overlooked; the ordinary symptoms of pain in the region of the stomach, tenderness on pressure, and thirst, are overlooked, and the sympathetic relations alone are attended

to. Observe what mischief may result from this error. The treatment of acute affections of the lining membrane, or parenchymatous tissue of the lung, is very different from the treatment of a gastritis. In the one case bleeding is necessary; in the other, its efficacy may be doubtful, or the practice even dangerous. In one, tartar emetic is one of the best and most expeditious means of effecting a cure; in the other, the use of antimonials has the worst effect. It will strike you that in such cases percussion and the stethoscope are of inestimable value. You are called to attend a patient in fever, you find he has cough, hurried breathing, and perhaps pain in the chest; from a consideration of the history of the case, and the primary symptoms, you have reason to think the case is one of gastritis, and you wish to know whether the symptoms be purely sympathetic, or caused by organic disease of the lung. In such a case, a person without the knowledge of the stethoscope is completely helpless, and unable to decide the point. This, I assure you, is a very common case, and should be a strong inducement to the study of the stethoscope. What advantage does a knowledge of the stethoscope give? It leads to the formation of an accurate diagnosis; it points out either that there is no disease in the lung, or if there be, that it is not sufficient to account for the symptom, and therefore that you should look for its cause in some other situation. You find a person with laboured and rapid breathing, perhaps fifty or sixty in a minute; you are struck with the apparent lesion of the respiratory system, but on percussing the chest, and using the stethoscope, you find the respiration perfectly clear, or perhaps a slight bronchitis, insufficient to account for such violent symptoms. Where such phenomena are observed, you will often find that they are connected with a gastritis, particularly where there is fever, and the local signs of a gastric inflammation. I can tell you, from a most extensive experience, that in such cases you can inform the patient's friends, that the most sudden and decided relief will be experienced from the use of iced water, and the application of leeches to the epigastrium. You can have hardly an idea of the rapidity with which all the symptoms of pulmonary irritation are removed by this practice. Cases of this extraordinary sympathetic irritation are very common in children, but you will also frequently meet with them in adults.

I have been called to decide the question, whether a disease was pneumonia or gastritis, where there was a difference of opinion between two practitioners. Now, it is very easy to come to a proper decision in such cases. There is one point which you should always hold in view, and that is, *the length of time the symptoms have lasted*. If symptoms of pulmonary disease have been going on for four or five days, and, at the end of that time, you find that there is no perceptible organic disease of the lung, you may be certain that it is gastric irritation; because if it were organic disease of the lung, it would have shown itself before that time, and could be detected by percussion or by the stethoscope. We have had many cases of these sympathetic irritations of the

lungs in the Meath Hospital, which recovered under the treatment for gastritis; and where the patients, by some excess or error in diet, brought on the pulmonary symptoms again, they were removed a second time by putting them on a low diet. Before I quit this subject, I wish to make one remark, by way of caution. When you have discovered the existence of those sympathetic irritations, you should not be thrown off your guard, and consider them only as functional affections. You should examine the next day and the day after, for you may find that in a very short space of time actual disease of the lung has taken place. You should be, therefore, watchful, and never omit making a daily examination; for if the sympathetic irritation be severe, it is very apt to run into actual organic disease.

We now come to speak of the treatment of simple acute gastritis. Here there are three principal indications. One of these is to remove inflammation as speedily as possible. You cannot, as under other circumstances, leave this disease to nature; the organ affected is one of the utmost importance to life; and if you do not cut it short at once, a typhoid state comes on, to which the ordinary and efficient means of antiphlogistic treatment are inapplicable. The first indication, then, is to cut short the inflammation as speedily as possible. The next thing is to prevent the introduction of any thing into the stomach which will excite the physiological action of that viscus. You are aware, that while the stomach is engaged in the process of digestion, its vascularity is very much increased, and that this, which in health is merely a physiological condition, is unaccompanied by any kind of danger. But in a state of disease it proves a source of violent excitement, and superadds very much to the existing inflammation. You must, therefore, be extremely cautious with respect to what enters your patient's stomach, and carefully remove every thing capable of adding to the excitement which always attends gastritis. The third indication in the treatment is to modify and remove the sympathetic or secondary irritations.

Now I shall suppose that we have to treat a case of simple acute gastritis, not produced by the swallowing of corrosive poison, or indigestible food. Here we have a patient labouring under violent inflammation of one of the most important organs in the body; and the question is, are you to adopt the ordinary and usual mode of stopping inflammation by opening a vein in the arm? I must here state, that we are very much in want of a series of well-established facts to guide our practice on this point, and to inform us how far general bleeding is useful in acute inflammation of the stomach. At the present period, the question is by no means settled, and the practice is uncertain. I believe, however, that when we are called in at an early period of the disease, where the patient is young and robust, the stomach previously healthy, the fever high, and the pain great, we may have recourse to general bleeding with advantage; bearing this in mind, however, that you are not to expect to cut short the inflammation by the use of the

lancet. Inflammations of the mucous membrane of the stomach and bowels, and perhaps of the lungs, are not to be overcome at once by the lancet; the only cases in which you can expect to cut short an inflammatory attack, are those in which the parenchymatous tissue of an organ, or its serous membrane, is affected. This is a general and important law. You will often be able to cut short a hepatitis or a pneumonia by a single bleeding, but you will not by the same means be able to repress a bronchitis or an inflammation of the mucous membrane of the intestines. If you bleed in gastritis, bleed at an early period; not too largely, or with the expectation of cutting short the inflammation, but in order to prepare your patient for the grand agent in effecting a cure—local bleeding. This is the principle on which you are to employ the lancet.

In the treatment of gastritis there is nothing more useful, nothing more decidedly efficacious, than the free and repeated application of leeches, whether the case be idiopathic, or produced by the swallowing of a corrosive poison. In this treatment of acute gastritis, you will frequently see, perhaps, the most striking instances of the rapid and decided utility of medical treatment; you will see the vomiting subside almost immediately, the epigastric pain and tenderness disappear, the cough and headache relieved, the fever subside, and the tongue change, after the application of leeches. To remove the symptoms, the best and most effectual means are leeches; and these must be applied again and again, according to the duration and obstinacy of the symptoms. Here I wish to make one remark of importance. From an opinion, very prevalent in former times, that pain and inflammation were inseparable, the older practitioners thought that when the pain ceased the inflammation also ceased; and hence many of our predecessors, and I fear some of our cotemporaries, never think of reapplying leeches, no matter what the existing symptoms may be, if pain has been relieved by the first application. Nothing is more erroneous than this practice. It frequently happens that the pain and epigastric tenderness are removed by the first application of leeches, but the breathing is still quick, the fever high, and the thirst ardent. So long as these symptoms remain, the inflammation of the stomach is still going on. The mere subsidence of pain or tenderness of the epigastrium should never prevent us from resorting to the application of leeches. In leeching the belly for inflammation of the stomach or bowels, it is a common practice to apply a poultice over the leech bites, with the view of getting away as much blood as possible. I am not inclined to approve of this practice. The weight of a poultice is frequently troublesome, and the heat produced by it disagreeable; the patients desire cold, and for this purpose they will often throw off their bed-clothes, feeling a degree of relief from exposing the epigastrium to a stream of cool air. Some practitioners have applied pounded ice over the stomach with good effects, as we see it frequently applied to the head with the same results in cases of encephalitis. Again: the

application of poultices causes an oozing hemorrhage, the amount of which it is impossible to calculate, which is often hard to be arrested, and which, in debilitated persons and children, has the effect of lessening the powers of life without removing the original disease. It is much better to leech again and again than to do this. Where there is not much epigastric tenderness, you may apply a cupping-glass over the leech bites with advantage, as you can get away as much blood as you choose, and the tendency to after-hemorrhage from the leech bites is diminished by the application of the cupping-glass. In very young subjects, the tendency to obstinate hemorrhage from leech bites is so great, that many practitioners are afraid to use leeches, and I believe some children have been sacrificed to this fear. The best mode of managing this is, if the leech bites cannot be stopped by the ordinary means (and in very young children they seldom can), to stop them at once by the application of caustic. Do not lose time in trying to arrest the flow of blood with flour, or lint, or sitcking plaster; wipe the blood off the bite with a piece of soft dry lint, plunge into it a piece of lunar caustic, scraped to a point, give it a turn or two, and the whole thing is settled; and you can generally go away with the agreeable consciousness of having prevented all further danger, and without being uneasy lest your patient should bleed to death in your absence.

With respect to the management of the bowels in acute gastritis, a few observations will suffice. You will always have to obviate the effects of constipation; both in the acute and chronic forms of the disease there is always more or less constipation; in fact, the same condition of the bowels is generally observed in both. Now, if you attempt to relieve this constipation in acute gastritis, by administering purgatives, you will most certainly do a vast deal of mischief. Nothing can exceed the irritability of the stomach in such cases; the mildest purgatives are instantly rejected, even cold water, or effervescing draughts are often not retained, and a single pill or powder is frequently thrown up the moment it is swallowed. Under such circumstances, it is plain that the administration of purgative medicine is totally out of the question. Even though the stomach should retain the purgative, you purchase its operation at too dear a price; for it invariably proves a source of violent exacerbation, kindling fresh inflammation in an organ already too much excited. In this state of things, the best thing you can employ to remove constipation is a purgative enema, repeating it according to the urgency or necessity of the case. Where there is no inflammation in the lower part of the intestinal canal, you may employ injections of a strong and stimulating nature, with the view not merely of opening the bowels, but also of exercising a powerful revulsive action. I shall mention here an interesting fact, proving that stimulant injections have a decided revulsive effect; and that their influence extends not only to other portions of the intestinal tube, but also to distant parts of the system. In South America, where, from the heat of the climate, and the preva-

lence of bilious affections, sick headache is a very common and distressing symptom, a common mode of cure is to throw up the rectum an extraordinary enema, composed of fresh capsicum, and other aromatic stimulants. The irritation which this produces acts as a very efficacious and speedy revulsive, causing the almost immediate removal of the cerebral symptoms.

In those cases of gastritis, where not only purgatives, but even the mildest substances, are rejected, the plain common-sense rule is to give nothing. Where cold water is borne by the stomach, it may be taken in small quantities, as often as the patient requires it. Solid ice, too, may be given with decided benefit. There is a mistake which prevails with respect to the employment of ice in gastritis, which I wish to correct. Some persons objected to this use, and reason in this way:—persons who have taken a quantity of cold water, or ice, when heated by exercise, have been frequently attacked with gastritis and fever, and consequently the use of these substances must be attended with danger in case of gastric inflammation. This, however, is false reasoning; you need not be afraid to order your patient ice, *ad libitum*; depend upon it, there is no danger in employing either ice or cold water in gastritis. There is nothing so grateful to the patient as ice. Let a quantity of it be broken into small pieces, about the size of a walnut; let your patient take one of these pieces, and, having held it in his mouth for a few moments to soften down its angles, let him swallow it whole. The effect produced by this on the inflamed surface of the stomach is exceedingly grateful, and the patient has scarcely swallowed one portion when he calls for another with avidity. It will be no harm if I should here mention to you a secret worth knowing. There are few things so good for that miserable sickness of the stomach, which some of you may have felt after a night's jollification with a set of pleasant fellows, as a glass of ice; Byron's hock and soda-water are nothing to it.

After the first violent symptoms of the disease have been subdued, I believe the very best thing which can be given is cold chicken broth. The point which we are always to keep in view is, to remove inflammation from the stomach, and this should regulate the use of every thing taken into the stomach. I believe we might derive much advantage from anodyne injections in gastritis. I cannot say that I have ever employed them in such cases; but if I were to reason from their utility in other forms of abdominal inflammation, I should be induced to look upon them as entitled to some consideration. There is another point to which I will briefly advert. In the treatment of acute gastritis, there is nothing more commonly used than effervescing draughts; yet I have frequently seen them produce distinct irritation of the stomach. In cases where gastric irritability is excessive, I would not advise you to give effervescing draughts, or, if you do, watch their immediate effect; see how the first one has agreed with the stomach before you venture to give any more. Patients labouring under this disease should be kept extremely quiet, as frequently a slight motion

brings back the vomiting. Every thing which is swallowed should be in small quantity; a large quantity of any substance frequently causes a return of the vomiting, by distending and irritating the stomach. One of the best things you can give, and the best way of giving it, is iced lemonade, giving a tablespoonful from time to time. The extremities, which are generally cold in cases of intestinal disease, should be swathed in warm flannel.

I shall mention here a rule which should be carefully observed in the after treatment. A patient has recovered from the violent symptoms of the disease; the fever, thirst, pain, epigastric tenderness, and sympathetic affections, have subsided; but he still is confined to bed, and in a state of great debility. Some patients, under these circumstances, have been unfortunately lost by allowing them to sit up in bed, or on the night chair. The nurse will sometimes, through ignorance, suffer a patient, thus enfeebled, to risk his life by sitting up in bed; sometimes, during the course of the night, she is overcome by sleep; the patient has a call to empty his bowels; and not wishing to disturb her, attempts to get up, and is found, in some time afterwards, sitting on the night chair quite dead. This is an unfortunate termination for the physician as well as the patient. A German author, Hoffmann, has written a treatise on the danger of the erect position after acute diseases; and in the course of the work, which is a very interesting one, he cites numerous instances of its bad effects. Not very long since, a patient was lost in the Meath Hospital, by the nurse allowing him to sit up after a severe attack of enteritis. Such, also, was the melancholy cause of death in the case of the late Mr. Hewson, one of my best and earliest friends. He got a severe attack, which was subdued with difficulty, and his convalescence was doubtful and protracted. One night, in the absence of his attendant, he got up for the purpose of emptying his bowels, and was found, some time afterwards, on the night chair, nearly dead. He was immediately brought back to bed, and the necessary means employed to relieve him, but without much benefit, for he never recovered the effect produced on his debilitated frame.

LECTURE V.

Pathology and treatment of gastritis—Application of blisters—Emetics can be seldom used in acute gastritis—Hæmatemesis and delirium tremens complicated with gastritis—Treatment of these affections—Dyspepsia, or chronic gastritis—Hypochondriasis—Termination of chronic gastritis.

There is one point connected with the treatment of gastritis which I have not yet touched upon—the use of blisters; and as this is the first time I have spoken of them, I shall make a few remarks on their general application.

It is a great error to think that blistering is a matter of course in inflammatory diseases, or that the proper period for their application should not be carefully marked. It is a common idea, that if a blister does no good it will do no harm; that it is probable some benefit may result from its employment, and that you may try it at all events. I need not tell you that all this is wrong, and that we must be guided by exact principles in this as well as in every other part of practical medicine. I am afraid there is a great deal of loose reasoning and empirical practice connected with this subject, even at the present day. Here is the general rule by which you should be invariably guided. No matter what kind of disease you have to deal with, if it be inflammatory, blistering in the early stage of it is decidedly improper. I might amplify this rule, and say, that if the disease be inflammatory and in its early stage, or if, under such circumstances, the symptoms require the general or local abstraction of blood, blisters cannot be used with propriety. The truth is, that many persons take a very limited view of this subject; they look upon blisters as merely revulsive agents, which, by their action on the surface, have the property of diminishing visceral inflammation. This I am willing to allow is true to a certain extent, but there is abundant evidence to prove, *that blisters have sometimes a direct stimulant effect on the suffering organ.* That this occasionally occurs has been established by many facts in medicine; and I have not the slightest doubt that the application of a blister over an organ in a state of high inflammatory excitement will certainly be productive of injurious consequences. But if you apply them at the period when stimulation is admissible and useful, (and there will always be such a period in every inflammation,) you then act on just principles, and will generally have the satisfaction of finding your practice successful. The greatest empiricism is sometimes practised in the application of blisters to the head in acute inflammation of the brain. You will see, in Mr. Porter's admirable work on the Pathology of the Larynx, how strongly he is opposed to the early use of blisters in acute laryngitis. Dr. Cheyne, also, may, among many others, be quoted in support of this doctrine.

If there is one system more than another likely to be injured by early blistering, it is the digestive. Broussais says that blisters

should not be applied in any of the stages of acute gastro-enteritis, and that in the early stage their application is the very height of malpractice. I do not go so far as to say that they should not be applied in any period of the disease, for when the skin is cool, the pulse lessened, and the local inflammation so far reduced as not to require the abstraction of any more blood, I think you may employ them with very considerable advantage. I shall again return to the subject of blisters; and will for the present merely remark, that blistering is almost always mismanaged, in consequence of persons who apply them being ignorant of their stimulating effects on organs. They generally allow them to remain on too long, and the consequence of this is often violent excitement of the organ over which they are applied, great constitutional irritation, strangury, and bad sores. The best mode of using them is to direct the person who prepares the blister to cover it with a piece of silver-paper before it is applied, and having put it on with the paper next the skin, to let it remain until a decided sense of smarting is produced, when it should be immediately removed. By adopting this plan, you will save yourself and your patient a great deal of inconvenience; you will have no strangury, stimulation of the whole economy, or excessive local irritation, and the inflamed surface will heal kindly. The mode (too often practised) of applying a blister sprinkled all over with an additional quantity of powdered cantharides, and leaving it on for twelve, twenty-four, or even thirty-six hours, particularly in the case of females, is nothing better than horse doctoring. During a seven years' experience in the hospital at Tours, Bretonneau, by attending to this principle, never had a case followed by these troublesome symptoms, and yet he never failed in producing the necessary degree of counter-irritation. The active principle of cantharides, being soluble in oil, exudes through the silver-paper in sufficient quantity to produce the necessary effect on the skin, without exposing the patient to the risk of having too much irritation excited by the direct application of the blistering plaster to the cutaneous surface.

With respect to emetics, I need not tell you that they can be very seldom used in acute gastritis, and that all your efforts should be directed to obviate and remove vomiting. But are we to interdict their use altogether? There are some few cases where we are compelled to use them; as, for instance, in cases of acute gastritis caused by swallowing corrosive poison, or by the irritation of indigestible food remaining in the stomach. The first step to be taken in a case of corrosive poisoning, is to evacuate the stomach. In the same way, when you are called to treat a case of gastritis produced by indigestible aliment, you must commence by giving an emetic. But even here the emetic is admissible only in the early period; and you should never trust to its operation for removing the gastritis altogether, unaided by other therapeutic means; nor are you to conclude that because you have produced vomiting you have succeeded in curing the disease. The same principles apply to the use of purgatives in enteritis as to emetics in gastric inflammation;

we should never have recourse to them except where inflammation is kindled and kept up by the presence of irritating matter.

There are two cases in which certain affections are complicated with an acute gastritis; and as these complications are not sufficiently known, and have been scarcely noticed by systematic writers on gastritis, I am anxious to draw your particular attention to them. One of these is *hæmatemesis*, the other that disease which has been termed *delirium tremens*. There are cases of vomiting of blood, which are little more than acute gastritis, in which there is a copious secretion of blood from the mucous surface of the stomach. Vomiting of blood may arise from various causes. It may be vicarious, as in the case of females, where the menstrual flux is suppressed; it may be accidental, as from the rupture of a blood-vessel; or it may be caused by mechanical obstruction to the circulation, either in the liver, spleen, heart, or lungs. But there is a species of gastritis, in which there is a copious vomiting of blood; or there is an *hæmatemesis*, of which the cause is gastric irritation. How are you to recognise this form of the disease?—The patient is vomiting blood; but then he has fever, hot skin, and excited pulse. Again, you will see some peculiar modification of the tongue; you will find ardent thirst and longing for cold drinks; you will observe fulness and tenderness of the epigastrium; you may have severe local pain; finally, you will have all these symptoms occurring in a person who, previously to the attack, exhibited nothing capable of accounting for the *hæmatemesis*. Here, then, we have an hæmorrhagic gastritis, very little known, and too often improperly treated. The ordinary practice, in such cases, is to give astringents. Astringents are very good and useful where they are clearly indicated; but there are many forms of disease where their routine employment is productive of a great deal of mischief; and I believe lives are sometimes lost by looking upon this affection as a simple *hæmatemesis*, and by practitioners contenting themselves with the use of astringents. But where you have the symptoms of this form of gastric irritation present, where, in addition to the vomiting of blood, you have fever, and thirst, and hot skin, and pain, and epigastric tenderness, you may be sure that it is a gastritis, and that the best treatment is leeches, iced water, and the other means recommended in the treatment of gastric inflammation. It may happen that, under this treatment, the vomiting of blood will not entirely subside; but the pain, the thirst, the fever, and epigastric tenderness will subside, and then you can with propriety give astringents. The best thing you can do in the commencement is to leech freely, give iced lemonade, and cold water; prohibit every thing purgative, stimulant, or astringent; and then, when you have reduced inflammation, if the *hæmatemesis* continues, have recourse to astringents.

A few words now with respect to the other complication—*delirium tremens*. You have all seen cases of *delirium tremens*, but you are not, perhaps, aware that it arises under two opposite classes of causes. In some cases, a patient who is in the habit of taking

wine or spirituous liquor every day in considerable quantities, meets with an accident or gets an attack of fever. He is confined to bed, put on an antiphlogistic diet, and in place of wine or whiskey punch gets whey and barley-water. An attack of delirium tremens comes on, and symptoms of high cerebral excitement appear. Another person, not in the habit of frequent intoxication, takes to what is called a fit of drinking, and is attacked with delirium tremens. In the first case the delirium arises from a want of the customary stimulus, in the second from excess. In each the cause of the disease is different; and, consequently, with this view of the subject, it would be a manifest departure from sound practice to treat both cases in the same way. Yet, I believe, this error is frequently committed, even by persons whose authority is high in the medical world, and is part of a system not yet exploded—*the system of prescribing for names and not for things*. The patient is treated for a disease which has been called delirium tremens, the present symptoms are only attended to, and the cause and origin of the affection are overlooked. What are the true principles of treatment?—In the first variety, where the delirium is produced by a want of the customary stimulus, there is no doubt that patients have been cured by the administration of the usual stimulants, by giving them wine, brandy, and opium. Indeed, this seems to be the best mode of treating this form of the disease. But is it proper or admissible in the second variety, where the delirium is caused by an occasional excess in the use of ardent spirits?—Certainly not. Yet what do we find to be the ordinary practice in hospitals when a patient is admitted under such circumstances?—A man, who has been attacked by delirium tremens after a violent debauch, is ordered a quantity of porter, wine, brandy, and opium; and the worse he gets, the more is the quantity of stimulants increased. Now this practice seems to me as ridiculous as the old principle of treating a case of hydrophobia with a hair of the dog that bit. Let us consider what the state of the case is:—A large quantity of stimulant liquors have been taken into the stomach, the mucous surface of that organ is in a state of intense irritation, the brain and nervous system are in a highly excited condition from the absorption of alcohol, or in consequence of the excessive sympathetic stimulation to which they have been subjected. Are we to continue this stimulation?—I think not. What would be the obvious and natural result?—Increased gastric irritation, encephalitis, or inflammation of the membranes of the brain. The super-vention of inflammatory disease of the brain in delirium tremens is not understood by many practitioners, and they go on administering stimulant after stimulant, totally unconscious that they are bringing on decided cerebral disease. I have witnessed the dissections of a great many persons who died of delirium tremens, and one of the most common results of the dissection was, the discovery of unequivocal marks of inflammation in the brain and stomach. Broussais considers all such cases as merely examples of gastritis, and ridicules British practitioners for inventing a “new disease;”

but in this he is certainly wrong, for there have been several cases in which no distinct marks of gastric inflammation could be discovered. In all cases, however, where the delirium supervenes on an excessive debauch, there is more or less of gastritis; and though it may occasionally happen, that a patient under such circumstances may recover under the stimulant treatment, yet I am convinced that the physician will very frequently do harm by adopting it.

This complication of delirium tremens with gastritis is also exceedingly curious in another point of view, as it illustrates how completely the local symptoms are placed in abeyance, and, as it were, lost during the prevalence of strong sympathetic irritation. The patient's belly will not be tender; the tongue may not be red; the symptoms present may be indicative of a mere cerebral affection, and yet intense gastric inflammation may be going on all the time, and all the appearance of cerebral disease be quickly removed by treatment calculated to subdue a gastritis. Is this all theory? No; for we have practised on this principle with the most extraordinary success in the Meath Hospital. We have seen cases of violent outrageous delirium subside under the application of leeches to the epigastrium, and iced water, without a single drop of laudanum. I beg of you, if you meet with any cases of delirium tremens under such circumstances, to make trial of this mode of treatment, and record its effects, for it is important that they should be more extensively known. I have seen the whole train of morbid phenomena, the delirium, the sleeplessness, the excessive nervous agitation, all vanish under the application of leeches to the epigastrium. In some cases where after the sleeplessness and delirium were removed by this practice, and the tremours alone remained, we have again applied leeches to the epigastrium, and succeeded in removing the tremours also. On the other hand, where a stimulant plan of treatment was employed, and the patients died, we have most commonly found inflammation in two places, in the stomach, or in the brain or its membranes. The rule, then, is this—in a case of delirium tremens from the want of a customary stimulus, use the stimulant and opiate treatment; but when it comes on after an occasional violent debauch, such remedies must be extremely improper. Adopt here every thing calculated to remove gastric irritation. We have facts to show that most decided advantage may arise, from the application of leeches, even where the symptoms of gastritis are absent.

We come now to consider chronic gastritis, an extremely interesting disease, whether we look upon it with reference to its importance, its frequency, or its Protean character. It is commonly called dyspepsia, and this term, loose and unlimited in its acceptance, often proves a stumbling block to the student in medicine. Dyspepsia, you know, means difficult digestion, a circumstance which may depend on many causes, but perhaps on none more frequently than upon chronic gastritis. In the great majority of dyspeptic cases, the exciting cause has been over stimulation of the

stomach, either from the constant excess in strong highly seasoned meats, or indulging in the use of exciting liquors. Persons who feed grossly and drink deeply, are generally the subjects of dyspepsia; by constantly stimulating the stomach they produce an inflammatory condition of that organ. Long continued functional lesion will eventually produce more or less organic disease; and you will find that in most cases of old dyspepsia there is more or less gastritis. But let us go farther, and enquire whether those views are borne out by the ordinary treatment of dyspeptic cases. When you open a book on the practice of physic, and turn to the article dyspepsia, one of the first things which strikes you is the vast number of cures for indigestion. The more incurable a disease is, and the less we know of its treatment, the more numerous is the list of remedies, and the more empirical is its treatment. Now the circumstance of having a great variety of "cures" for a disease, is a strong proof, either that there is no real remedy for it, or that its nature is very little understood. A patient afflicted with dyspepsia will generally run through a variety of treatment, he will be ordered bark by one practitioner, mercury by another, purgatives by a third; in fact, he will be subjected to every form of treatment. Now all this is proof positive that the disease is not sufficiently understood. What does pathology teach in such cases? In almost every instance where patients have died with symptoms of dyspepsia, pathological anatomy proves the stomach to be in a state of demonstrable disease. It appears, therefore, that, whether we look to the uncertainty and vacillations of treatment, or the results of anatomical examination, the case is still the same; and that, where dyspepsia has been of considerable duration, the chance is that there is more or less of organic disease, and that, if we prescribe for dyspepsia neglecting this, we are very likely to do mischief. I do not wish you to believe that every case of dyspepsia is a case of gastritis. This opinion has brought disgrace on the school of Broussais. His disciples went too far; for whether the gastric derangement depended on nervous irritation, or anæmia, or disease of the liver, or mental emotion, they prescribed leeches and water diet, and thus very often brought on the disease they sought to cure. We may have functional disease, independent of structural lesion, in the stomach, as well as in any other organ; it is no unusual circumstance, and the practical physician meets with it every day. A great deal of confusion, however, arises from the similarity of the symptoms. I remember an accomplished friend of mine getting into disgrace with one of the members of a board of examiners on this subject. He was asked to tell the difference between the symptoms of chronic gastritis and dyspepsia, and in reply stated that he could not. For this he was nearly rejected; but, I believe, on a candid review of the circumstances, you will agree with me, that he knew more of the matter than the learned professor. In ninety-nine cases out of a hundred of chronic gastritis there is no fever, scarcely any thirst, often no fixed local pain, and this leads persons away from any idea of the existence of an inflammatory

condition of the stomach. What are the symptoms of a chronic gastritis? pain of occasional occurrence, flatulence, acidity, swelling of the stomach, feid eructations, sensation of heat and weight about the epigastrium, and perhaps vomiting. Well, these are also the symptoms of dyspepsia, whether it be accompanied by inflammation or not. How then, when called to a case of this kind, are you to determine the point? I must mention to you here, that it is often hard to do this with certainty. There are two circumstances, however, which you should always bear in mind, as they will afford you considerable assistance in coming to a correct diagnosis; *first, the length of time which the disease has lasted*; secondly, the result of the treatment which has been employed. You will find, that where the disease is a chronic gastritis, that it has been of some duration, that it has come on in an insidious manner, and that it has been exasperated by the ordinary treatment for dyspepsia. Many persons think, that if you give a patient medicine, without regulating his diet or issuing a prohibition against full meals, that you can cure him, and that, as he has no fever, and can go about his usual business, there is no necessity for antiphlogistic regimen. But as the disease goes on, he complains of pain in the stomach during the process of digestion, feels uneasy after dinner, there is an unpleasant degree of fulness about the epigastrium, he also experiences a variety of disagreeable symptoms, sometimes being annoyed with pain in the chest, sometimes he says he feels it in the region of the heart, and sometimes about the cartilages of the eighth and ninth ribs. These symptoms subside after the process of digestion is completed, but during its continuance they harass the patient. Very often relief is obtained by vomiting, and hence some persons are in the habit of throwing up their food for the purpose of relieving themselves, and consequently can have no benefit by it. In some cases digestion goes on until the food seems to reach a particular point, and then an acute feeling of pain is experienced. In these cases the gastritis is generally circumscribed, and is likely to terminate in circumscribed ulceration. Various fluids are rejected from the stomach during the course of a gastritis; sometimes acid, sometimes alkaline, sometimes insipid and sweet, sometimes bitter and bilious. There is generally a degree of fulness about the stomach, and the epigastrium is tender on pressure, but no decided tumour, either of the pylorus, liver, or spleen, although the epigastrium presented that appearance of fulness and tension termed by the French "*renitence*." The bowels, too, are constipated, and this is a matter worthy of your attention, for it sometimes unfortunately happens that the practitioner, mistaking the gastritis for simple constipation, goes on prescribing purgative after purgative, until the patient gets incurable disease of the stomach. I know a case of a lady who gets one stool a week by taking eight drops of croton oil. Some years ago she was in the enjoyment of excellent health; her bowels happened to get confined, and she was treated by a systematic practitioner with continued purgatives; her bowels are now completely torpid, except when they are

subjected to this unnatural stimulus. There are thousands of persons treated in this way, because practitioners look to consequences and not to causes.

There is one remarkable difference between acute and chronic gastritis, which deserves your attentive consideration, as it exemplifies a law applicable to all viscera under similar circumstances, and this is, that the sympathetic irritations are not so frequent or so distinct in chronic inflammation as in the acute form, and hence, in a case of chronic gastritis we almost never have fever, and the affections of the nervous respiratory or circulating systems are by no means so well marked. It may even go on to actual disorganisation of the stomach, and yet the patient will not complain of any particular symptom during its whole progress, which you could set down as depending exclusively on the sympathetic irritation of gastritis. Some of these cases, called dyspeptic phthisis, by Dr. W. Philip, are most probably examples of the sympathetic irritation of the lungs from chronic gastritis. Another case, respecting which much error prevails, is what has been called hypochondriasis. Persons labouring under these affections are condemned to run the gantlet of every mode of treatment; sometimes (and fortunately for themselves) they are sent to travel, sometimes they are treated with musk and antispasmodics, then with the mineral acids, then with purgatives and mercurials, and lastly with bark, nitrate of silver, and stimulants. They go about like spectres from one practitioner to another, trying remedy after remedy, alternately sanguine with hope or saddened by disappointment, until at last they die, and, to the astonishment of all the doctors, the only disease found, on dissection, is inflammation and thickening of the mucous surface of the stomach. A condition, which, under these circumstances, it was difficult to say whether it was the original disease, or produced by "*fair trials*" of a number of powerful agents. Hypochondriasis is not always gastritis; but it is now found that, in many cases, it commences and terminates with disease in the upper portion of the digestive tube and the assisting viscera. This you must always bear in mind.

Chronic gastritis terminates in various ways. Sometimes the inflammation is limited to a particular spot of the stomach, and here we frequently discover circumscribed ulcerations. In very bad cases these ulcers go on perforating the various coats of the stomach, until at last the contents of that organ escape into the serous cavity of the abdomen, and the patient rapidly sinks under a fatal peritonitis. It does not follow however, that, in all cases of perforation, the contents of the stomach get into the peritoneum, causing death. Very often adhesions are formed, and the base of the ulcer is the serous covering of some other portion of the digestive system, or a false passage may be formed into the colon. One of the most common terminations of a chronic gastritis is, that the inflammation extends to other viscera; the patient gets disease of the liver, spleen, peritoneum, or lungs, and sinks under a complication of disorders. It was somewhat in this way that Napoleon

died. He laboured for a considerable time under chronic disease of the stomach, which seems to have been overlooked by his medical attendants, and this terminated in the extension of disease to various other organs.

LECTURE VI.

Treatment of chronic gastritis—Frequent excitement of the vascular system necessary to the performance of the functions of the stomach—Local bleeding—Regimen—Counter-irritation over the stomach—Treatment of Broussais—Use of vegetable tonics—Oxide of bismuth—Acetate of morphia.

I shall begin to-day with the treatment of chronic gastritis, and I beg of you to bear in mind what I mentioned at my last lecture, that this disease, in its true and pathological meaning, is not sufficiently recognised. In general, it gets some wrong name or other; and as many practitioners are in the habit of prescribing for names, it generally meets with wrong treatment. It is called every thing but what it is, and its remedies are as numerous and as various as its appellations. By some, it is called dyspepsia, and is treated with bitters, astringents, and stimulants; by others, it is termed constipation, and treated with purgatives; the school of Abernethy look upon it as an affection of the liver, and prescribe blue pill and black draught; others give it the name of hypochondriasis, and exhaust the whole catalogue of nervous and anti-dyspeptic medicines in attempting its removal; in fact, it is called every thing but what it is, and the result is an unsteady and mischievous empiricism.

You will recollect a fact, to which I alluded in my last lecture, that the physiological condition of the stomach requires that it should be subject to frequent excitements of its vascular tissue, and that this increased vascularity being the consequence of a natural process, digestion is, generally speaking, exempt from any kind of danger. If the brain or lungs were to experience an equal increase of vascularity, sensibility, and excitement, the consequence would be dangerous, or perhaps fatal, and we should have pulmonary and cerebral diseases produced. But though the stomach enjoys such a remarkable exemption from the liability to acute inflammation, under circumstances of repeated vascular excitement, yet the slow, insidious, chronic gastritis, is an exceedingly common affection. I feel convinced that many persons die of it, or of the extensive class of fatal diseases which it frequently induces. But I rejoice to say, that we have good reason to hope that the progressive amelioration of medical science will materially diminish the amount of human suffering from this cause. As physiological medicine advances, the number of those who die of unrecognised chronic visceral disease will be less and less, because diagnosis

will become more extended and certain, and practice more simple and successful.

The first thing you should do, when called to treat a case of dyspepsia, is to ascertain whether it be a purely nervous disease, or a chronic gastritis. The majority of practitioners give themselves no trouble about this matter, not recognising the fact, that of the number of dyspeptic persons who seek for medical advice, a considerable proportion are really labouring under a chronic gastritis, and forgetting, that, in consequence of long-continued functional injury, what was at first but a mere nervous derangement may afterwards become complicated with organic disease. You must also bear in mind, that the stomach is perhaps placed under more unfavourable circumstances for bringing about a cure than any other organ, because the life of the individual demands that the stomach, though in a state of inflammation, should still continue to perform its functions. In treating diseases of other organs, you will have the advantage of a comparative state of rest; but, in a case of the stomach, if you wish to preserve life, you cannot prohibit nutriment, and, consequently, you must run the risk of keeping up these periodic vascularities which its condition requires, which, though harmless in health, become a source of evil when the stomach is diseased. The obvious deduction from this is, that the cure of chronic gastritis depends as much upon regimen as upon medical treatment, and particularly where the symptoms have arisen from long-continued excitement, as in the case of persons who live highly. Here the treatment chiefly depends on regulating the diet, and if your patient has sense enough to live sparingly for a few weeks or months, you may be able to effect a cure without other treatment. The great error is, that most practitioners attempt to cure the disease by specifics, and when these fail, they then go to the symptomatic treatment, prescribing sometimes for acidity, sometimes for nausea, sometimes for flatulence, sometimes for constipation, or "the liver," or debility.

You should be careful in the examination of such cases, and should try to ascertain whether these symptoms may not depend upon inflammation of the stomach; for as long as the patient is in this state, the less you have recourse to symptomatic or specific treatment the better. It is hard to mention one single medicine which, in this state, will not prove stimulant, and if the stomach be unfit for stimulants, it must be unfit for the generality of medicines. There are numbers of cases of persons labouring under chronic gastritis, which have been cured by strict regulation of diet, and by avoiding every article of food requiring strong digestive powers. We find that articles of diet vary very much in this respect; some are digested with ease, some with pain. We might express this otherwise, by saying, that some require very little excitement of the stomach, and others very great vascular excitement. Patients, in this irritable state of stomach, can scarcely bear any kind of ingesta; and when you consider the great vascularity, thickening of the mucous membrane, and tendency to

organic disease, you will be induced to think that every thing entering the stomach should be of the mildest kind, and not requiring any powerful determination of blood to that organ.

If you continually prescribe for symptoms, neglecting or overlooking the real nature of the disease, giving arsenic to excite the system, and iron to remove anæmia, and bitter tonics to improve the appetite, and alkaline remedies for acidity, and carminatives to expel flatus, you will do no good; you may chance to give relief to-day, and find your patient worse to-morrow; and at last he will die, and you may be disgraced. On opening the stomach, after death, you are astonished to find extensive ulceration, or, perhaps, cancerous disease. Very often, in such cases, practitioners say that it is cancerous disease, and that no good can be done. But the thing is to be able to know, when you are called to a case, whether it is a case of mere nervous dyspepsia, or chronic inflammation of the stomach. Some of the best pathologists think that most of the cancerous affections of the stomach are, in the beginning, only chronic inflammations of that organ.

I believe we have not yet in this country adopted the plan of moderate application of leeches to the epigastrium in cases of chronic gastritis. I have seen, in many cases, great benefit result from the repeated application of a small number of leeches to the epigastrium, at intervals of two or three days. Here is a point which you will find very useful in practice. You will meet with cases which have lasted for a long time; cases where there is strong evidence of organic disease, and which have resisted the ordinary dyspeptic treatment. You will be called frequently to treat these three different cases:—where the disease has been of long duration; where there is distinct evidence of organic disease; and where the disease has resisted the ordinary dyspeptic treatment. Here is a case of a patient labouring under what is called indigestion, and which has resisted the stimulant, and tonic, and purgative treatment. Here is one fact. In the next place, the disease is chronic, and the probability is that there is inflammation, and consequently that there is chronic gastritis. Now if, in such a case, you omit all medicine by the mouth, apply leeches to the epigastrium, keep the bowels open by injections, and regulate the diet, you will often do a vast deal of good. I have seen, under this treatment, the tongue clean, the pain and tenderness of the epigastrium subside, the acidity, thirst, nausea, and flatulence, removed, the power of digestion restored, and all the symptoms for which alkalies, and acids, and tonics, and purgatives, were prescribed, vanish under treatment calculated to remove chronic inflammation of the stomach.

What is next in importance to regulated regimen and local bleeding? A careful attention to the bowels, which, in chronic gastritis, are generally constipated, and this has a tendency to keep up disease in the upper part of the digestive tube. Is this to be obviated by introducing purgative medicine into the stomach? No. If you introduce strong purgative medicine by the mouth,

you will do a great deal of mischief. You must open the bowels by enemata, or, if you give medicine by the mouth, by the mildest laxatives in a state of great dilution. A little castor-oil, given every third or fourth day, or a little rhubarb, with some of the neutral salts, will answer in most cases. The diet, too, can be managed, so as to have a gently laxative effect. The use of injections is, however, what I principally rely on. I have seen many cases of gastritis cured by the total omission of all medicine by the mouth, by giving up every article of food which disagreed with the stomach, and by the use of warm water enemata. I have seen this treatment relieve and cure persons whose sufferings had lasted for years previous to its employment, and who had been considered by many practitioners to labour under organic disease of an incurable nature. It is important that you should bear this in mind. The old purgative and mercurial treatment of gastritis, I am happy to say, is rapidly declining; and British practitioners are now convinced that they cannot cure every form of dyspepsia by the old mode of treatment. I do not deny that many diseases of the digestive tube may be benefited by the mild use of mercury and laxatives, but I think I have every reasonable and scientific practitioner with me in condemning the unscientific routine practice, which was followed by those who took the writings of Abernethy and Hamilton for their guide. I do not say that, where cases of gastric inflammation, treated after the plan of Mr. Abernethy, have proved fatal, the medicines have destroyed life; I merely assert that the patients died of inflammation, over which these medicines had no control; and the error lay in mistaking and overlooking the actual disease, as much as in its maltreatment. You will find some practitioners (they are becoming fewer in number every day), who seem to have but two ideas, the one a purgative, the other a pot full of fæces; but the connecting link—the gastro-enteric mucous membrane—that vast expansion, so complicated, so delicate, so important, seems to be totally forgotten. But practitioners are now beginning to see that purgatives are not to be employed empirically; that they should be administered in many cases with great caution, and with a due attention to the actual condition of the alimentary canal, and that they have been a source of great abuse in the medical practice of these countries.

Next to leeching, and a proper regulation of the bowels, is the employment of gentle and long-continued counter-irritation over the stomach. This may be effected by the repeated application of small blisters, or by the use of tartar emetic ointment. I have been in the habit of impressing upon the class, that the tartar emetic ointment used in these countries is too strong, the consequence of which is an eruption of large pustules, which are excessively painful, and often accompanied with such disturbance of the constitution as amounts to symptomatic fever. In fact, tartar emetic ointment of the ordinary strength produces so much irritation, that few patients will submit to it long. The form which I recommend you to employ is the following:—Take seven drachms

of prepared lard, and, instead of a drachm of tartar emetic, which is the usual quantity, take half a drachm, directing, in your prescription (this is a point of importance), that it be reduced to an impalpable powder; and you may add to it what will increase its action, one drachm of mercurial ointment. This produces a crop of small pustules, which give but little pain and are easily borne; and the counter-irritation may be kept up in this way for a considerable time, by stopping, for a few days, until the eruption fades away, and then renewing the friction. I have often seen the utility of this remedy exemplified in cases of chronic gastritis, where the symptoms of gastric irritation, which had subsided under the employment of friction with tartar emetic ointment, returned when it was left off, and again vanished when it was resumed. The case of the celebrated anatomist, Beclard, furnishes a very remarkable proof of the value of a well-regulated diet and repeated counter-irritation in the treatment of this disease. While he was engaged in the ardent prosecution of his professional studies he got an affection of the stomach, which he considered to be a chronic gastritis, and immediately put himself under a strict regimen, using, at the same time, repeated counter-irritation. He kept up the counter-irritant plan for a considerable length of time, for he found that, when he discontinued it, the gastric symptoms had a tendency to return. In this way he got completely rid of the disease. Several years afterwards he died of an attack of erysipelas; and, on opening his stomach, the cicatrix of an old ulcer was discovered in the vicinity of the pylorus, which was exactly the spot to which he had referred his pain during the continuance of his gastric affection.

There is, perhaps, no science in which the motto, "*medio tutissimus ibis*," is of more extensive application than in medicine. Some physicians on the continent, particularly the disciples of Broussais, having repeatedly witnessed the advantages of strict regimen and local depletion in chronic gastritis, have pushed this practice too far. They seemed to forget that the system requires support and nutrition, which can be effected only through the agency of the stomach; they saw the evils which result from the use of stimulating food in cases of chronic gastritis; and, looking to these alone, they ran into the opposite extreme, the consequence of which was, that they kept their patients so long upon low diet that they actually produced the very symptoms which they wished to remove. The patients became dyspeptic from real debility of the stomach and the whole frame. You remember a general law of pathology to which I have alluded on a former occasion, and which I shall again mention, as it illustrates this point, namely, that opposite states of the economy may be accompanied by the same symptoms. Thus we observe, that palpitation may depend on two different causes—on a sthenic or asthenic condition—on the presence of too much or too little blood in the heart. Now, it frequently happened that patients, labouring under chronic gastritis, and who had been treated for a long time after the strict plan

adopted by the Broussaists, finding themselves not at all improved, went to other physicians who had different views, and were rapidly cured, by being put upon a full nutritious diet. In this way numerous cases, which water diet and depletion had only aggravated, were relieved, and the consequence was, that a mass of facts was brought forward and published, not long since, by a French author, against the antiphlogistic treatment of dyspepsia and chronic gastritis. It must be stated, however, that the cases which he published were chiefly those in which the depleting system had been carried to excess, and that they cannot, therefore, be received as proofs of the value of a stimulating diet in the treatment of chronic inflammation of the stomach. Bear this in mind; the sooner you can put your patient on a nutritious diet the better will it be for him. It would be absurd to keep a patient for many months, as the Broussaists have done, on slops and gum-water. It will be necessary for you to feel your way and improve the diet gradually. Commence by giving a small quantity of mild nutritious food; if your patient bears it well, you can go on; if the gastric symptoms return, you can easily stop. If a small portion of the milder species of food rests quietly on the stomach, you may increase it the next day, or the day after, and thus you proceed to more solid and nutritious aliment, until the tone of your patient's stomach regains the standard of health. Never lose sight of this fact, that you may have a case of dyspepsia depending on a chronic gastritis, in which, though you remove the *inflammation* by a strict antiphlogistic treatment, you may not by this remove the *dyspepsia*; and if you continue to leech, and blister, and starve your patient, *after the inflammatory state be removed*, you will do great injury. Such a patient, falling into the hands of another practitioner who treated him on a different system, might be relieved, and his case quoted against you and your treatment, though this, at the commencement, was judicious and proper.

With respect to internal remedies, the school of Broussais think that there is nothing required but cold water and gum. This is going too far. In a former lecture, I have drawn your attention to the fact, that in the treatment of acute inflammation there is a point where antiphlogistics should cease, and where tonics and stimulants are the most efficient means of cure. Of this fact, the disciples of Broussais appear to be ignorant, and they consequently declare against every remedy for chronic gastritis except leeches and cold water. Now is this right? I think not. We find that, in all cases of gastric inflammation, a change in medication seems to be useful at some period of the disease, that is, a change from antiphlogistics to tonics and stimulants, and I believe that in cases of chronic gastritis these remedies may be used with very great advantage, having, of course, premised depletion and counter-irritants. I believe, too, that most of the remedies, which we see every day unsuccessfully employed, would have acted beneficially, if the preparatory treatment, which I have mentioned, had been adopted. Among the best remedies of this kind is the oxide of

bismuth; I have seen more benefit from the use of this than of any other medicine, after the treatment already alluded to. Generally speaking, the list of internal remedies for chronic gastritis is very small, but after the use of antiphlogistics, you may prescribe the vegetable tonics and oxide of bismuth with advantage. The most decidedly valuable remedy, however, in the after stage of a chronic gastritis, is the acetate of morphia, which, I am convinced, has a very powerful effect in allaying chronic irritation of the stomach. Dr. Bardsley, of Manchester, in one of his published works, entitled "Hospital Facts and Observations," adduces many cases of gastric irritation which were completely relieved by the use of this remedy, and I am perfectly satisfied of the truth of his statements. It may be said that Dr. Bardsley's cases were only instances of dyspepsia. But as his cases were extremely numerous, some of them of long standing, and the symptoms very severe, the great probability is, that some of them at least must have been cases of chronic gastritis. I know very few books, the perusal of which I would more strongly recommend to you than Dr. Bardsley's accurate and instructive work. The great besetting sin of medical writers is, that their statements of successful practice are grounded on a very limited number of cases, or that, in publishing the result of their practical investigations, they only give their successful cases, and leave out those in which the treatment recommended has been found inefficacious. Yet this is a circumstance which should never be neglected. If a man declares that he has discovered a cure for gastritis, or dyspepsia, and brings forward one hundred cases in which the remedy has done good, the statement is still unsatisfactory and insufficient, because there may be one thousand cases in which it has totally failed. Unless he comes forward and gives both his successful and unsuccessful cases, of what value are his statements? Dr. Bardsley, with the candour and good sense which always characterise the philosophic enquirer, gives the result of *all* his cases, forms them into tables, and then leaves his readers to judge for themselves. From an inspection of these tables, you will be convinced of the efficacy of acetate of morphia in the treatment of chronic gastritis. I have been in the habit of using it with the most gratifying results after leeching, regulating the diet, and paying proper attention to the state of the bowels. There are some forms of the disease in which it is more useful than others. The particular form, in which it proves most serviceable, is where there is a copious secretion of acid from the stomach (that form in which all kinds of alkalies have been exhibited), where severe pain and constant acidity are the prominent symptoms. Here I have seen the acetate of morphia act exceedingly well. You may begin with one twelfth of a grain, made into a pill with crumb of bread, or conserve of roses, twice a day; the next day you may order it to be taken three times, and you may go on in this way until you make the patient take from half a grain to a grain and a half in the twenty-four hours. I shall here mention the circumstances of a case, which I do not mean to bring

forward as an instance of cure, but as an illustration of the extraordinary power which acetate of morphia possesses in relieving gastric irritation. A gentleman of strong mind and highly cultivated intellectual powers, which he kept in constant exercise, got a severe chronic gastritis; his appetite completely declined; he had frequent vomiting of sour matter; fetid eructations; and such violent pain in the stomach, that he used, when the attack came on, to throw himself on the ground, and roll about in a state of indescribable agony. He applied to various practitioners, had several consultations on his case, and the opinion of the most eminent medical men was, that he had incurable cancerous disease of the stomach. These symptoms continued for several years, but for the last two or three years they were quite intolerable. He had repeated cold sweats, vomited every thing he took, even cold water, was reduced to a skeleton, and led a life of complete torture. Under such circumstances he tried, for the first time, by my advice, the acetate of morphia. He tried it first in doses of one tenth of a grain three times a day, and experienced the most unexpected relief. On the third day all his bad symptoms were gone. He had no pain, no vomiting, no sweats; his spirits were raised to the highest state of exhilaration, and he thought himself perfectly cured. He went out in the greatest joy, visited all his friends, and told them that he had at last got rid of his tormenting malady. In the evening he joined a supper party, indulged very freely, and next morning had a violent hæmatemesis, to which he had been for some time subject. All his old symptoms again made their appearance. He again had recourse to the acetate of morphia, and again immediately experienced relief, but the vomiting of blood again returned, so that he discontinued the remedy. This gentleman is now in the enjoyment of good health. He regulated his diet, left off all medicine by the mouth, used warm water injections, and thus recovered from his supposed cancer.

I do not bring this case forward as an instance of the curative effect of acetate of morphia, but as an instance of its powerful effect in allaying gastric irritation. I could adduce other cases in proof of its value in the treatment of the after stage of chronic gastritis, and particularly of that form in which pain and acidity are the prominent symptoms; but I perceive my time has nearly expired. At my next lecture, I shall give some other particulars connected with this subject, and then proceed to the consideration of diseases of the small intestine.

LECTURE VII.

Friction with croton oil—Attention to diet during convalescence—Organic disease of the stomach—Principles of treatment—Diet and attention to the bowels—Duodenitis—Inflammation of the jejunum—Ileitis, complication and nature of—Dothinenenteritis—Ulceration of the mucous membrane—Symptoms and diagnosis of ileitis.

In speaking of the employment of counter-irritation in cases of chronic gastritis, I forgot to mention the use of friction with croton oil, which has been found beneficial in many cases of chronic inflammation. It has been extensively used by many practitioners in the treatment of chronic affections of the joints, and in various forms of pulmonary disease; and I have employed it myself in some cases of chronic gastritis with benefit. I cannot say that the cases in which I have used it presented all the symptoms of chronic gastritis, but they were certainly cases of chronic gastrodynia, with severe local pain, nausea, and loss of appetite. It is an excellent counter-irritant, and gives very little pain. The mode in which I employ it is this—take a few drops of croton oil, five or six, for instance, drop them on the epigastrium, and rub them in with a piece of lint or bladder, interposed between your finger and the skin, and the next day you have an eruption of small papulæ, which you can increase at will. There is one interesting circumstance connected with the use of croton oil frictions, which you should be made acquainted with. The liability to produce counter-irritation, seems to depend upon the absorption or non-absorption of the croton oil; if it be absorbed it will purge, but if it be not it will produce counter-irritation. In cases of this kind, therefore, where it produces the necessary degree of irritation in the skin, the chances are, that it will not act disagreeably by bringing on catharsis. I have only seen one case where there were both the eruption and catharsis. This was a gentleman who had lately suffered from dysentery in warm climates.¹

I may also mention, that, in convalescence from an attack of chronic gastritis, you must pay great attention to diet for a long time, because there is no affection of any organ in the body, in which an error in diet so rapidly induces a return of the original symptoms, as in diseases of the stomach, while each return of the disease renders the attack more dangerous and unmanageable, until at last disorganisation takes place.

This leads me to speak of organic disease of the stomach. On

¹ There is one variety of revulsion which is effectual in removing chronic gastritis, and, indeed, every form of dyspepsia, at times when every other remedy has failed. We allude to constant change of air, society, and scenery, with exercise in the open air, such as traveling affords. Even a single voyage, as from one side of the Atlantic to the other, is often sufficient to break in upon the morbid catenation; and Dr. James Johnson remarks, emphatically, that he believes every form of dyspepsia, unaccompanied by organic mischief, would yield to the revulsion produced by traveling two or three thousand miles over such a country as Switzerland.—R. D.

this subject I shall be very brief; the best mode of communicating information will be to exhibit these preparations; you will derive more instruction from their inspection than from any lecture I could deliver. (Dr. Stokes here exhibited a number of beautiful preparations from the Park street museum, illustrative of various organic lesions of the stomach.) Here is a case, which some pathologists would call cancer, others chronic gastritis. I may remark here, that pathologists are divided as to what is the cause of cancer of the stomach, but the best informed are of opinion that, in those cases of gastric disorganisation which are called cancer or scirrhus, all that can be demonstrated by the knife is referable to the results of chronic inflammation. This is a different proposition from saying that chronic inflammation *alone* will produce cancer. As yet we know little of cancer; dissection of cancerous organs gives but scanty information; but this seems certain, that, in particular conditions of the economy, an inflammation of the stomach will end in cancerous disease. Here is an excellent preparation of the stomach of a person who died of cancer of that organ. For several years before his death he had a jaundiced look, an emaciated appearance, frequent vomiting, and severe pain towards the termination of the digestive process, a circumstance which denotes disease of the pylorus. He also had hæmatemesis. You see the inner surface in the vicinity of the pylorus presents ulcerations of the mucous membrane and thickening of the submucous cellular tissue. The pylorus itself does not appear to be at all contracted, but the parts around it are in a state of extraordinary disease. Look at the preparation again, and say what could bitters, or acids, or alkalies, or tonics, have effected in a case of such extensive disease. Here is a stomach in a state of long continued chronic inflammation, and exhibiting lesions, which some would designate as cancer of that organ. Now, though I do not know the treatment which this patient underwent, I would venture to say that he took plenty of the usual anti-dyspeptic medicines. Yet, in a vast number of cases, where enormous quantities of these remedies are taken daily, the stomach is in as bad a state as that preparation exhibits, and I feel the more strongly convinced of this, because I am aware that many persons die after having gone through the whole routine of anti-dyspeptic practice, and, when they are opened after death, incurable disease of the stomach is discovered. Here is an example of vast cancerous disease of the stomach; here is a very interesting specimen of chronic gastritis, chiefly representing a most remarkable and circumscribed ulcer at the termination of the stomach. Here you see is the ulcer, with raised, thickened, and introverted edges. Now, in all probability, this ulceration was exceedingly chronic, for you perceive nature has been at work with it, and has made some attempts at reparation. It is in such a case as this that patients generally refer their pain to a particular part of the stomach: digestion goes on without any pain until the food reaches a certain point, when acute pain is felt, and this continues until it is relieved by vomiting. The

occurrence of this symptom, after an attack of acute gastritis, would lead you to suspect the formation of one or more ulcers, and the persistence of this localised pain should induce you to persevere in employing every means in your power calculated to remove the disease. The preparation which I now exhibit is interesting, as it shows the effect of corrosive poison on the stomach. The patient, to whom this stomach belonged, died in consequence of swallowing a quantity of sulphuric acid; here you see the consequences—the mucous membrane is black and disorganised, exhibiting this ragged appearance. In some cases of malignant fever we have found the stomach presenting somewhat similar appearances; and the same state of the stomach is described by some writers as occurring in cases of intertropical fever. Here is a preparation which you should inspect; chronic gastritis with a large ulcerated patch in the centre of the stomach. Here is another example of extensive cancerous disease.

A very few words will suffice for the state of the science on the subject of cancer of the stomach. It is very hard, nay, even almost impossible, to draw a line of distinction between the symptoms of cancer of the stomach and chronic gastritis, and I believe it is admitted on all hands that the same causes give rise to both. Long continued irritation will, in one case, produce cancer of the stomach, in another, chronic gastritis. Again, it is admitted by many, that what is called cancerous ulceration of the stomach has no appreciable difference from ulceration in various other organs; and hence some other persons have gone so far as to say that there is no such thing as cancer of the stomach (separately considered); and that all the cases adduced of it are nothing more than so many forms of chronic gastritis. In the present state of medicine, we are not, indeed, possessed of any data which would enable us to come to a final determination on this question. It is certainly impossible to determine this point; but if there be any thing peculiar in cancerous matter, similar to tubercular or melanotic matter, there is no reason why, under the influence of inflammation, it should not be developed in the stomach as well as in any other part of the body. But whatever views we entertain on this subject, we must confess that, in the majority of cases, there is a chronic gastritis, and that the principles of treatment which would alleviate the patient's sufferings and prolong life, *are those which are calculated to prevent the occurrence of gastric inflammation.* The more you approximate the treatment of cancer to that of chronic gastritis, the greater comfort will you afford your patient, and the more will you prolong his existence.

The most celebrated case on record of this affection is that of the Emperor Napoleon. He died with extensive ulceration of the stomach, which, of course, was called "*cancerous*," and there were also distinct traces of disease in the liver, the mucous coat of the intestines, and the lungs. His disease was believed by himself to have originated in the stomach, and to this opinion he adhered, notwithstanding the results of some solemn consultations, at one of

which his affection was declared to be an "*obstruction of the liver*," with a "*scorbutic dyscrasy*." At another it was pronounced to be a "*chronic hepatitis*," and a course of mercury recommended! When we reflect on this, and read in the account by Gaubert, (which you will see in the *Examen des Doctrines Medicales*;) the regimen which was used, and the list of stimulating medicaments employed, you will not wonder at the words of this great man, when he was pressed to take more drugs, to swallow the universal nostrum, mercury, to which he had the greatest aversion. "Your disgusting preparations are good for nothing. Medicine is a collection of blind prescriptions, which destroy the poor, sometimes succeed with the rich, but whose whole results are more injurious than useful to humanity." But he got mercury, notwithstanding, mercury for his "digestive organs;" to "excite the liver;" to "remove its obstruction," and mercury to create bile, and purgatives to remove it; and tonics, and antacids, and stimulants; and he died in torture, and his body was opened, and the stomach was found "*cancerous*."

I should not omit mentioning to you, that in those cases of chronic gastritis which run on to an incurable stage, the best treatment consists in a careful regulation of diet, in keeping the bowels open by enemata, or the very mildest laxatives, and in avoiding every thing capable of producing excitement. You will also derive advantage from the employment of gentle counter-irritation, and from the internal use of narcotics, which in such cases appear to have a more beneficial effect than any other class of remedies. With the exception of these, I do not know any other kind of medicine you can safely employ? and I believe that, in the majority of cases, you will find that the patients have taken already too much medicine. Anxious for relief, and urged on by the hope of obtaining some remedy capable of relieving their sufferings, they have recourse to every grade of quacks, are persuaded to swallow every kind of drug, and are subjected to every form of harassing and mischievous treatment. The diet which you prescribe for such patients should be sparing but nutritive; give the stomach as little to do as will be consistent with the support of life and strength; and you may take it as a general rule in the treatment of all chronic affections of the digestive tube, whether cancer of the stomach, scirrhus of the pylorus, or stricture of the intestines, that there are two great principles of general application—preserving a gently open state of the bowels, and allaying inflammatory excitement.

Let us now proceed to the remaining parts of the digestive tube, of which the next in order is the duodenum. I shall not dwell much to day on the subject of duodenitis; as I shall revert to its consideration when speaking of jaundice, because inflammation of the duodenum is a common cause of jaundice, perhaps the most common, if we take the whole of its cases together. You are not to suppose that I wish to inculcate the doctrine that jaundice is a necessary complication in duodenitis;

but it has been proved that there is an extraordinarily frequent coincidence between both, and that jaundice very often seems independent of any mechanical cause, such as an obstruction of the biliary ducts. So far from this, that, in some cases, particularly those which are produced by, or accompany, a duodenitis, we have intense universal jaundice at the same time that the bile is flowing freely into the digestive tube.

The researches of the immortal Bichat gave the first hint which directed the attention of practitioners to the circumstance, that, in many cases where jaundice had existed during life, there was no obstruction or disease in the liver or biliary ducts, but that in such cases there was always more or less inflammation in that part of the digestive tube into which the bile was immediately discharged, and this led ultimately to the discovery of the connection which exists between inflammation of the duodenum and jaundice. In treating of the sympathies which depend upon continuity of surface, Bichat refers to the connection which exists between the surfaces of mucous membranes and the ducts which open on them, and endeavours to show that the natural mode of excitement in all secreting glands is a stimulus applied to the surface on which their ducts open. As examples of this, he instances the effect which food and other substances, applied to the mucous membrane of the mouth, have in stimulating the salivary glands; the effect which stimulants, applied to the conjunctiva, or nose, have on the lachrymal gland, and many others. Hence Broussais concludes that, when the mucous surface of the duodenum is thrown into a state of excitement, we may have a consequent affection of the liver, for the duodenum bears the same relation to the liver as the mouth does to the parotid glands. That this is frequently the case, I think, is very probable. It is now established, that the cause of the yellowness in what has been called yellow fever, is disease of the upper part of the digestive tube, in which the duodenum is always involved; and that the fever itself (the typhus icterodes of the nosologists) has been found to be greatly connected with inflammation of the stomach and duodenum. During the epidemic of 1827, we had in the Meath Hospital a great many cases which bore a striking resemblance to the yellow fever of warm countries, and particularly in this, that they were accompanied by intense jaundice, and inflammation of the upper part of the digestive tube. You will see in the works of Rush and Lawrence, two of the best American writers on yellow fever, that, of the numerous bodies they examined, there were scarcely any in which the jaundice was found in connection with liver disease, but that in all cases there was intense inflammation of the digestive surface. I shall return to this subject when I come to speak of liver disease.

With respect to the jejunum, I may state that we know very little of the symptoms which characterise inflammation of this part of the intestinal canal; and it is a curious pathological fact, that this portion of the tube is, of all others, the least liable to inflammation.

In point of fact, we have no means of ascertaining what are the

prominent symptoms of inflammation of the jejunum, because, in almost every case in which jejunitis has been discovered, there has been also extensive disease of the rest of the small intestine. We have cases of simple gastritis; there have been also cases of distinct disease of the duodenum. We may have disease in the lower third of the ileum, unaccompanied by an affection of any other part of the tube. The same thing may occur in the case of the cæcum, colon, or rectum, but it seldom or never occurs so far as the jejunum is concerned. I shall therefore pass over jejunitis, and proceed to draw your attention to one of the most important diseases to which the human subject is liable—*inflammation of the ileum*.

Inflammation of the ileum is a most important affection, for two reasons; first, in consequence of its extraordinary frequency, and, in the next place, of its insidious latency, the disease generally requiring a considerable degree of tact and experience on the part of the practitioner to make out its diagnosis with certainty. In fever, it is the most frequent of all forms of intestinal inflammation; and hence Broussais, finding inflammation of the ileum of such constant occurrence in fever, concluded that fever was only symptomatic of intestinal inflammation. Further researches have shown that he was mistaken, and that the inflammation of the digestive tube is, in many cases, secondary; but it is still a circumstance of almost constant occurrence, and in many cases of fever is the cause of death. Now, the portions of the intestinal tube most commonly affected in fever are the stomach and lower part of the ileum; and the frequent occurrence of this in fever is very remarkable. There are few cases of typhus without it. In some cases of typhus you will, on examination after death, be astonished to find extensive disease of the intestinal canal, which, during life, had not attracted any particular notice, and this you will most commonly find in the lower part of the ileum. So common is it, that Louis says that ileitis is the grand anatomical feature of typhus fever; that is, had he been obliged to pitch on the lesion of some particular organ as giving a character to typhus, he would say that it was ileitis. There are other diseases, too, in which inflammation of the ileum forms the principal complication. In the diseases of children, which go by the names of worm fever, remittent fever, and bilious fever, I believe that ileitis is generally the first affection, and that the fevers are only symptomatic of it. It constantly occurs at some period or other of tabes mesenterica; and I believe that in many cases it precedes the affection of the mesenteric glands. It is exceedingly common in phthisis. In every case of phthisis, where diarrhœa has lasted for some time, the probability is, that there is ulceration in the cæcum, colon, and lower part of the ileum.

Now, what is the nature of this ileitis? This preparation, (*handing one for inspection*,) which I beg of you to hand round, will furnish a very good illustration of the disease. Here is a portion of the intestine exhibiting various distinct ulcerations of different

sizes, occupying the situation of the mucous glands. I do not mean to say that the character of the disease consists in this distinct ulceration; it is an essential disease of the mucous membrane, and of its glands, which exist in great numbers on the surface of the lower third of the ileum, and are called *solitary* and *aggregate*. These glands frequently take on the inflammatory condition, become softened, run into ulceration, and produce extraordinary sympathetic irritation of the whole system. There has been lately a great deal of discussion with respect to the question—Whether disease begins in the glands or in the mucous membrane, and whether we can separate disease of the glands from disease of the mucous membrane. This has been carried to a great extent; and a change has been attempted to be made in the name of the disease, it being entitled *dothineritis* by those who say that the inflammation commences in the glands. But this I think is a mere refinement, and is carrying the thing too far. It is next to impossible for the glands to be affected without involving the mucous membrane, or for the mucous membrane to be affected without an extension of the disease to the glands. We sometimes, however, see the mucous membrane diseased without the glands being apparently engaged; but I think the glands are never engaged without the co-existence of disease in the mucous membrane. In this preparation you see the mucous membrane is just giving way; and here is an actual slough, where the mucous and submucous tunics have yielded to the inflammation. In the lower portion of the ileum we meet with an infinite variety in the size and number of the ulcerations: in some they are very close and numerous, in others there are only two or three detached ones; in some, the whole circle of the intestine is destroyed; and the ulcer is nearly as broad as the palm of your hand. It is interesting to consider, with respect to the pathology of the respiratory and digestive systems, how it comes that ulceration of the mucous membrane is so much more common in the digestive apparatus than in the respiratory. For one ulceration of the bronchial mucous membrane from acute disease, you will have one hundred of the gastrointestinal. For this peculiarity we cannot clearly account; but there seems to be more development in the digestive than in the respiratory system, and that this over-development produces a tendency to disease. This, perhaps, is an approximation to an explanation of the facts; and to this may be added, that the mucous membrane of the intestines is exposed to the influence of a much greater variety of agents. It is difficult to give an accurate idea of the symptoms of ileitis, as we can only arrive at a knowledge of it by negative evidence, or, as the French term it, "*par voie d'exclusion*."

In a case of gastritis and of inflammation in the upper part of the digestive tube, the most prominent symptoms are thirst and vomiting. In this affection, too, there is thirst, but it is by no means so urgent as in the former cases, and there is generally no vomiting. In a case of acute gastritis there is always a desire for

cold drinks. In this disease there is also a desire for fluids, but the patient prefers them warm. Here you perceive two symptoms connected with the predominance of disease in the upper part of the digestive tube are absent—vomiting and the desire for cold drinks.

Now, you are aware that, in a case of inflammation of the colon and rectum, the most prominent symptoms are diarrhœa, tenesmus, and the passing of a quantity of morbid secretions. These symptoms, in a case of ileitis, are either wanting, or they are so slight as to excite very little notice. If, then, in a case of intestinal disease, we abstract the characteristic symptoms of disease in the upper and lower part of the digestive tube from the phenomena of the existing disease; if we find that it presents symptoms which do not properly belong to either the stomach, duodenum, colon, or rectum; we conclude that it must depend on a lesion of the remaining part of the canal, and we are in this way led to the diagnosis of ileitis. Let us enumerate the symptoms of an ileitis. In the first place, thirst, without a preference for cold drinks; in the next, absence of vomiting; again, in the early period of the disease there is generally a tympanitic state of the belly, and the patient seldom complains of pain, even in fatal cases. This is a point of extreme importance. There is, however, most commonly a degree of tenderness over the ileum, which you will be able to detect by an accurate examination, and this tenderness presents a remarkable difference from the tenderness of gastritis, both in degree and situation. It is very seldom so exquisite as in a case of gastritis, the patient can bear a considerable degree of pressure, and the tenderness, in place of being towards the epigastrium, is situated between the umbilicus and the crest of the ileum on the right side; here pressure excites pain. The tongue in this affection is generally of a dirty white, pointed, and red along the edges and tip; the pulse is quick and small, and the face is contracted. As to the nature of the discharges from the bowels they are exceedingly various; there has been as yet no diagnosis founded on their appearance, and in some fatal cases they have been observed to retain an almost perfectly healthy appearance throughout. What would the gentlemen who draw their diagnosis from the chamber-pots say in such cases? I have seen perfectly natural stools in cases which immediately after have terminated fatally, and where, on examination after death, there was a vast extent of ulceration in the ileum. In addition to the symptoms just recited, the patient most commonly has *fever*, and this presents itself under various forms, frequently assuming the type of a simple continued fever; hence, in a great many cases, *the patient is merely supposed to labour under simple continued fever, and the existence of extensive inflammation of the ileum is entirely overlooked.* In other instances, there is more or less prostration, which increases with the progress of the disease, and the fever frequently receives the appellation of typhoid. Under these circumstances, the patient often gets bark and wine, every means is taken to support his

strength and remove the typhoid condition of the system, the inflammation of the intestine is exasperated by neglect and mal-treatment, the patient dies, and, on dissection, the ileum presents an enormous sheet of ulcerations.

In cases of this kind, where the diagnosis depends as much on negative as on positive circumstances, it is of importance to have a direct sign by which we may be able to ascertain, with some degree of certainty, the existence of a suspected enteric inflammation, and I think I have discovered one, which I believe has not been as yet noticed; this is increased pulsation of the abdominal vessels. In many cases of acute inflammation of the brain, the increased pulsation of the carotids has been frequently remarked, and every one sees, that, under such circumstances, there is an undue excitement of these vessels, or, in other words, that there is a want of proportion between the action of the carotids and that of the arteries of the extremities. If your finger be attacked by paronychia the same phenomenon is observed, the artery leading to the inflamed finger beats much stronger than the artery of the corresponding one on the opposite side. From these circumstances I was led to conclude, that, in cases of acute inflammation of the digestive tube, there would be increased pulsation of the abdominal aorta; and on following up the investigation by examining several persons who had distinct and well marked intestinal inflammation, I found that my conclusions were well grounded. In such cases, I found not only a remarkable throbbing of the abdominal aorta, but I also discovered that this throbbing was prolonged to the femoral arteries, and that, on the other hand, there was little or no corresponding excitement in the arteries of the upper extremities.

LECTURE VIII.

Diseases of the small intestines—Symptoms of ileitis—Occurrence of diarrhœa with fever symptomatic of this form of inflammation—Frequency and symptoms of the disease in children—*Tabes mesenterica*, treatment of.

At my last lecture I was engaged in the consideration of disease of the small intestines: let us now resume the subject. You remember I mentioned to you that most of our knowledge of the inflammatory affections of the small intestines refers to the ileum, and that, in point of fact, we know little or nothing of disease of the jejunum. This, however, is not of much importance, as, of all the parts of the digestive tube, the jejunum is the least liable to disease, and is seldom or never engaged without the co-existence of disease in the ileum or duodenum. You recollect I drew your attention strongly to the extreme frequency of inflammation in the lower third of the ileum, and the importance which it derives from

this as well as from its insidious latency. I showed that it was one of the most common secondary lesions in typhus fever, and a frequent cause of death. This cannot be impressed too much upon your minds—it is a point of pathology on which the best informed medical men are agreed. It may also, and very often does, occur as a pure idiopathic affection, without being preceded or superinduced by that morbid state of the whole economy to which we give the name of fever. I said it was extremely common in children; that here it was in many instances mistaken for worms, or bilious, or remittent fever; that it constantly occurred during the progress of *tabes mesenterica*, and often appeared to have the initiative. I alluded to the discussion which has arisen as to the question whether disease begins in the glands or mucous membrane, and stated that such discussions are useless, as it is impossible to separate the two affections in diagnosis or treatment, and practical medicine gains nothing by the distinction.

With respect to the symptoms of ileitis, I observed that they were those of a general affection of the digestive tube, the phenomena which indicate irritation at its upper and lower part being absent. That if you abstract from symptoms of a general affection of the intestinal canal, the vomiting and desire for cold drinks which characterise inflammation of the upper part, and the diarrhœa and tenesmus which denote disease of the lower part, you will have the diagnostic marks of an ileitis. At our last meeting I showed you some preparations illustrative of this disease; I intended to have exhibited others of the same kind to-day, but regret that I cannot lay my hands on them at present. Allow me to rehearse the symptoms of ileitis once more. Thirst, without desire for cold drinks; absence of vomiting, and of the characteristic symptoms of inflammation of the colon and rectum; early tympanitis, generally on the second day of the disease; absence of pain, but existence of tenderness on pressure between the umbilicus and the crest of the ileum; pointed tongue, of a dirty white on the upper surface, and red at the sides and tip; contracted features; quick, small pulse; fever, and, what I forgot to mention in my last lecture, scanty high-coloured urine, a very constant symptom, so much so that I have known this disease mistaken for an affection of the kidney, and the patient treated accordingly. I must add, that the patient died, that the kidney was found perfectly healthy, the ileum in a state of violent inflammation, and the suppression of urine to be referred to this cause alone.

I drew your attention at my last lecture to the increased pulsation of the abdominal aorta and its immediate branches, and stated that I looked upon this as a direct sign of abdominal inflammation. I do not mean to say that every case of increased action of the great abdominal arteries is significant of ileitis or intestinal inflammation. We see unusual pulsation of the abdominal aorta in hysterical females, and see it subside under the use of antispasmodics; we see it in painter's colic; we see it in cases of extreme emaciation; we see it in disease of the aorta, or of some of its first

large branches. What I wish to draw your attention to is this: where we have this symptom in addition to other signs of inflammation of the digestive tube, it is of considerable value as a diagnostic.

You may remember I stated that ileitis, from being generally attended by fever of the continued type, has been frequently supposed to be simple continued fever, and that this was one of the consequences which resulted from the latency of the disease. Petit was the first who described this disease rightly. He described it under the name of entero-mesenteric fever, that is to say, fever depending on disease of the mesenteric glands and small intestine. The following is an outline of his description: "The attack comes on with debility, irregular fever, quick, small pulse, sunken countenance, perhaps some diarrhœa, a lustrous expression of the eye." I may remark here that the occurrence of diarrhœa without any evident affection of the great intestine, and *accompanied by fever*, is almost always a sign of ileitis. It too often happens that practitioners, as I before remarked, prescribe for names. In cases of pulmonary disease, if the patient has fever, with copious expectoration, they say he is labouring under an attack of bronchitis; but in case of intestinal inflammation, accompanied by increased secretion, it is different; they merely say he has diarrhœa, and prescribe for it without connecting it with its proper cause. The general rule is, *that when you have diarrhœa with fever, there is inflammation of the digestive tube.*

In inflammation of the ileum the patient generally lies on his back, and avoids motion as much as he possibly can, his skin is dry and harsh; he is feverish; he has thirst, but little desire for cold drinks; he scarcely ever vomits; his alvine dejections are sometimes thin and purgative, sometimes figured and natural. But there is one circumstance which is of considerable importance in pointing out the amount of disease, even in cases where patients have considerable diarrhœa, and this is, that the diarrhœa is not sufficient to account for the extraordinary prostration. There must be some cause for the great reduction of vital power besides the mere diarrhœa, and I must state to you that there are few diseases which bring on such rapid prostration as inflammation of this portion of the digestive tube. In the advanced stage of this disease the patients have cold skin, subsultus tendinum, petechiæ, involuntary discharge of urine and fæces, low delirium, coma, gangrenous ulcerations of the back, sinking of the powers of life, effusions into the head and chest, in fact, all the symptoms which characterise the last stage of typhus. Generally speaking, the disease is more or less prolonged, and the patients die of exhaustion, but in some cases the approach of death is more sudden and formidable. Some of the ulcers pass deeply into the substance of the intestine, perforate all its coats in succession, the contents of the intestine escape into the peritoneum, and the patient is carried off by a rapid peritonitis.

Inflammation of the ileum is very frequently met with in chil-

dren, and it is most important that you should be aware of the extreme frequency, as well as the symptoms, of this disease, in those little creatures. There is one fact in pathology which seems not to be generally acted on—that there is a class of diseases which are intra-uterine, and with which a child may be born. There are a great many cases of this kind on record, but still, I must confess, there is a great scope for investigation, and that our knowledge on this subject is imperfect. I believe that any one who has the opportunity of dissecting a great number of still-born children, or of those who die immediately after birth, would, by examining the state of the different cavities, and publishing the results of his examinations, earn for himself very great reputation. It is a well known fact that children may be born with hydrocephalus, with tubercles in the lungs, with acute inflammation of the stomach; nay, more, children have been known to be born with chronic gastritis, and with old ulcerations in the ileum and colon. When children happen to be born with gastro-enteric disease, they are puny and weak; the fact of this occurrence is generally overlooked, the case is considered to be one of general debility, and hence most of those children are lost in consequence of their medical attendants being ignorant of the real nature of the disease. It is a very curious fact, too, that where enteric disease occurs in very young children, it is frequently met with without any accompanying fever, and this is a point of great importance. Here is a fact not generally known. A new-born infant has vomiting, swelled belly, contracted features, but at the same time he has cold skin and feeble pulse; he has no distinct symptoms of fever, and a puny and feeble state of constitution appears to be the prominent symptom. He dies, and on opening the body you find distinct traces of enteric inflammation. The younger the child is, the less will be the chance of fever occurring as a sign of enteric inflammation. It seldom happens that this takes place after dentition, but before it is very common.

Now, what are the circumstances which would enable us to recognise this disease in children who have passed the period of first dentition? If you find the child vomiting, thirsty, with swelled belly, hot skin, a tendency to diarrhœa, and an erythematous redness about the anus, you may be sure that there is disease of the digestive system; if the child is restless, and you perceive that the symptoms of irritation of the head are coming on, you will be more certain, and in such cases pathology will inform you that the disease is chiefly in the ileum. In the advanced stage the diarrhœa is lessened, but the belly continues tympanitic, the child exhibits traces of long suffering, and the circumstance of the teeth not being developed gives it the appearance of premature old age, which cannot be mistaken by an experienced eye, and is a sign of long continued and extensive intestinal disease. In some cases, the child gets a common attack of diarrhœa; this is neglected, but after going on for two or three days, symptoms of fever begin to appear. Here we arrive at a practical rule. Where a child has

diarrhœa, and, after labouring under this for a few days, gets an attack of fever, you may be almost sure that it is a case of enteritis, and that you will be acting wisely in treating it as such. In the opinion of many well-informed practitioners, that form of fever which has been called infantile remittent, is only an example of this disease. In proof of this fact, Dr. Marsh, my friend and predecessor in this school, in his paper on jaundice, makes some excellent remarks on this subject. "There is yet one form of disease of very frequent occurrence, the seat of which is in the stomach and small intestines. That to which I allude, is the *infantile remittent fever*, or, as it is vulgarly termed, the *worm fever* of children. Its characteristic symptoms, if closely analysed, will be found all of them to point to the mucous surface as the original seat of morbid action."—*Dublin Hospital Reports*, vol. iii.

It would be well for medicine, if the valuable information conveyed in Dr. Marsh's paper was more universally diffused. I feel convinced that many children fall victims to malpractice under circumstances of this kind. A child gets symptoms of diarrhœa, has irregular or bad appetite, and swelled belly. The disease is called worm fever; he gets a dose of calomel and jalap, and, perhaps, passes some worms; for, when we come to speak of worms, we shall find that disease of the mucous surfaces is intimately connected with worms, and, in the opinion of one practitioner, worms may be the result of enteric inflammation. Well, some worms are passed; the purgative is again used; the child may not pass any more, or he may pass one or two in a week to encourage the practice. But all the symptoms of intestinal inflammation, the diarrhœa, the tympanitis, the thirst, the fever, are supposed to depend upon the presence of more worms, and these are to be evacuated by purgative medicine; and thus the affair goes on, until the child falls into tabes mesenterica, or gets sympathetic inflammation of the brain, and dies of hydrocephalus. I regret to add, that in many cases of this kind the head alone is opened; a little fluid is discovered in the ventricles of the brain, the doctor's diagnosis of the head is found to be correct, and all parties are satisfied. In cases of this kind, the early application of leeches to the belly, the regulation of diet, keeping the bowels gently open by enemata and mild counter-irritation, would have saved the patient. This is not mere theory; it is but a statement of facts, supported by the experience of practical men.

I wish to say a few words here with respect to tabes mesenterica. In a course of lectures like the present, it would be impossible to examine, in detail, the different forms of this disease; it will be as much as I can do to draw your attention to the general principles of its pathology and treatment. The term, tabes mesenterica, is employed to designate that species of consumption which depends upon disease of the mesenteric glands. The common idea formerly entertained with respect to this affection, and, I believe, still to a great extent, is, that the disease first commences in the mucous glands, and from these extends to the lymphatic ganglia of the

mesentery, which, in their turn, become enlarged, thickened, and less pervious, so that a sufficient share of nutriment cannot be absorbed, the consequence of which is, that the patient dies of atrophy and exhaustion. With such views of the case, the principles of treatment consisted in employing a class of medicines called deobstruent, the operation of which was supposed to be efficacious in removing this obstruction, this deposition in the substance of the mesenteric glands, and the enlargement by which it was accompanied. This was, and this, I am sorry to say, is the idea still entertained by many. What is the actual state of the science with respect to this disease? It is found that the glands are certainly changed in their structure, and that they are manifestly enlarged; but this is only a link in the chain of phenomena, for it has been proved that in *the majority of cases the disease is ushered in by enteritis, and that the swelling of the glands is the result of disease, propagated along the course of the lymphatics from the mucous surface of the intestines to the mesenteric ganglia.* This preparation, which I shall send round, will give you an idea of the actual state of the disease. Here is one of the glands which has been cut through; it exhibits the cheesy texture commonly observed in this disease, but you can perceive there are a number of lines running towards each of the glands; these are the engorged lymphatics, which, you see, correspond with ulcers on the mucous surface of the small intestine. That this is the true pathology of the disease will appear from the following circumstances:—First, it has been proved that the glands of the mesentery commonly become inflamed, enlarge, and suppurate, in cases of inflammation of the mucous membrane of the intestinal canal in the adult. A patient gets enteric inflammation and dies; on dissection, we find distinct marks of disease in the intestines, and, in addition to this, we find the glands evidently diseased. Here is one fact. In the next place, it has been proved that, in a great many cases of *tabes mesenterica*, if you retrace the history of the disease, if you go back to its first and earliest phenomena, you will find that it began with the symptoms of what has been termed remittent fever, or that the patient had enteritis or diarrhœa, which afterwards became chronic, and that then the symptoms of *tabes mesenterica* began to appear. In the third place, you will find that, in a vast number of cases, where a fatal termination has occurred, if you pursue your dissection, and slit up the whole of the ileum, you will discover numerous old ulcerations of the mucous membrane, and find that the lymphatics which correspond with these ulcerations are in a state of manifest disease. Lastly, it has been observed that the best treatment for *tabes mesenterica* is that which is calculated to remove enteric inflammation, and that the old treatment, founded on the principle of removing obstruction, by the use of alkalies, absorbents, and solvents, is erroneous and false in the majority of cases. So that we have proof of the origin of this disease in intestinal inflammation, drawn from the occurrence of analogous affections in the adult, from the phenomena of the disease

in its early stage, from morbid anatomy, and from treatment. I think there can be no doubt that, in most instances, it commences by intestinal inflammation. Of course a predisposition to disease of the glandular system will favour the occurrence. But is there no case in which the disease has commenced in the glands, and where the mucous membrane of the digestive tube is secondarily engaged? My answer to this question is, in a few cases we cannot prove that the disease commenced in the mucous membrane, and there is no reason why the glands of the mesenterica should not be liable to primary tuberculous or scrofulous deposition as well as those of any other part of the body; but, in a vast number of instances, the enlargement of the mesenteric glands is secondary, and resembles the inflammation of the inguinal glands which results from chancre on the penis. I would advise you to consult the Commentaries on Pathological Propositions by Broussais. On this subject, also, Dr. Mackintosh's Practice of Physic.

There is one thing more connected with this disease, which is of considerable importance, and to which I shall briefly draw your attention, and this is, that this inflammation of the glands of Peyer and Brunner, this *dothinerteritis*, as it has been called, is a very common cause of slow convalescence in fever. You will meet with cases of fever, which will go on to the 17th or 21st day, and then something like a crisis takes place; you expect that from this time forward the patient will get progressively better; but in the course of a few days you will be surprised to find no amendment, and that he is not gaining strength; you feel his pulse, and find it quick and small, his attendant informs you that he is restless at night, and when you ask him how he feels, he says he has no particular complaint, but that he is very weak, gets no sleep at night, and has no appetite. Under these circumstances you are anxious to find out what his disease is; you enquire into the state of the heart, lungs, and brain; you find no evidence of disease in any of these organs; you run over in your mind the symptoms present, the feverishness, quick pulse, want of appetite, restlessness, and finding some degree of abdominal tenderness and tympanitic swelling, you arrive at the conclusion that the return of health and strength is impeded and delayed by the existence of a *dothinerteritis*. The first person who discovered this fact was Dr. Cheyne. "In these cases," says he, "the distress of the patient often bore no proportion to the danger he was in; the former was very little, while the latter was extreme. The disease would proceed without violent symptoms; nay, a patient would seem to be recovering, although without any critical discharge; he would call for full or middle diet, and for days take his food regularly. The only circumstance in his situation which demanded attention was, that he regained neither flesh nor strength, and he expressed no desire to leave his bed. Then, his pulse again became quick and his tongue dry; and he would complain of dull pain and uneasiness in his belly, attended with soreness on pressure, and a degree of fulness in the upper part of the abdomen. Then came

on a loose state of the bowels, and great weakness. Probably at the next visit the patient was lying on his back, with a pale sunken countenance, and a very quick pulse; his mind without energy. Then his stools (mucous) passed from him in bed, and the urine also. Perhaps a hiccup came on; next his breathing became frequent, in which case death was at no great distance." In all these cases the mucous membrane and glands were found in a state of decided disease.

Now, what was the nature of this disease? It came on as a secondary affection during the course of fever, became more marked and intense, and finally destroyed the patient. I have seen very many cases of this disease. I give you this as a general rule:—when, after the apparent termination of a fever, your patient convalesces very slowly and imperfectly; when you find that he is becoming weak, that his pulse is quick, his belly tympanitic, his thirst still present, *and all this without evidence of disease in the respiratory, circulating, or nervous system*, you may suspect inflammation of the mucous glands of the digestive tube, which may terminate in deep ulcerations; and you will not be surprised if your patient should be carried off by rapid peritonitis, occasioned by an ulceration of all the coats of the intestine. I have witnessed many instances of the truth of this statement.

It has been objected to the doctrine, that infantile remittent fever and tabes mesenterica depend on inflammation of the mucous membrane of the digestive tube, because it has been found that purgatives are sometimes useful in the treatment of the disease; and those who bring forward this objection ask, "if purgatives give relief, how can it be intestinal inflammation?" Now, what are the real facts of the case? These cases, which have been relieved by purgatives, are cases in which purgative medicine has been given in the early stage, and has been productive of benefit; or, in other words, where the disease is only just commencing, and where its cause is proved to be the presence of irritating matter in the bowels. A physician is called to a case of this kind; he gives a purgative; a quantity of offending matter is evacuated, and the child gets better. You should act in the very same way, and have recourse to purgatives whenever you have reason to suspect the existence of irritating or indigestible matter in the bowels. You are to employ purgatives on the same principle as every one employs emetics in cases where corrosive poison has been swallowed; but no one is inclined to think that he will be able to cure the disease by the continued use of emetics. But, unfortunately, persons do not attend to the actual state of the digestive tube; they go on prescribing purgative after purgative, until the irritation, which was originally produced only by indigestible matter, becomes exacerbated, and terminates in ulceration of the intestinal mucous surface, accompanied by all the symptoms of tabes mesenterica.

The treatment of this affection is both simple and easy, particularly when the patient applies to you at an early period. In the case of children, one of the first things you have to determine is,

whether you shall have recourse to the employment of purgatives or not. If you happen to be called in at an early period, or if the patient has taken no purgatives, and there is reason to suspect a loaded state of the bowels, you will be right in employing some mild laxative. You cannot commence your treatment better than by prescribing some mild opening medicine, particularly when you discover that the patient has been taking indigestible improper food. This plan I think both reasonable and useful. You will frequently meet with cases in which all the bad symptoms will disappear after the use of a few laxatives. Here is a point on which the followers of Broussais erred. They declared that the exhibition of a single laxative would be to endanger the patient's life; and that the only treatment which could be relied upon consisted in the use of leeches, low diet, and cold water. But I think there is as much reason in giving a laxative to remove indigestible matter from the bowels in a case of this kind, as there would be in giving an emetic in a case of gastritis produced by the presence of indigestible matter or corrosive poison in the stomach. But if, after having evacuated the bowels, the symptoms of intestinal irritation should continue, you are not to persist in the use of purgatives; change your hand, and attack the symptoms of intestinal inflammation, which have now decidedly commenced.

We shall occupy ourselves, gentlemen, at our next lecture, in considering the treatment of this disease in the adult as well as children, and then go on to the disease of the large intestines.

LECTURE IX.

Treatment of ileitis—Advantage of leeching—Stimuli sometimes beneficial—Infantile remittent fever—Inflammation of the mucous membrane—Enteritis with diarrhoea—Effects of opium in inflammation of serous and mucous membranes—Pathology and treatment of diarrhoea and dysentery—Perforation of the intestine—Diseases of the large intestine.

We shall be occupied to-day in considering the treatment of inflammation of the mucous membrane of the small intestine. You may recollect that in my last lecture I spoke of the employment of laxatives in this disease, and mentioned that we are to employ laxatives in enteritis, on the same principle as emetics are used in cases where corrosive poison has been taken into the stomach. We are not to expect to be able to cure the disease by the use of laxatives, nor are we to have recourse to them in every case; we employ these remedies where we have decided evidence of the existence of offending matter in the bowels. We may meet with a case in the early stage, under such circumstances that the removal of the irritating matter by judicious purgation may completely relieve the patient, and this, I believe, is the foundation on which the super-

struction of the British purgative practice in ileitis and tabes mesenterica was raised. It was concluded that a laxative treatment, which had on many occasions succeeded in removing the first symptoms of the disease, would necessarily cure it in all stages and cases. This, I need not tell you, is wrong. Whenever you give purgatives or laxatives in enteritis, bear this in mind, that the effect which you have to produce is to be brought about at the least possible risk. If you can unload the bowels with a little castor oil or rhubarb, or some mild neutral salt, it is much better than to have recourse to calomel, or scammony, or colocynth. As a general rule, drastic purgatives must be avoided in inflammation of the mucous membrane of the intestines. The school of Broussais committed an error, on the one hand, by never admitting the use of laxatives, and British practitioners have been wrong, on the other hand, by giving too much purgative medicine. The error of the latter arose from looking always upon purgatives as antiphlogistics, which they are certainly, so far as they contribute to relieve inflammation by causing an increased secretion from the intestinal mucous surface. But this increase of secretion can be produced only by stimulating the organ to which they are applied; and hence, before they can become general antiphlogistics, they must of necessity be local stimulants. Further; if in a case of inflammation of the digestive tube you prescribe a purgative, and it fails in causing an increase of secretion, it will add considerably to the existing inflammation. It is, however, of very great importance that there should be no accumulation of offending matter in the bowels; and hence, when you find a degree of fulness in the belly, and the dejections scanty, you should always give a laxative, and follow it up by the administration of a narcotic. By using enemata, you can do a great deal of good, and this without any injury to the digestive tube; and I think they may be always employed with benefit in disease affecting the ileum. Recollect, gentlemen, what I wish to impress upon you respecting this part of the treatment is, that laxatives are to be employed in ileitis as one of the means of cure; but you are not to expect that a cure by the use of these alone will always be a matter of constant occurrence. It is true that many cases presenting symptoms of enteritis, have, in the beginning, yielded to laxatives; but it is true, also, that horrible mischief has been done by their continued or indiscriminate employment.

A few observations now with respect to bleeding. There is in simple inflammation of the mucous membrane of the intestines this peculiarity—it very seldom happens that it is necessary to use the lancet. The whole class of intestinal inflammations is so generally accompanied, even in the early period, with marked prostration and a typhoid condition of the whole system, that general bleeding is very seldom employed. But when the disease is recent, the constitution vigorous, the patient young, the skin intensely hot, and the pain violent, (a combination of circumstances which is not of very common occurrence,) you may employ the lancet with

safety and with great advantage to your patient. But what I wish to impress upon you is this—you must not expect to cut short an attack of enteric inflammation by general bleeding. Over inflammations of mucous membranes in general, but particularly of the intestinal mucous surface, the lancet has comparatively but little direct power; it is in the inflammatory affections of parenchymatous tissues and serous membranes, that we generally observe the most brilliant and decided effects of venesection. Neither can you, as in parenchymatous inflammation, bleed a second and a third time with benefit. In cases of inflammation affecting the mucous membrane of the intestinal canal, you are to look upon venesection as a preparatory step to leeching. Where the pain is violent, the fever high, the attack recent, and the constitution strong, you will do well to bleed; but only bleed once, and then apply leeches in abundance over the suffering organ. There is nothing of more importance, nothing of such decided value, as bleeding by leeches in inflammation of the mucous membrane of the intestinal canal, and here we arrive at a fact, the explanation of which is involved in much obscurity. A patient is attacked with inflammation of the mucous membrane, and glands of the digestive tube, twelve or twenty leeches are applied to the integuments of the abdomen, and their application is followed by extraordinary relief. This is a very curious fact when we consider that between the place where we apply the leeches, and the tissue which is affected, there intervene skin, cellular membrane, superficial fascia, cellular membrane again, deep-seated fascia, muscular substance, cellular membrane again, two layers of peritoneum, and muscular substance enveloped in cellular tissue. Yet, notwithstanding this extraordinary succession of tissues, it is an undeniable fact, that the application of a dozen leeches to the surface of the belly will frequently cut short an intestinal inflammation, or materially diminish its intensity. Here is a fact, the explanation of which is extremely difficult; and I tell you candidly, I cannot explain it. The school of Broussais attempt to explain it as follows. They state that it is a constant law of the economy, that there is a strong sympathy between the internal parts and their respective integuments, but they do not say why this sympathy should exist. We frequently, however, observe facts confirmatory of this law; you are aware that it often happens, that, in cases of the deep-seated muscular phlegmon mentioned by Mr. Crampton, in abscess of the liver, and in empyema, we have a swelling of the integuments, showing the existence of a sympathy between the integuments and the internal organs.

In treating a case of inflammation of the small intestine, I think you may generally commence with the application of twelve or eighteen leeches over the ileo-cæcal region. The ordinary result of this application is, that the pain and tympanitis are reduced, and the thirst diminished; but the patient still has fever, and you are to bear in mind that the mere subsidence of pain does not imply the removal of the disease. We may modify the character of an ileitis very considerably by a single application of leeches, but we are not

on that account to expect that we shall be able to remove the disease entirely. In general it is necessary to apply them two or three times, lessening the number at each succeeding application, and taking care that they are applied in the proper place, that is midway between the umbilicus and the crest of the ileum. Many practitioners are afraid of employing leeches in the advanced stage of this affection, in consequence of the great debility which characterises the advanced stage of this, as well as inflammation of every other part of the digestive tube. But though I am quite of opinion that the school of Broussais is wrong in using them at any period, still I think they may be employed even where the disease is advanced, *particularly if they have not been used before*, and I have frequently seen leeches applied with advantage as late as the twelfth day. I have employed them myself in the Meath Hospital as late as the ninth and tenth days with decided benefit. Many physicians on the continent are in the habit of treating inflammation of the digestive system by the application of leeches to the anus, and this is said to have a very good effect, and the number of leeches required is smaller. In disease of the great intestine accompanied by diarrhœa, tenesmus, and tormina, I think this is an excellent mode, but when the disease is in the upper part of the tube, I prefer applying them to the belly over the situation of the inflamed organ.

Now with respect to internal medicines. In this disease every thing that is administered should be given with the view of removing irritation, and for this purpose I know no better preparation than a combination of ipecacuanha and opium, as in Dover's powder. The exhibition of the compound powder of ipecacuanha is attended with decided advantage. You are all aware of the long established use of ipecacuanha and opium in diseases of the intestinal canal, and I think there can be no doubt that they possess considerable utility. With this I generally combine some mild mercurial; the best you can employ is the hydrarg. cum cretâ. Give two or three grains of each every second or third hour, as the case may be, and you may continue this for several days. Where there is no diarrhœa, and the bowels have a tendency to be constipated, it will be necessary to order, every second or third day, a mild laxative, a little manna, or rhubarb, or some castor oil; you should insist on the daily use of enemata, and if they answer the purpose sufficiently I would advise you to be sparing of the use of laxatives by the mouth. In addition to these remedies, I am in the habit of giving a considerable quantity of gum Arabic, which appears to have an extraordinary efficacy in disease of the small intestine. I look upon it as peculiarly valuable in the diseases of children. The ordinary mode of prescribing it is to give a certain quantity of gum water. If this is insufficient, you should order half an ounce or an ounce of the gum to be dissolved in a pint or quart of water, which the patient is to use during the day. After the use of the hydrarg. c. cretâ and Dover's powder, this has a decided value in the treatment of ileitis.

In this way by leeching, mild laxatives, prescribing mercury with chalk, and compound powder of ipecacuanha with gum water, your patient begins to improve. The tenderness of the epigastrium disappears, the tongue begins to clean, the fever diminishes, the thirst goes off, and appetite returns. This is the favourable termination. When the patient is of a weak and delicate habit, it is of great importance to pay particular attention to supporting the strength, *even from an early period of the disease*. In such a case, after the first week, the physician who neglects the proper means of supporting his patient's strength does wrong, and it has justly been remarked, that a practitioner will be right in supporting the general strength, at the same time that he is employing local antiphlogistics. It is in steering clear between these two opposite dangers that the judicious practitioner is seen; he does not allow his patient to die of inanition, while at the same time he takes care to remove local inflammation. I have seen several experienced physicians prescribe leeches to the abdomen on the same day that they ordered the patient to have chicken broth, and even a little wine. There is nothing improper in this; an inexperienced practitioner, who has his eye merely on the local inflammation, is apt to fall into the error of overlooking the constitutional debility, and allowing it to steal upon him. He finds very little difference between the appearance of his patient this day and the next, and thinks the slight increase of debility undeserving of any attention. At last his patient begins to sink visibly, he gets alarmed and has recourse to stimulants, but it is now too late. Besides, there are several articles of diet which support strength, without increasing inflammation; as, for instance, chicken broth, sago, arrow-root, strained rice, &c. These do no harm, and they prevent the patient from falling into a dangerous typhoid condition. Let us look at this in another point of view. Suppose you are called to a child who is said to have had an attack of worms, or bilious derangement, or that his bowels were costive, and purgatives were given, that the discharges were found to be bad, and more purgatives were administered; or suppose you are called to a child of a weak scrofulous habit, who had been taking large quantities of purgative medicine, for what has been termed *derangement of the bowels*, and you find the little sufferer with pale, shrunken face, a black circle round his eyes, cold extremities, rapid faltering pulse, great thirst, and evident symptoms of increased cerebral excitement; the little arms and hands are cold as death, but the belly burning, tympanitic, and very sensible to pressure, and when you compare the radial artery with the femoral, as it turns over the pubis, you will have some conception of the excited condition of the abdominal vessels; and in addition to this train of morbid phenomena, you find there is suppression of urine. Are you to attack these symptoms with antiphlogistic means? No; the first thing you are to do, is to prevent any further mischief, by totally inhibiting every kind of purgative medicine. You are next to consider carefully what the best line of treatment to be pursued is, for here you are under

circumstances of difficulty, and have a great many prejudices to contend with. What I find generally to be most successful is this. I begin by taking proper steps to support the strength, ordering the patient to take chicken broth, arrow-root, or jelly; the extremities are to be wrapped up in warm flannel; and if the patient is sinking, and has his mouth and teeth crusted with dark sordes, a little wine, watching its effects. If it produces sleep, if the pulse comes down under its use, and the fever is not increased, it will do a great deal of good, and you can gradually increase the quantity. Always bear in mind that there is a certain period in all inflammations, in which stimulants prove to be antiphlogistics, a circumstance which has been overlooked by the school of Broussais. So far with respect to constitutional treatment; but what will you do with local disease? The application of blisters is of decided use, nay, I have seen a few leeches very effective. Apply a blister to the abdomen, and dress it with mercurial ointment, at the same time you may employ frictions with mercurial ointment; you will also swathe the belly with flannel, so as to keep up a comfortable temperature. In this way you will be able to do a great deal of good. You will also prescribe hydrarg. c. cretâ, with Dover's powder; and if the bowels are confined, emollient injections. By steadily pursuing this plan of treatment, you will often rescue from imminent danger a case which would prove fatal under the purgative plan, and you will add greatly to your own reputation.

There is one form of this disease in which diarrhœa is a prominent symptom, where the purging is from the very commencement. On this form I am anxious that you should have clear ideas. In cases of this kind there is a copious discharge of fluid matter from the bowels. In the majority of cases you may lay down this law, that where there is a decided irritation of any secreting organ, increased discharges from the surface of that organ give more or less relief. Suppose two cases of hepatitis; in the one we have no secretion of bile, in the other the secretion is copious; the latter is certainly most favourable. Again, suppose two cases of bronchitis; in one there is a copious expectoration, in the other it is extremely scanty; now every medical man knows that the former is more easily managed. The increased secretion of any organ in the early stage is to be looked upon as a relief to the inflammation. The practical inference to be deduced from this is, that we should be cautious in adopting any means of arresting this discharge, as it is one of the modes which nature employs in relieving the irritation of a suffering organ. Well, then, suppose you have a case of enteritis, and that on the first or second day diarrhœa sets in, what does the routine and systematic physician do? He gives chalk mixture and opium with tincture of kino and catechu, and what is the consequence? The belly becomes tympanitic; the pain is increased, and even peritonitis may supervene;—this is one result of the increase of inflammation; or the breathing becomes difficult, and the patient gets bronchitis or pneumonia. Diarrhœa occurring in the early period of this disease is not to be interfered

with, except when it gets to such a height as to threaten the patient's life; and where it increases his sufferings by the frequency of the discharges. In the first week or fortnight, when there are only three or four discharges, or even five in the twenty-four hours, I believe it is better not to interfere by prescribing direct astringents; *but in the advanced period, when the powers of life are low, or the discharges very copious*, then the physician comes to the assistance of nature with just reason, and in such cases you should always interfere. The best mode of managing diarrhœa of this kind is to employ small, frequently repeated doses of Dover's powder, with anodyne injections. And here I may mention briefly, to such of you as have not seen them used, the best way of employing them. As these injections are used on a different principle from the common, the latter being intended to empty the great intestine and be discharged, the former to be retained, we are constantly to make the basis of our anodyne injection in such a manner, that it will not prove stimulant from its bulk, or from any irritating substance it may contain. Mucilage of starch, new milk, or linseed decoction may be used as the basis, and the quantity taken for one injection should never exceed three ounces. To this, for an adult, you add from fifteen to thirty drops of tincture of opium, for it is a curious fact connected with this subject, that opium given by the rectum has frequently been observed to exercise a much more powerful effect on the system than when an equal or even smaller quantity has been taken by the mouth. The rule then is, that when you first make trial of the remedy in this manner, feel your way cautiously, and if you find that your patient bears ten or fifteen drops, you can increase the quantity on repeating the enema. An eminent practitioner of this city thinks the narcotic effect of opium by the rectum much better marked than by the mouth, and I believe this to be true in many instances. I believe the administration of opium in this way requires a good deal of caution. I recollect the case of a man who had been for a considerable length of time in the habit of using laudanum in large quantities, and was, in fact, a regular opium eater. During an attack of illness he got an injection containing sixty drops of laudanum; this produced, in a very short time, symptoms of decided narcotism, from which the patient never recovered; in fact, he died with every appearance of being poisoned by opium. There is another fact with respect to this disease which I would have you to bear in mind, that, under certain circumstances, inflammation of the small intestine will produce a remarkable tolerance of opium. This applies not only to the advanced stage of enteritis, but also to many other forms of disease. Some time since I made a series of clinical experiments with the view of ascertaining the power which opium possesses in relieving inflammation, and the result has been, that in many cases where the powers of life are so low that we cannot have recourse to the lancet, or any kind of depletory measures, opium alone furnishes us with a powerful means of subduing inflammatory action. When we come to treat of peritonitis, I shall

have occasion to speak of the good effects of very large doses of opium, particularly in that form of disease which results from intestinal perforation. My first trials of this remedy were in affections of serous membranes, and to this I was led by some interesting clinical experiments made by Dr. Graves. I next tried it in diseases of mucous membranes, where antiphlogistics were inadmissible, and here, as in the former cases, I had many proofs of its great efficacy. I shall state the particulars of a very remarkable case. A young gentleman, a pupil of mine, and a member of the class at Park street, of an irritable habit, was attacked with intense inflammation of the mucous membrane of the intestines. He had a high degree of fever, and his thirst was so insatiable that for two days he never ceased calling for drink. His pulse was weak but rapid; his tongue red and pointed; respiration very much hurried; but the stethoscopic signs of disease of the lung were absent. His belly was exceedingly tender on pressure; and he had another remarkable symptom—constant smacking of the lips. The case, as you may perceive, was one of severe gastro-enteritis, and it was treated in the ordinary mode, by leeches, cold water, &c., but the disease showed great obstinacy, and at the end of a month the patient was evidently in a state of imminent danger. At this period a curious revulsion took place: the chest became engaged, and the patient got bronchitis. For this he was blistered, and took the decoct. polygalæ with large doses of carbonate of ammonia, under the use of which he recovered. The bronchitis disappeared, but was almost immediately replaced by symptoms of intense gastro-enteric inflammation, thirst, quick pulse, tympanitis, low delirium, and subsultus tendinum. In the course of two or three days diarrhœa come on, becoming more profuse as it advanced. The first day he had four discharges, the next eight, and thus it went on increasing until there was a constant discharge of thin fluid matter from the anus. The patient was quite run down, and on three different occasions his friends thought him dead. Having made an unsuccessful trial of various stimulants and astringents, I determined to try what might be expected from large doses of opium. The patient was dying, and it was necessary to do something instantly which would be likely to arrest the diarrhœa. I ordered a grain of opium to be given every hour; on the first day he took twelve grains with apparent benefit, the next day he took six, the same quantity on the third day, and on the fourth the diarrhœa had so much diminished, and the young gentleman was so much better, that I thought it might be safely omitted. From this period my patient recovered rapidly. I would not bring forward this case in proof of the efficacy of opium if there were not many others of a similar kind; and I have no doubt that this was a cure effected by the use of opium in large doses. In the treatment of this disease by opium, there is one simple rule, by observing which you will be able to avoid all difficulties, and at the same time have a criterion to judge of the value of the opiate treatment. If the remedy produces the ordinary narcotic effects of such large doses on the

system, *it will not do much good*. You begin, therefore, cautiously; and if, after the first or second dose, you find that decided narcotism is produced, or at least more than you would think the quantity given could have brought on, give it up—it will be dangerous. But if he bears one, two, or three grains, or if, after having taken six or eight grains in the twenty-four hours, he appears to be improving, you may then persevere in the administration of opium, and it will be attended with decided advantage.

We have next to proceed to the consideration of the pathology and treatment of diarrhœa and dysentery; I shall, however, first exhibit a few preparations illustrative of the diseases of the small intestine. Here is a preparation of the affection called *tabes mesenterica*. You see here various masses of those cheesy glands which are generally supposed to be the result of original scrofulous deposition; but if you look along the folds of the intestine, you will see a vast number of engorged lymphatics running up directly to those glands, and you will perceive that these lymphatics correspond at their commencement with ulcerative disease of the intestinal mucous surface and glands. Here is an interesting preparation, exhibiting three distinct ulcers. In one of these you see the bright vascularity and turgescence of the areola, and the ulcerative process which has just begun in the centre. Close to this is another large ulcer, which has destroyed the texture of the gut down to its serous covering, through which you perceive the light is shining. The last is an example of perforating ulcer; all the coats of the intestine have been destroyed, and on turning the preparation you see evident marks of peritoneal inflammation. This preparation also exhibits one of the modes in which an ulcerative perforation of the intestine may terminate. Sometimes, at the very moment the ulcerative process has succeeded in destroying the last coat of the intestine, inflammation of the serous membrane in the immediate vicinity takes place, a quantity of lymph is poured out, and if the matter be not in great quantity, and the hole not too large, the opening is closed up by the effused lymph, and a stop is put to further mischief. Again, by the effusion of lymph the ulcerated portion of the intestine may form an adhesion to another sound portion, the effused lymph does not permit the passage of the contents of the intestine into the peritoneum, but does not prevent them from getting into the sound portion by a continuance of the ulcerative process, and in this way we have another termination, in the formation of a false passage. Here is a good example of disease of the cœcum, here is an example of disease of the colon, and here is another with a vast number of ulcerations. Here is an interesting specimen of disease of the large intestine. The patient to whom it belonged died of phthisis;—look at it and you will see what extensive ravages have been made by the ulcerative process.

We come now to take up the subject of disease of the large intestine, which, as I find my time nearly past, I must reserve until our next meeting. I shall then speak of dysentery and diarrhœa,

and shall draw your attention to some new and curious facts respecting the discharge of fatty matter from the bowels. In the last number of the Medico-Chirurgical Transactions, three separate papers have appeared on this subject from Dr. Elliotson, Dr. Bright, and Mr. Lloyd. Dr. Bright has brought forward several interesting facts tending to show that discharges of fatty matter may be found to be indicative of certain forms of disease of the digestive tube and the neighbouring glands.

LECTURE X.

Diseases of the large intestines—Treatment of diarrhœa—Apyrexial period of diarrhœa—Danger in suddenly arresting the discharge—Purging in phthisis—Dysentery—Epidemic dysentery.

To-day we proceed to the consideration of the nature and treatment of some of the diseases of the large intestine. You will see, in the various systematic treatises on the practice of physic, separate descriptions of the affections of this portion of the digestive tube, you will find diarrhœa in one chapter and dysentery in another, and you will observe, that a great deal of ingenuity has been expended in forming nosological differences between these affections. I fear that much of what has been written respecting them is rather calculated to puzzle and mislead than to inform the student. Viewed anatomically, there is no essential difference. You may for every practical purpose place them in the same class, and consider them as the result of the same morbid condition of the same part, namely, an inflammation of the lower portion of the digestive tube. Some persons may quarrel with the term inflammation—call it, then, irritation, if you please; but the truth is, that it is a disease of the lower portion of the intestine, the results of which are increased sensibility and altered secretion; and this description, I think, will fairly apply to one as well as the other. If a man has purging, with fever and pain, it is called dysentery; if he has purging, without pain, and without any manifest febrile excitement, we call it diarrhœa. But, in cases where persons have died, after having laboured under diarrhœa for a length of time, we *generally* find, on dissection, lesions of the mucous membrane of the intestinal canal, sufficient to account for death. There are some cases, indeed, in which the mucous surface takes on a gleet discharge, similar to that which follows gonorrhœa, and under such circumstances you will not be able to discover any distinct anatomical evidences of disease. These, however, are comparatively rare, and bear little or no proportion to those cases which present distinct traces of organic lesion.

On the subjects of diarrhœa and dysentery I shall be very brief, as our time is short, and every thing relating to the pathology and

treatment of these affections may be expressed in very few words. First, then, as to diarrhœa, which is the frequent passing of stools of a more or less watery consistence, and which may, and generally does, occur without fever. This affection may be considered to arise under three different circumstances; but, in point of fact, every form of the disease may be referred to a single cause, as there is no essential difference in the actual nature of the circumstances by which they are produced. A patient, for instance, takes a quantity of indigestible food, this produces irritation in the gastro-intestinal mucous surface, and diarrhœa is the consequence. Another is exposed to cold, or gets wet feet, the mucous membrane of the bowels becomes more or less inflamed, and this terminates in diarrhœa. Again, a patient, labouring under hectic, has profuse perspirations, these go off and are replaced by frequent fluid discharges from the bowels—here, also, the result is called diarrhœa. All these forms are, however, referable to the same cause—irritation of the mucous lining of the digestive tube.

A man commits an excess at table, eats something that he cannot digest, and gets diarrhœa. If you happen to be called to such a case at an early period, your course is very plain and easy; there is every chance that the affected organ has received (as yet) no material injury, and is attempting to relieve itself by increased secretion. The indication here, is to get rid of the source of irritation as soon as possible, and this is best done by prescribing a laxative, to remove the offending matter, and then following it up with an opiate. The simple rule is to relieve the intestine, and prevent the liability to inflammation. A mild laxative, followed by opiates and demulcents, keeping the patient on a low regimen for a few days, and in a warm temperature; this is sufficient for the management of the first form of diarrhœa. In point of fact, the principal thing which the practitioner has to do, is to watch his patient, and take care not to permit the inflammatory action to become developed. It is in such cases as these that the expectant medicine is of value. What you are to direct your attention to, is the state of the intestinal surface. If a patient gets an attack of pain, if his belly becomes tender on pressure, if he is more or less feverish, you may be sure there has been some mischief done. If, on the contrary, the diarrhœa yields to the exhibition of a mild laxative and light diet; if the pulse be soft, and the belly not tender, you have no reason to fear. But if the purging becomes more distressing, if the pain is severe, the abdominal tenderness evident, the thirst and restlessness continue unabated, it is a sign that the irritation has produced something more than mere increased secretion, and that actual disease of the mucous tissue is setting in. We have now a true inflammatory diarrhœa, which may be looked upon altogether as an enteritis of that kind in which there is a copious secretion from the surface of the intestine. You observe this leads us at once to the principles of treatment. Here we have fever, pain, frequent morbid stools, thirst, and abdominal tenderness. Well, then, what are you to do? In a case where these

symptoms are so severe as to excite alarm, at once begin with applying leeches. Where there is merely evidence of intestinal irritation, caused by indigestible food, give a laxative, and follow it up with an opiate; where, in addition to the ordinary symptoms, you have fever, pain, and tenderness, never omit the application of leeches. Many a time have I seen cases of this kind, in which chalk mixture and astringents not only failed but even caused additional suffering, speedily and completely relieved by the application of a few leeches. In using leeches, too, we are not like the practitioners who trust to astringents, playing at the game of double or quits; nor do we stop the purging by exchanging it for something else equally bad, or even worse, for a peritonitis or a bronchitis, for instance; *by removing its cause we not only check the diarrhœa, but we obviate any tendency to a metastasis of inflammation to other tissues, and our mode of cure has at once the merit of being successful and safe.*

A patient who has had an attack of diarrhœa should have his belly swathed with flannel; this should never be neglected. He will also experience a great deal of benefit from the use of the hip bath and occasional opiates. Give, also, a combination of rhubarb and Dover's powders, and you will find that it will do him a great deal of good. This is the remedy which Rhæderen and Wægler found to be of extraordinary advantage in the mucous fever, with diarrhœa, which ravaged parts of Germany in the last century. Give two or three grains of each every second or third hour, and increase or diminish each of the ingredients according to circumstances, increasing the Dover's powder where the indication is to remove pain and irritation, and increasing the rhubarb where you wish to produce a laxative effect. This combination forms a remedy of decided value in enteric inflammations; it has been much used in such cases by Dr. Cheyne, and I have repeatedly employed it in the Meath Hospital with marked advantage. You are also to bear in mind, that though the principle of treatment in this disease is to remove its cause and put a stop to the purging, still you are in no case authorised to give it a sudden check, by astringents, in the early period. I gave the reasons for this at my last lecture, and showed that it was based upon a general law of the economy. If an organ in a state of inflammation pours out an increased quantity of secretion, *it is the mode in which nature attempts to give relief; and if you suddenly arrest this secretion, the probability is, that you will excite more inflammation in that organ, or cause a metastasis to other parts.* This is particularly the case if inflammatory fever exists. You must also attend to your patient's diet. Your object here is to support him on such a diet as will require but little digestive power, and will not produce large collections of fœcal matter in the bowels. Jellies, arrow-root, chicken-broth, and mild farinaceous food, are the only things that can be used with safety, until the intestinal irritation has subsided.

By pursuing this plan of treatment with steadiness and decision, you generally succeed in cutting short the disease. In some cases,

the diarrhœa will run on to the chronic stage, just like the gleet which follows gonorrhœa; and this is to be looked upon as the apyrexial period, in which antiphlogistic remedies are no longer admissible, and where you may employ stimulants and astringents with effect. The best way to manage this form of the disease, is to make your patient use warm clothing, an even temperature, and mild nutritious diet; to prescribe the vegetable and astringent tonics, the hip bath, and the occasional use of mild laxatives, followed by an opiate. In this way, after some time, the disease generally goes off, and the patient recovers his strength. But it may happen that this gleety discharge will continue unabated; it is running the patient down, and he wants some decided remedy to check it. Now, the remedies which appear to have the greatest power in stopping this discharge, are the metallic astringents, and the turpentine and balsams, combined with some of the preparations of opium. It is a curious and interesting matter to consider how these remedies act. They are a class of medicines which exercise an extraordinary influence over discharges from mucous surfaces, in a way we do not understand, but the effect is to arrest these discharges. In a case of ophthalmia, accompanied by copious secretion from the conjunctiva, or in a case of chronic gonorrhœa, we know there is nothing more beneficial than metallic astringents and balsams; and we are also aware of the great value which turpentine and balsam copaiva possess in checking the increased expectoration of a chronic bronchitis. In diarrhœa, also, they have the same power; they check inordinate secretion, and remove the morbid condition of the mucous membrane on which it depends, by some effect produced on the surface of that membrane, but in what manner this is accomplished we know not. In severe cases of this gleety discharge, one of the most certain remedies we can employ is acetate of lead. You will seldom have occasion to use this, or any of the other remedies alluded to, in the case of a healthy person, because the disease will seldom pass into this second or gleety stage; but if it should, and that it is running down the patient, it behoves you to check it as soon as possible, consistent with safety. Give, then, the acetate of lead in free and repeated doses, and it is singular to remark what quantities of it patients under such circumstances will bear without any bad consequence ensuing. Hitherto, many persons have been afraid to employ it in large quantities, from fear of producing painters' colic; but at present it is known that this disease is to be attributed to the absorption of the carbonate of lead in almost every instance, and that the acetate is comparatively harmless. On this point I can mention one interesting fact, namely, that I have been in the habit of using it constantly, and in considerable doses, for the last six years, and I cannot bring to my recollection one single instance of colic produced by it. One patient, in particular, who was under my care, took it in very considerable doses for six weeks, without any apparent injury. The only cases in which I have seen the acetate of lead act as a poison, were those in *which it had been*

used as an external application. Whether it be that this remedy is more pernicious when employed after the endermic mode, or whether, when applied to the skin, it attracts carbonic acid from the air, and is converted into a carbonate, I do not know, but of this I am certain, that where bad effects have followed the employment of the acetate of lead, they have been brought on it by its external use. I generally use this remedy in the form of pill, prescribing two grains of the acetate of lead and a quarter of a grain of opium, three times a day. With the same intention you may employ the turpentine and balsams, which have a powerful effect in checking mucous discharges. Dr. Pemberton, in his work on Abdominal Diseases, speaks very highly of the efficacy of balsam copaiva; and I have seen many cases where turpentine has had a great efficacy in arresting chronic diarrhœa. You will see, in the works on materia medica, some other remedies which you can employ with benefit in such cases, but I may mention one which is not generally known—the alkali of the nux vomica. Strychnine was first used in checking mucous discharges by a German physician, and afterwards by Dr. Graves in this city. The cases in which it proves most successful, are those in which there is a mere gleet discharge, a copious secretion from the mucous surface, without any inflammatory action whatever, or if there be, where it is so low as not to produce the least feverish excitement or pain. Cases of this kind, in which strychnine has been eminently successful, have been published by Dr. Graves.¹ Among others, is that of gentleman, who had sudden calls, so that he often had not time to reach the close-stool. He passed a quantity of thin jelly-like substance, and then experienced a transient relief until another attack came on. This case was cured by the use of strychnine, one twelfth of a grain, three times a day, made into pills with crumb of bread or aromatic confection.

I may mention here, that, in treating gleet diarrhœa in this way, one thing should be always borne in mind—it is always dangerous to check any copious secretion suddenly, and the danger consists in the liability to metastasis or new inflammation. Never forget this. What generally happens is, that the patient's belly begins to swell, and you have ascites rapidly formed. Now, I have never seen a case do well in which this kind of ascites came on after the sudden checking of a diarrhœa; the patients all died. Another consequence is the rapid supervention of pulmonic inflammation, and here the disease is almost as bad as in the bowels. You will ask how this unfavourable termination may be avoided. The best mode is, while you are arresting the discharge from the bowels, to promote a determination to the surface. While you are using opiates, and stimulants, and astringents, employ general warm bathing, or the hip bath, dress the patient in flannel,

¹ In similar affections it has been used successfully by Professor Gedding, of South Carolina. We have often given it, sometimes apparently with success, at others not.—R. D.

and use mild diaphoretics every night. You will also do right in blistering the belly occasionally. In this way you will succeed in curing the worst cases of this chronic flux, without exposing your patient to the risk of new inflammation, or translation of disease to other organs.

One of the most common forms of diarrhœa is the purging which occurs in cases of phthisis; a physician will be called to treat this as often as any other, and it is of importance that you should have correct ideas with respect to its pathology and treatment. The ordinary opinion is, that this kind of diarrhœa is one of the results of hectic fever, and many practitioners, in treating the purging of consumptive patients, overlook the actual condition of the intestine, and only take into consideration the state of the whole constitution, of the hectic state of which the diarrhœa is looked upon as one of the symptoms. The consequence of this is, that they do not proceed on the same principles in the treatment of this as of other similar affections of the intestinal canal. Now, I would impress upon you, that you should always consider the diarrhœa of phthisis as depending, in almost every instance, on enteric inflammation. There is no fact in medicine better established than this. Persons think it is the hectic which produces the purgation, but I believe the converse of this proposition is often much nearer the truth, and that the constant diarrhœa often produces and keeps up the hectic. If you examine the digestive tube of a patient who has died with symptoms of phthisical diarrhœa, you will commonly find extensive ulcerations in the colon, cœcum, and ileum. In some cases of consumption, where the purging has been very severe, the amount of disease will often be found to be quite extraordinary; I have often seen the whole of the lower part of the tube one sheet of extensive ulceration. I find I have not brought up any specimens of the effects of phthisical diarrhœa from the museum, but will exhibit them at our next meeting. The preparations before us are those which are illustrative of dysentery, but they will convey to you a good idea of the state of the great intestine in the diarrhœa of consumption, for the effects are nearly the same. Observe now, the importance of this fact, and recollect that in treating every case of consumption, with diarrhœa, you will have constantly to bear in mind this enteric complication. Recollect, also, that one of the best means of stopping it, when all other remedies have failed, is a blister applied over the abdomen. If the purging depended on hectic, this would not be the case. I could bring forward several cases in which every thing had been tried without success, when a blister was applied to the belly, and from the time it rose, the patients ceased to be troubled with diarrhœa, and continued so up to the period of death. I do not mean that you should in these cases proceed to attack the enteritis with the same vigour as you would a similar disease in the healthy subject. Generally speaking, I believe this form of enteritis to be incurable; but it is of importance that you should be aware of this enteric complication in phthisis, and when you are called in to treat such

a case, you should carefully avoid prescribing any thing calculated to add to the existing irritation.

Before I quit this subject, I wish to make one remark by the way of caution. It not unfrequently happens that a person, labouring under chronic diarrhœa, comes to consult a medical practitioner, and tells him that he has been suffering from this complaint for months, that he has eight or nine discharges by stool in the day, and that he has been under the care of five or six doctors in succession, without any benefit. Well, you are determined to have your trial, too, and you commence operations by putting him on full doses of acetate of lead. After a week or a fortnight, he comes back and tells you he is not a bit the better. You then try turpentine or balsam copaiva—no use. Nitrate of silver—the same result. The man get stired of you in turn, and perhaps goes to a surgeon to ask his advice. The surgeon examines the rectum carefully, and finds, at a short distance from the anus, an ulcer, which he immediately touches with a strong solution of the nitrate of silver. The ulcer begins to heal, the irritation of the gut ceases, and the diarrhœa goes off. The surgeon is extolled to the skies, and the doctors disgraced for ever in the opinion of the patient. Now this is not an uncommon case. I have seen several instances of it, and I must tell you I was once mistaken in this way myself. These ulcers are situated close to the verge of the anus; they occur chiefly in persons of broken-down constitution, and those who have taken a great deal of mercury. They produce irritation in the colon, tenesmus, griping, frequent discharges by stool, and, most commonly during the straining, a little blood is passed. During the course of last summer, I treated a soldier for this affection, who had been discharged from the East India Company's service (as was stated in his discharge) for incurable dysentery. I examined the rectum, and finding some ulcers close to the anus, had them touched with the nitrate of silver. Under this treatment a rapid amendment took place, and in the space of three weeks the man was discharged, quite cured. Now, are you to make this examination in every case? I believe you will act rightly in doing so in every case of chronic diarrhœa in the male, but the examination is absolutely necessary in all cases under the following circumstances: first, when the diarrhœa has been of long standing; secondly, when it has resisted a great variety of treatment; thirdly, when it has been combined with tenesmus and a desire of sitting on the night-chair after a stool has been passed, showing irritability of the lower part of the great intestine; and, lastly, when the patient's health does not appear to be so much affected as it naturally should be, where there was long-continued disease of a large portion of the great intestine. A patient will come to consult you, who will inform you that he has had eight or ten alvine evacuations every day for the last six months, and yet he eats heartily and looks quite well. Under these circumstances, the cause of the diarrhœa will generally be found to be ulceration of limited extent low down the tube, and capable of being quickly and effectually

removed by a strong solution of the nitrate of silver. I shall recapitulate all the circumstances under which an examination is indispensable; where the symptoms have been persistent, have resisted a variety of treatment, are accompanied by tenesmus, and where the injury done to the general health is not in proportion to the duration of the disease. I may mention here, that a medical friend of mine has communicated to me the particulars of another case of this form of diarrhœa in a soldier who was invalidated on this account, and who experienced sudden and permanent relief from the application of nitrate of silver to some ulcerated spots which were discovered near the termination of the rectum.

We come now to the subject of *dysentery*. I shall draw your attention briefly to the general principles of the pathology and treatment of this affection; but I do not intend to enter upon the consideration of its general history, which you will find sufficiently detailed in books. The first principle I have to enforce on this subject—and you may take it as an observation based on the soundest pathology—is this, that dysentery is inflammation of the large intestine. In some cases it is complicated with fever, and in others with disease in the upper portion of the digestive tube; and I believe that those cases which are termed *epidemic dysentery*, are those in which this disease is combined with typhus fever, or with an extensive affection of the small intestines—where there is ileitis as well as colitis. I shall not take up your time with discussions respecting epidemic dysenteries, or those of warm climates; it will be sufficient, for the present, to allude to that form of disease which is observed in this country.

I have told you that dysentery is an inflammatory affection of the great intestine, and all the symptoms during life, as well as the phenomena revealed by dissection, tend to confirm this view of the subject. We often have fever, because the constitution sympathises with the inflammation of an important organ; we have excessive pain and irritation of the intestine, in consequence of its muscular fibres being involved in the inflammation; and we have discharges of morbid, purulent, and bloody secretion. You will now please to inspect this preparation, and hand it round. See the effects of dysentery—the extensive inflammation, ulceration, and sloughing, of the mucous membrane. Here is another preparation; you perceive the whole surface of the colon is covered with coagulable lymph, which, in some cases, forms a chief part of the dejections. Here is a preparation which exhibits extensive sloughing of the mucous membrane; its tissue, you see, is quite abraded and destroyed. Here is a preparation of chronic dysentery, which presents a very curious appearance; the mucous membrane is finely mamillated, as it were, and it is stated on the label, that the process of cicatrisation was going on. If you compare it with the others, you will find a remarkable difference. Here is another specimen of dysenteric destruction.

Here, then, is a disease in which we have violent inflammation of the mucous membrane and submucous cellular tissue, and, in

severe cases, I believe, of all the coats of the great intestine, except the serous. Let us rehearse its symptoms briefly. Fever of an inflammatory or typhoid character, great pain and excessive irritability of the great intestine, morbid discharges of purulent, bloody, and lymphic matter, twisting pains called *tormina*, and frequently the absence of fæcal matter in the dejections.

At my next lecture I hope I shall be able to finish this subject, and I shall then bring before you some remarks on constipation and collections of air in the great intestine, two points upon which much light has been lately thrown.

LECTURE XI.

Sporadic dysentery—Nature of this disease—Treatment; mercurial, stimulating, antiphlogistic—Recommendation of Dr. Elliotson—Success of Dr. O'Beirne in the use of tobacco injections—Tympantitis, or meteorism—Windy colic, remedies for the cure of.

I drew your attention briefly, in my last lecture, to the subject of dysentery; I stated that its anatomical character is now known to be inflammation of the great intestine, and gave it as my opinion, that, in many cases of the epidemic, disease of the large intestine occurs under one of two conditions, either as secondary to typhus fever, or with an extension of the inflammatory process into the small intestine. These circumstances should, I think, be always taken into consideration in cases of epidemic dysentery; but the ordinary sporadic dysentery of this country, which we have now to consider, is, generally speaking, an inflammation of the large intestine. The old doctrine on this subject was, that dysentery was the result of an irritation caused by the presence of scybalæ in the colon; and the indication was to attempt their removal by purgatives. You will find this opinion put forward in many of the older authors, and that the plan of treatment which they recommend is in perfect accordance with their notions of the disease. It is a very curious fact, however, that in this country these hard fæcal masses, or scybalæ, are very seldom met with in cases of dysentery. During the epidemic of dysentery, which occurred in Ireland in 1818, a series of clinical investigations was made on an extensive scale by Dr. Cheyne, who at that period had charge of the Hardwicke Hospital; and he states, that on a strict examination of the discharges in a vast number of cases, no scybalæ could be discovered; and in the sporadic cases, which we receive from time to time into the Meath Hospital, I have never found that the patients passed them. It is a great error to think that dysentery depends on the presence of scybalæ; the notion is now shown to

be founded on a false pathology, and the treatment which it inculcates decidedly bad. You will be convinced of the latter when you recollect that the disease is inflammation of the great intestine, that its effect is to throw the muscular fibres of the gut into violent and painful contractions, and that the existing mischief must be therefore greatly increased by the exhibition of strong purgatives. For a knowledge of the true and scientific treatment of this disease, we are indebted to the light which modern pathology has shed upon practical medicine. We now employ purgatives with extreme caution, we use general or local bleeding, according to the urgency of the case; and we treat the disease as an inflammatory affection of the lower intestine demanding active depletion. All writers are unanimous in recommending the employment of the lancet, in cases of acute inflammation; and acute dysentery is one of those cases in which general bleeding seems to have the best effect. Dr. Cheyne states, that in this disease the most decided relief resulted from the use of the lancet. He says that in several cases in which there were excessive pain and tormina, and in which nothing was passed for several days but mucous and blood, as soon as venesection had been performed, the patients became comparatively easy, *and passed large quantities of feculent matter*. He also found that the blood drawn was buffed and cupped; and states that his experience led him to conclude that this disease was best treated by the lancet. Dr. Mackintosh, who has had great experience in dysentery, says, that laxatives will act with the best effects when blood-letting has been premised. In fact, the utility of general bleeding in dysentery is established beyond any possibility of doubt; and those who object to the use of the lancet object to it on theoretical and not on practical grounds. As a proof of this, you will see a great many cases in which decided relief is obtained by a natural hemorrhage from the bowels; and this I think ought to be sufficient to overcome the doubts of those who are skeptical as to the value of general bleeding in acute dysentery.

Next to bleeding, the best thing you can have recourse to is the free application of leeches, a practice not sufficiently appreciated or followed in this country. I would advise you to apply leeches freely along the course of the colon; and if the tenesmus be constant and distressing, round the anus also. The case in which the application of leeches round the anus is attended with the greatest relief, is that in which the tormina and tenesmus are excessive, and in which a quantity of blood is found blended with each discharge. After you have applied the leeches, I would strongly recommend you to direct your patient to sit in a hip bath for some time, and you will find that he will experience great relief, because the bath will act as a fomentation, and promote the flow of blood from the leech bites. I have often seen the application of a dozen leeches round the anus, followed by the hip bath, attended with the most rapid and signal advantage in dysentery.

Many persons are in the habit of giving small doses of some mild saline laxative in this affection; of this practice I cannot

speaking much from experience, and I think more benefit will be derived from the free use of demulcents, gum-water, whey, barley-water, and linseed tea. But the internal remedies on which we chiefly rely in the treatment of dysentery, are mercury and opium. Blue pill and Dover's powder are an excellent combination, so are calomel and opium, and you may give either of these remedies alternately with a mild laxative, whenever you are led to suspect an accumulation of fecal matter in the bowels. In very bad cases it will be necessary to continue the mercury until the mouth is affected; but in the sporadic dysentery of this country you will very seldom be under the necessity of bringing on actual salivation.

Permit me here, gentlemen, to make a few observations on mercurial action. In treating a case of dysentery, it does not, in the first place, follow as a matter of course, that you will cure your patient by subjecting him to the full influence of mercury. You are not to expect that salivation will be always attended with success. There is another point which should never be forgotten, although it is one which I believe has not been sufficiently considered. It is a common idea with respect to the administration of mercury in cases of local inflammation, that if you produce salivation you do a great deal towards accomplishing a cure, and this is true in most cases. Many persons are of opinion that it is the ptyalism which carries off the disease, and hence it is that we so often see the principal share of a practitioner's attention directed to produce salivation *at all hazards*. This is the history of the medical treatment ordinarily pursued in warm climates, where such vast quantities of calomel are given. Here the idea seems to be, that the disease is to be subdued by salivation alone, and accordingly the practitioner "throws in" mercury, an expression evidently arising from the enormous quantities given. There are many cases on record in which eight hundred and even one thousand grains have been given for the cure of a single local inflammation. But it is remarkable, that in several cases in which vast doses have been given, no ptyalism has been produced, and thus it frequently happens, that the practitioner goes on increasing the quantity, lest he should have failed in consequence of not having given enough. All this practice is wrong and founded on false notions; and I think that when you come to practice yourselves, you will be inclined to adopt the opinion, that, in cases in which mercury has been employed in the treatment of local inflammation, salivation is to be looked upon more as the result of the relief of inflammation to a certain degree than as its primary cause. For instance, suppose you are called to treat a case of acute enteritis or hepatitis; you give ten grains of calomel two or three times a day, and find that day after day passes without any appearance of salivation. Another practitioner is called in, who bleeds the patient, and this is almost immediately followed by the appearance of salivation and relief. My friend, Staff-Surgeon Marshall, who is intimately conversant with the diseases of India, has informed

me that *he has never known a case in which abscess actually formed in the substance of the liver*, in which salivation could be produced; and that when the patient became salivated, he believed it to be a proof that there was no inflammation of an intense character, or that no abscess had formed. The greater the intensity of the disease, the less was the chance of salivation occurring, so that the salivation in certain cases appears to be the result of the same influence which produces a relief of inflammation, and not the cause of that relief. When, therefore, you have given mercury in free and repeated doses for twenty-four or forty-eight hours, and find no sign of salivation appearing, you should be cautious how you proceed, because in such cases the inflammation may be of that intense character which will not permit the mouth to be affected. Under such circumstances, the use of mercury, if rashly persevered in, will only aggravate the disease. In many cases of intense pneumonia, you will find that the patient will not be salivated until an advanced period, when, in consequence of the subsidence of intense irritation, the mercury is, as it were, allowed to produce its effect on the salivary glands. You may also frequently observe instances of intervals between the salivation, in which, during the course of an inflammation, the patient's mouth becomes affected by mercury; but if he gets fresh symptoms of the original affection the salivation disappears, and returns only when the new attack has been overcome by appropriate treatment. I think that, under these circumstances, we are authorised in considering salivation as the effect of a certain degree of reduction of inflammation, and not as its cause. You will see the importance of these observations when you reflect in how many cases of local inflammation practitioners are in the habit of trusting to calomel alone; not being aware of the fact, that inflammation of an intense character has a powerful tendency to prevent it from acting on the salivary glands. Be assured of this, that if, in any acute visceral inflammation, after you have performed the usual depletions, you find an unusual resistance to the action of mercury, you may, on that account, form a more unfavourable prognosis.

There is one point in the treatment of dysentery which it is necessary you should be acquainted with. Sometimes the symptoms steal on gradually, and the patient appears to be in a condition not at all dangerous, when, all at once, the disease explodes with violence, and exhibits an extraordinary intensity; the fever is ardent, the tormina excruciating, the tenesmus constant and harassing, the dejections frequent and blended with lymph and blood. Such an array of threatening symptoms must be met with a corresponding activity. In such a case as this I would bleed, leech, use the hip bath, and give free doses of calomel and opium; and if you were to ask me to which of the internal remedies used I should attribute the most decided alleviating influence, I should say to the opium. Dr. Cheyne says, "after the lancet, the best remedy I know of is opium." He says further: if another epidemic, similar to that which he witnessed, occurred, he would

have no hesitation in giving opium, in four-grain doses, in such cases.

There was a very curious circumstance connected with the history of the epidemic dysentery of 1818—19. At one time the deaths happened to be extremely numerous, and every thing which the experience or ingenuity of Dr. Cheyne could suggest failed in arresting the disease, in many cases. An English physician, who happened to be in Dublin at that period, and was in the habit of visiting the hospital, proposed the administration of large doses of cream of tartar, stating that he had tried it on several occasions under similar circumstances, and was convinced of its value. As the cases were not succeeding which had been treated after any of the ordinary modes, Dr. Cheyne consented to the exhibition of the cream of tartar, and allowed the physician to prescribe and administer it himself. Accordingly, he proceeded to give it in doses of half an ounce every fourth hour. Its first effect, generally, was to produce violent distress, and to aggravate all the symptoms, but, after three or four doses, bilious and feculent stools came away, and the patient experienced the most extraordinary relief. Many cases which had been considered desperate improved and recovered, and Dr. Cheyne expresses his conviction that many persons were saved by this practice, who would have been lost under the ordinary modes of treatment. One of the old German authors has also alluded to this singular efficacy of cream of tartar in the treatment of dysentery; and from the result of Dr. Cheyne's experiments, there can be no doubt that it is entitled to a high rank among the remedies usually employed. In case you should prescribe castor oil as a laxative, it will be necessary to combine it with mucilage of gum arabic and a few drops of laudanum; given alone it will be likely to prove too irritating, particularly during the acute stage. In the advanced stage much benefit will be derived from a combination of castor oil with tincture of opium and a small quantity of oil of turpentine. This is not at variance with the pathology of the disease, for there is a period in this as well as in every other form of inflammation, when stimulants may be used with benefit.

Such is the treatment of the ordinary forms of acute dysentery; but it may happen that you will be called to a case in which you cannot employ these decided measures; and here I shall mention, that in all local inflammations it is of the utmost importance that you should act with judgment and decision in the commencement. Every hour is precious; a single day is worth much; and if two or three days are allowed to pass, and the treatment is inactive or indecisive, the patient too often sinks into the chronic stage, or dies. Whenever you happen to be called to treat a case of acute local inflammation, attempt to cut it short as soon as possible; it is much easier to cure an inflammatory attack in its commencement than to save the patient from the effects of it in the advanced stage. Now, if you should be called to a case of dysentery of some standing, and on your arrival find the patient lying on his back, his skin of a pale dirty hue, his eyes sunk and without lustre, his

extremities cool, and bedewed with a clammy sweat; his pulse small, rapid, and feeble; his thirst ardent; his pains and tormina incessant; and constantly passing from his bowels a quantity of fluid matter, blended with depraved mucus, lymph, and blood, with great irritation about the anus, and if these symptoms have lasted for some days you may be sure there is extensive ulceration of the lining membrane of the large intestine. How are you to act under such circumstances? The patient will not bear bleeding, or perhaps the application of a small number of leeches. Here your sole object must be to support your patient's strength; you must give wine, (if the skin be cool,) strong chicken broth, beef tea, jellies, &c.; you must wrap your patient in flannel, and have recourse immediately to anodyne and astringent injections, and you should blister the abdomen, taking care to remove the blister at a proper time, and not leave it on so long as may add to the existing irritation. You may also prescribe the acetate of lead, or the sulphate of zinc with tincture of opium. I have seen several cases of this kind in the Meath Hospital, in which the administration of the sulphate of zinc was attended with good effects. The best mode of using it is to dissolve ten or twelve grains of the sulphate of zinc in six or eight ounces of cinnamon water, with a proportion of laudanum, and direct this quantity to be taken during the twenty-four hours. Dr. Elliotson recommends the sulphate of copper, and you can employ it in combination with opium. In this way, by supporting your patient's strength, keeping him warm, paying attention to the state of his bowels, using counter-irritation, and prescribing astringents combined with opiates, (taking care not to check the discharge too suddenly,) you will often succeed, even in very bad cases. Before I quit this subject I may observe, that Dr. O'Beirne has succeeded in some cases, and in others has given great relief by the use of tobacco injections. You can understand this when you reflect that tobacco acts powerfully on the general system, and produces effects somewhat analogous to bleeding. Like general bleeding it brings on faintness, vomiting, cold skin, perspirations, and feeble pulse. It is also a powerful antispasmodic, and Dr. O'Bierne states, that its employment has been attended with the best effects in several very bad cases. I have not tried this remedy myself, but I think it well worthy of a trial in the acute stage of dysentery, when there is room for an antiphlogistic treatment. In the advanced stages, of course, it is inadmissible.

We come now to consider the affection of the digestive tube, which merits a separate consideration, and this is tympanitis, or, as it is sometimes termed, meteorism. I shall not enter upon the general pathology of æriform effusions into the abdomen; we are not acquainted with that peculiar condition of parts which produces them, but it is now established that we may have effusions of air, not only into the digestive tube, but also into every part of the body. The term tympanitis is limited to effusion of air into the digestive tube, in all parts of which we may find it. We detect it in the stomach under two circumstances; first, as a recent and

transient affection, as when it comes on after swallowing indigestible matter; secondly, in a more permanent form, as when it depends upon hysteria, hypochondriasis, or chronic gastritis. It may be also frequently seen in very young children, when there is feverishness with irritation of the digestive system. I recollect a very remarkable case of this kind, in which the distension was so great, and the pressure on the diaphragm so considerable, as to cause displacement of the heart upwards;—this, I believe, has not been mentioned among the causes of displacement of the heart. The symptoms of this affection are sufficiently obvious;—a sense of uneasiness and distension at the region of the stomach; when the effusion is in excess, a distinct tumour can be felt, and the sound on percussion, over the stomach, is like that of a drum. It often happens, also, that when the patient is shaken, a distinct sound of fluctuation is heard, a circumstance which more than once has led to the suspicion of the existence of pneumothorax, or empyema. There are also cases on record, in which the distension was so great as to cause rupture of the stomach, and effusion of its contents into the cavity of the peritoneum, causing intense inflammation and rapid death.

The effusion of air into the intestinal tube is extremely common in cases of acute enteric inflammation and gastro-enteritis, after the disease has lasted for a few days, and, as this is a matter of considerable interest, I wish to make a few remarks upon it. It is of importance that you should bear in mind that this is one of the results of enteric inflammation, because many persons are in the habit of looking upon it, not as a mere symptom of another affection, but as a peculiar form of disease, forgetting that it may occur with, as well as without, inflammation. In consequence of this limited and imperfect view of the subject, they are in the habit of prescribing turpentine as a specific remedy for tympanitis. Now, I can say that I have seen the most dreadful effects from the administration of turpentine in the tympanitis of acute enteric inflammation. The immediate effect is to produce a rapid diminution of the tympanitic swelling; but this is purchased at too dear a rate; for you will find next day that there will be a violent exacerbation of the existing symptoms, and the tympanitis becomes worse than before. You should never, therefore, interfere in this way with the tympanitis of acute enteric inflammation, nor should you alter your practice on this account in the slightest degree, except where the tympanitis is so great as to interfere with the due performance of the function of respiration; but, in the advanced stage, after the twelfth or sixteen day, when the fever has abated and the tongue is moist, I have frequently seen great advantage result from the use of turpentine. *But as long as the condition of your patient admits of antiphlogistic treatment, be assured that the administration of turpentine is hazardous.* When the patient is in a low state, when you can no longer have recourse to bleeding or leeching, when the tympanitis is connected with an asthenic condition of the intestinal mucous membrane, then, and not till then, should you venture on

the employment of turpentine. I shall return to this subject when we come to speak of hysteria.

I may mention here, that the occurrence of flatus in the intestines sometimes gives rise to dreadful sufferings in that affection which has been termed windy colic. A person in the enjoyment of good health happens to take at his dinner or supper a quantity of indigestible food, he goes to bed without feeling any particular inconvenience, but about the middle of the night he awakes with an attack of pain and tormina, which extend from the hypochondria to the umbilicus. This subsides for a short time, and then returns with violence, and the patient often finds that it is relieved by pressure. In a short time the pains get worse, and the abdomen begins to swell, sometimes at one point, sometimes at another, as if the air was confined and pent up in particular situations. The patient begins to suffer indescribable anguish, he has great anxiety, extreme prostration of strength, his face is pale, his extremities cold, a cold sweat breaks out all over the body, and he sits bent forwards, with his hands pressed on his stomach to relieve the paroxysms of pain which come on with increasing rapidity. In some cases there is distressing hiccup, in some a large quantity of aqueous urine is passed, in some there are loud borborygmi, and the intestines may become so enormously distended as to fall rapidly into a state of gangrene. Hippocrates has given a description of one of the forms of this disease, which terminates by the passage of air upwards and downwards, by which the patient obtains relief; this he calls dry cholera. This windy colic is an exceedingly violent disease: one of the first cases of it which I witnessed, presented such an array of alarming symptoms, that I thought every moment the patient would expire. It is, however, a disease which is generally easily managed if taken in time. One of the first things to be done is to apply heat to the abdomen by anodyne stupes, or warm flannel. Flannels wrung out of a decoction of poppyheads, as hot as can be borne, will do a great deal of service, and in some cases will give complete relief, when assisted by the use of carminative draughts. But of all the remedies which I have seen, the most efficacious is an injection with tincture of *asafetida*, turpentine, and opium. This is generally followed by speedy relief, the pulse becomes more natural, the belly soft, and the excruciating agony is relieved. This is the mode of treatment in which I have the greatest confidence. After the acute symptoms are removed, it will be proper to exhibit a laxative, for the purpose of removing the exciting cause of the disease—indigestible matter; unless you get rid of this, your patient is liable to a return of the attack, and even to an inflammation of the tube itself. Be not, therefore, satisfied with merely relieving your patient; watch him carefully, and, by a proper treatment, obviate a recurrence of the symptoms, and prevent any tendency to inflammation.¹

¹ When the flatus is pent up in the large intestine, as it often is, (the very term *colic*, indeed, indicates the common belief of old, that the colon is its ordinary seat,) instantaneous relief is often afforded by passing up a hollow

LECTURE XII.

Pathology of jaundice—Its co-existence with a flow of bile—Case of aneurism of the hepatic artery—The disease, independent of mechanical construction—Colouring of the various parts—Effects on the milk, and humours of the eye—Jaundice with preservation of health—Icterus infantum.

To-day we have to enter upon the consideration of a subject, the nature and extent of which claims for it a more than ordinary share of importance—I allude to that form of disease which is termed jaundice. I have selected this disease for our present lecture, because I think we may look upon it as presenting a series of phenomena, which form a distinct link of connection between affections of the liver and the digestive tube. In the first place, jaundice, and I wish to impress this upon your attention, is to be regarded as a symptom rather than a disease *sui generis*, and that it is a symptom which occurs in many diseases of a most essentially opposite pathological character. There is nothing, for instance, more different than disease accompanied by acute inflammatory action, and disease without any inflammation at all; yet we may have perfect jaundice as a consequence of the one as well as the other. No diversity can be more complete than that which exists between the jaundice arising from inflammation and organic lesion of the liver, and that which results from simple mechanical obstruction of the biliary ducts. It is, therefore, to be looked upon not as a disease but as a symptom, and we may define it by saying, that it is a state in which the solids and fluids of the body are tinged more or less deeply with bile. Generally speaking, this presence of bile in fluids and solids where it should not be normally, is accompanied by the absence of that secretion in the place where it is naturally found, the digestive tube. Yet it is an interesting physiological fact, and one of practical importance, also, that we may have plenty of bile in the stools during an attack of jaundice, or *that we may have jaundice co-existing with even a copious flow of bile*. This is a strong proof in favour of the opinion, that some cases of jaundice have no connection or dependence on the absorption of bile into the system, as, in the instances to which I have alluded, there is no mechanical retention of bile; the biliary ducts and gall-bladder are open, the bile passes freely into the intestines, and yet the whole body is jaundiced.

I have told you that jaundice is a symptom which is produced by a variety of causes—these I shall briefly enumerate. Without entering into the ultimate mode of action of these causes, and their

bougie through the annulus of the rectum into the colon. The editor had the satisfaction of preserving the valuable life of an aged individual, who had been long honourably engaged in the service of his country, by this simple expedient. Injection after injection had been employed ineffectually. An elastic gum male catheter was passed up, the flatus was immediately discharged, and the pain and tympanitic distension were at once removed.

R. D.

separate effects on the economy, it will be sufficient for my purpose to mention them individually. The first of these causes I take to be mechanical obstruction to the exit of the biliary secretion. Under such circumstances one of these two things is supposed to take place, either that the bile, which is poured into the biliary duct and gall-bladder, and cannot get into the duodenum, is re-absorbed, or, according to another opinion, that the innervation of the liver is injured; in other words, that the liver is paralysed and unable to perform its ordinary functions, and that, consequently, it does not separate the materials of bile from the blood. The latter opinion has been advanced by men of high authority in the medical world, but when we find, on dissection, (as is not unfrequently the case in jaundice,) the biliary ducts and gall-bladder distended with bile, we cannot infer a paralysis of the liver as the cause of the disease, we must attribute it to the re-absorption of bile. I have taken mechanical obstruction to the flow of bile as one of the causes of jaundice. Now, you will find this to depend, in the first place, upon the presence of gall stones in the biliary or common ducts. A biliary calculus is formed in one of these ducts, it excites violent irritation, spasmodic pain, and often (but not always) jaundice. At my next lecture I will show some specimens of this obstruction. In the second place, the biliary ducts may, from various causes, become obliterated; they may be closed by adhesion, as the consequence of inflammation, or they may be impervious as the result of congenital malformation. In some cases children have been born without biliary ducts, in others the ducts have terminated in a *cul de sac*. A third cause of jaundice by mechanical obstruction is, where the flow of bile has been prevented by the pressure of tumours on the biliary ducts. Of this one of the most familiar instances is disease of the head of the pancreas, or malignant disease of the pylorus or duodenum. I have on a former occasion alluded to a case of jaundice produced by aneurism of the hepatic artery, one of the rarest pathological circumstances on record, and one which has not been hitherto described. So rare is it, that at a late meeting of the Académie de Médecine, that eminent pathologist, Cruveilhier, stated that he had never seen a case of it. I was so fortunate as to meet with an instance of this uncommon form of disease, and will take an early opportunity of exhibiting the preparation of it to the class. You will see by it how an aneurism of the hepatic artery may cause a complete obstruction to the flow of bile, and I shall be able to show you, that not only the trunks, but also the minute ramifications of the biliary ducts, are enormously dilated and filled with retained bile, and that these dilatations are continued up to the peritoneal surface of the liver, forming as it were so many aneurisms by dilatation of the biliary ducts themselves. The last cause of jaundice from mechanical obstruction, is that which depends upon the accumulation of scybalous matter in the bowels, a thing frequently met with in old persons. Dr. Marsh alludes to this form of the disease in his admirable paper on jaundice in the Dublin Hospital Reports, and brings forward cases in which the

jaundice disappeared rapidly under treatment calculated to remove accumulations of hard fecal matter from the intestines. So much for the varieties of jaundice which depend upon mechanical obstruction. Before I quit this part of the subject it will be necessary to allude to another form of the disease, which bears some analogy to those already mentioned, namely, the spasmodic jaundice. With respect to this variety there exists a great deal of doubt; some persons maintain that the ducts are muscular, and consequently liable to spasm like all other parts of the muscular system; others deny the existence of muscular fibres in the ducts; while a third party are of opinion that the spasm resides in the duodenum, and that the contraction of its muscular fibres is the sole obstacle to the free passage of bile. It is of very little consequence which of these opinions we adopt; the fact is, that this is a form of the disease which we occasionally meet with in persons of an hysterical or hypochondriac habit, but what is its exact seat we cannot ascertain. The probability is, that it is spasm of the duodenum itself.

The next class of causes giving rise to jaundice, are those which are connected with acute or chronic disease of the liver, as, for instance, the different varieties of hepatitis and the existence of morbid growths in the substance of the liver. Here, however, it must be recollected that the occurrence of hepatic disease in the acute or chronic form does not necessarily imply the existence of jaundice; in other words, there are some cases of disease of the liver in which bile is freely discharged into the digestive tube, others in which it is not, so that the non-secretion of bile and the consequent production of jaundice are to be looked upon as accidental complications. I have seen a case in which there was enormous destruction of the liver from suppuration, where one of the lobes was almost entirely converted into a bag of purulent matter, and the other extensively diseased, yet the patient had not the slightest tinge of jaundice. We are ignorant, therefore, of the cause which determines the production of jaundice in one case of hepatic disease, and not in another; the question remains to be decided by future investigations. All we know is this, that it may occur or be absent in every form of acute or chronic disease of the liver.

The third great source of this affection is disease of the mucous surface of the stomach and duodenum, the most important, because it is the most frequent, cause of jaundice. We are indebted to the researches of modern pathology for a correct notion of this form of the disease, and for the invaluable light thrown upon its treatment, which, up to the time of Broussais, had been extremely confused and empirical. Inflammation of the upper part of the digestive tube is an extremely frequent cause of jaundice, and this result is, generally speaking, *independent of any mechanical obstruction of the gall-bladder or biliary ducts*. This phenomenon may be explained by calling to mind the various examples of sympathetic irritation, and by recollecting that disease in one situation frequently produces disease in another, or, in other words, that we have an irritation of the stomach and duodenum, in which the liver sympa-

thetically partakes, and, as a consequence of this, the biliary secretion is arrested. In a former lecture, I alluded to the strong sympathy which is known to exist between mucous membranes and the glands whose ducts open upon their surfaces. It is supposed by some that the irritation existing in the duodenum may be extended to the liver, producing paralysis of the functions of that organ and jaundice. It would appear, also, that the yellow fever of warm climates is only a variety of jaundice depending upon irritation of the gastro-intestinal surface. On this point the best pathologists seem to have made up their minds.

The last cause of jaundice seems to consist of the sympathetic action of the brain upon the liver, and this is an extremely curious circumstance. There are numerous cases on record of persons who have received an injury of the brain becoming jaundiced, and the same affection has been repeatedly known to supervene on powerful mental emotion. Thus we find that Murat, on learning that his queen had assumed the sovereign power of Naples in his absence, fell into a violent passion, and became almost immediately jaundiced. The close connection which exists between the brain and the biliary system has been long known; it is unnecessary, therefore, that I should enter upon its consideration, for the purpose of accounting for an occurrence the nature of which must be obvious to all. You will, however, find that jaundice is, in the majority of cases, connected with disease of the gastro-intestinal surface, and that this is one of the most common causes of the sporadic jaundice of this country. I shall return to this subject on a future occasion, when we enter upon the consideration of hysteria.

Before I enter upon a description of the separate forms of jaundice, it will not be amiss to premise a few general remarks. I told you, at the commencement of my lecture, that we define jaundice by saying, it was that state in which the solids and fluids of the body were tinged more or less deeply with bile. Now, is this definition to be received without any exception? and does it embrace all the solids and all the fluids of the body? I have stated, that in some cases you will not be able to detect the slightest trace of bile in the stools. This is, however, but an apparent exception; it is, perhaps, because the bile is too small in quantity to be able to overcome the diluting power of the ingesta, or that the portion of it which finds its way into the digestive tube is too small to be appreciable by our senses under these circumstances. The rule of universal colouring in this disease will not, I believe, hold good, at least there are certain fluids and solids which are tinged only in a very slight degree; but the majority of the textures and fluids have been observed to be more or less distinctly coloured. For instance, we find the jaundiced tint appearing in bone, cartilage, muscle, in the cellular membrane, in the central portions of the teeth, but not in their enamel. It is doubted whether the hair is coloured or not, but it is the opinion of many that it is, and a professional friend of mine has assured me that he has had unquestionable proofs of the

colouring of the hair. The membranes of the brain are distinctly tinged. I have seen the arachnoid and pia mater decidedly coloured in a case of dreadful gastro-duodenitis, to which I shall call your attention on a future occasion. The substance of the brain, however, has not been found to partake in this universal discoloration. Frank, who is a good authority on this point, states that the substance of the brain is never coloured, though the membranes may, and most commonly are. In my experience of jaundice, I have found the membrane distinctly coloured, but never could see any tinge of yellowness in the *substance of the brain*. I have however observed, that when a horizontal section of the brain had been made in such cases, the orifices of the divided vessels, which are denoted by bloody points in the healthy state, seem to pour out a quantity of yellowish blood, but the substance of the brain appeared white and normal.

With respect to the state of the fluids, you will find the blood distinctly coloured; the saliva also is yellow; the urine is loaded with bile, it stains the linen, and chemical analysis shows that a large proportion of the biliary secretions is blended with it. The perspiration is also tinged with it; and if you apply a blister you find the exuded serum bilious. If a person, labouring under phthisis or bronchitis, should happen to get an attack of jaundice, the pulmonary secretions will be often tinged with yellow. The mucous secretions from the vagina and uterus are also discoloured; but it is an interesting and curious fact, that the milk during lactation seems to escape the general impregnation with bile, and is never tinged. This would appear to be a beautiful provision of nature to prevent the child from being injured. Frank, who witnessed two epidemics of jaundice, one at Mayence, in 1754, and another at Ghent, 1742, states that he has never seen the milk tinged with bile. Dr. Marsh, in his paper on jaundice, mentions that in the case of one unfortunate female a yellow fluid was squeezed from the breasts after death; but this cannot be considered as a proof of the existence of bile in the milk during life.

In jaundice the eye almost always presents a very distinct yellow tinge, and yet it is a curious and interesting fact, that the patients very seldom complain of yellow vision. Out of several thousand cases of jaundice, Frank only met with five in which this symptom was observed. The occasional occurrence, however, of yellow vision in jaundice, has excited a good deal of interest; and Drs. Graves and Elliotson, who have turned their attention to this subject, have made some ingenious and valuable remarks on this singular phenomenon. Dr. Elliotson's opinion is, that where this symptom is complained of, the cornea is in a state of irritation or inflammation, and that under these circumstances its vessels, which in their physiological condition are too small to allow of the passage of coloured fluids, become dilated, so as to carry bilious blood across the field of vision, and thus cause all objects to wear a yellow hue. To support this opinion, he brings forward the case of a jaundiced patient, who had a considerable degree of inflammation in one eye

but none at all in the other, and who saw objects yellow with the inflamed eye, but of their natural colour with that which was free from inflammation. This case is, indeed, as far as it goes, extremely interesting; but I think it does not prove the point in question, namely, that the cause of jaundiced vision is irritation of the cornea, for it is a fact that even when the cornea is deeply tinged, yellow vision is not of constant occurrence, nor does it affect all persons alike. One person sees objects in their natural colours; to another under the same circumstances every object appears to wear a yellow hue, and what is equally remarkable, this yellowness of vision is frequently intermittent; it is present to-day and disappears to-morrow. These are extremely curious facts.

The object of Dr. Graves on this subject, in the Dublin Medical Journal, is to explain the cause of the absence of yellow vision in certain cases of jaundice. He believes that the humours of the eye frequently escape the jaundiced tinge, and suggests that this may be a beautiful provision of nature for the preservation of sight. From his own observations he states that the aqueous, and perhaps the vitreous, humours escape. But, it may be objected to this, that when all the fluids, the blood, saliva, serum, perspiration, &c., are impregnated with bile, how is it possible that the fluids of the eye should escape?—Does it not seem very extraordinary?—It does, certainly; but that it is possible seems to be established by the following circumstances;—you are not to conclude, because all the fluids which are found to exist in the blood are filled with bile, that the secretions, properly so called, which do not exist in the blood, should be also tinged with bilious discoloration. This is the answer which Dr. Graves makes to this objection—I recollect two cases of malignant cancerous disease of the liver, which were some time ago in the Meath Hospital, and which presented symptoms of universal jaundice before death. In these cases we found fluids deeply impregnated with bile—every thing, in fact, seemed bilious and discoloured; and yet, you will hardly credit me when I tell you, that, on opening the gall-bladder, *it was found to contain a quantity of beautiful limpid fluid, perfectly transparent, and of a high refractive power.* Here, then, is a fact to prove that we may have intense general jaundice, and yet find in a sac, existing in a system so diseased, a quantity of fluid perfectly free from any bilious admixture, proving, at least, that it is possible that the humours of the eye may in a similar manner escape. Dr. Graves further remarks, that, even where the humours of the eye happen to become tinged, the alteration in the colour of objects may still escape the observation of the patient; because the change takes place gradually and insensibly. The patient does not think every thing he sees is yellow; he believes still that they are white, because the transition from one colour to the other has been so insensible as to escape his notice. This reasoning may, I think, apply to cases of yellow vision coming on gradually, but will not explain those in which it has been of sudden occurrence. The other cause which Dr. Graves adduces as tending to prevent a patient with a yellow

cornea from seeing objects of the same colour, is, the want of some standard of comparison to judge by. He has no means of comparing objects; and, though he sees this piece of paper, for instance, (yellow,) he thinks it white, because every standard he looks to, every other piece of paper he examines, presents the same tinge, Dr. Johnson states, that most of the jaundiced patients whom he has interrogated were sensible of the alteration in vision to a greater or less degree, and observes that the power of appreciating varieties of colour is retained, though we look through a yellow medium not deeply dyed, though yellow, of course, is made to enter into this composition. You will see this observation in the Medical Chirurgical Review for October, 1833.

I shall conclude this subject with an observation which suggests itself to me, and this is, that the alteration of colour and vision may arise from other causes than the mere jaundiced condition of the eye; and that it may (I believe this has not been taken notice of before) depend upon direct nervous influence. There are cases on record of patients labouring under typhus fever, who, without being in the slightest degree jaundiced, saw every thing yellow. There are also numerous instances of various colours, differing from the natural hues of the objects, being seen by patients in consequence of affections of the nervous system; and hence it is extremely probable that many cases of yellow vision in jaundice may depend upon a functional lesion of the optic nerves. I have one fact to bring forward on this subject of great importance. In the case of jaundice from aneurism of the hepatic artery, the patient saw every thing intensely yellow, until a few days before death, when all yellow vision subsided, and he saw objects of their natural colour, though the jaundice continued, if possible, more intense than ever. In this case there was no inflammation of the eye. I do not think that Dr. Elliotson's observations apply to all cases of this phenomenon. All that he has said is, that where the cornea is in a state of inflammation, there is a greater probability that there will be yellow vision in the affected eye or eyes; and this can be easily accounted for by the increased size of the vessels which the inflammatory process brings on. We may however conclude, that, in some cases, the alteration of vision may be owing to a yellow state of the humours of the eye, that in some it is the result of inflammation, and that in some it may be fairly attributed to a lesion of innervation. I think that the latter statement is borne out by the facts that there is a want of constancy in the occurrence of this phenomenon, that it is often of a more or less intermittent character, being one day present and another day absent, and that it has been observed in cases where not the slightest symptom of jaundice existed. We must also bear in mind that some of the most remarkable nervous systems commonly occur in jaundice, such as coma, &c.; and we may enquire how far the occurrence of yellow vision may be looked on as an indication of an excited state of the brain, and so lead us to measures calculated to remove impending danger.

Let us now return to the more immediate consideration of *jaundice*. One of the first diseases of children is the *icterus infantum*, or, as it has been termed by nurses, the *yellow gum*. Children, shortly after birth, without any known cause, become suddenly jaundiced, and this, after continuing for some days, goes off, frequently without any treatment. This form of jaundice appears to depend upon some particular irritation of the intestinal canal, which seems to result from the circumstance of the digestive system being called into active exertion for the first time, and receiving a new stimulus from the mother's milk. It is a curious fact, that this form of jaundice generally disappears spontaneously. Now, it is remarkable, in this as well as other cases, (when we recollect the nature of jaundice, and that there exists in the fluids of the body an irritating substance like bile,) that the effects of an admixture of the biliary secretion with those fluids should not be attended with more striking symptoms. In some instances we shall have intense jaundice without any particular effect upon the economy. There is some itching of skin, ardour urinæ, a little depression of spirits, and vertigo, which last for a few days and then disappear. Dr. Gregory mentions many cases of persons affected with jaundice who went about their ordinary business, and performed all the functions as if in a state of perfect health, eating, drinking, and sleeping in their usual manner. I have myself seen persons who laboured under this affection for more than a year, and yet had all that time their digestion good, their bowels regular, the flow of urine natural, and the circulatory, nervous, and respiratory systems apparently conformable to the standard of health. Dr. Blundell gives the cases of two children who lived for four months, apparently well fed and healthy; and, on opening their bodies, it was found that the biliary ducts terminated in a cul de sac, and that, consequently, not a drop of bile had been discharged into the intestines. Sir Everard Home gives a remarkable case of the total absence of the gall-bladder, and no passage of bile into the intestines, occurring in connection with a perfect state of health. These are curious facts, and should be borne in memory. I remember two cases of protracted jaundice in the persons of two male servants, who were admitted into the Meath Hospital with symptoms of irritation in the upper part of the digestive tube. From this both recovered under an appropriate treatment, but the jaundice continued in one for eighteen, and in the other for sixteen months. One of them, a stout, well-built, and fully developed man, came into the hospital some time afterwards in the apparent enjoyment of perfect health, except that he had still the jaundiced colour. He wished to be taken into the hospital to get cured of his jaundice, stating that, in consequence of the peculiarity of his appearance, he could not get a place any where, and was in a very distressed condition. From these facts it seems fair to conclude that the symptoms of other affections, occurring after jaundice, are owing to some other cause than the bilious state of the blood.

I find that my time is nearly expired; I cannot, therefore, enter

into the various causes of jaundice to-day ; at our next meeting I hope I shall be able to conclude this subject, and then pass on to the consideration of hepatic disease.

LECTURE XIII.

Jaundice from gastro-duodenitis—Researches of Broussais and Marsh on—Jaundice without hepatic inflammation—Nervous symptoms—Treatment—Yellow fever—Its occurrence in this country—Predominance of gastric irritation in warm climates—Typhus icterodes—Jaundice from biliary calculi—Different situations in which biliary calculi may be found.

We commence to-day with the consideration of that form of jaundice, which, taking *all* its cases into account, appears to be the most common. The pathological expression for this form of the disease is, that it is inflammation of the upper portion of the digestive tube, or, in other words, that it is the result of a gastro-duodenitis. In this case, an inflammatory affection of the stomach and duodenum acts sympathetically on the liver, and we have jaundice occurring independent of hepatic inflammation or mechanical obstruction to the flow of bile. This variety of the disease it is important you should be accurately acquainted with, as it is not only exceeding common in temperate climates, but because I believe it is a great cause of mortality in warm countries, and that the yellow fever of the tropics is reducible, in a great measure, to this form of disease. In other words, that the cause of the yellowness, and many other of the symptoms, is to be referred to an intense irritation or inflammation of the digestive tube, with a predominance of that irritation in its upper portion.

The jaundice which depends upon gastro-duodenal inflammation, was first accurately described by Broussais. Dr. Marsh has also made many valuable additions to our knowledge on this subject, in his paper on jaundice, published in the fifth volume of the Dublin Hospital Reports. You will find, too, that in a case of jaundice described by John Hunter, he suggests the possibility of its being preceded by inflammation of the duodenum. But I believe we are chiefly indebted to Broussais for our first correct notions of the pathology of this disease, and for its scientific and successful treatment.

The disease may occur in the acute form, or it may come on in a slow insidious manner ; but in either case, as far as my experience goes, it is always accompanied by symptoms referable to a morbid state of the mucous membrane of the intestines. Dyspeptics, and individuals subject to diarrhœa, are liable to it ; but it may also attack strong and healthy persons from the two following causes. A man is exposed to considerable heat, his body is bathed in perspiration, he experiences some degree of lassitude, and is very thirsty ; in this state he takes a large draught of cold water. In a

few hours afterwards he begins to feel uneasy, and complains of being unwell; he gets shivering, nausea, thirst, and fever, and this fever and thirst, with bilious symptoms (as they are called), continue for two or three days, when some morning, on awaking, the patient finds himself jaundiced. The same thing may happen as a consequence of error in diet. A person eats at supper a quantity of indigestible food, next day he has vomiting and thirst, and in a day or two more jaundice appears. I may remark here, that this indisposition of two or three days' standing is a very curious and interesting feature in the disease, and would seem to be connected with the progress of disease in the mucous surface of the stomach and duodenum. Jaundice from gastro-duodenitis generally occurs in this country under two varieties. The first is an extremely mild disease; it comes on with very slight and transient symptoms of constitutional or local derangement; it seldom prevents the patient from pursuing his ordinary avocations, and generally disappears without any trouble. The second variety is an extremely severe and frequently a fatal disease; between this and the former there are numberless shades and gradations.

Let us take a case of the more severe form of jaundice. The cause of this, as I have already mentioned, is often the taking a copious draught of cold water while the body is heated by exercise, or eating a quantity of indigestible food. The patient is indisposed for two or three days before the jaundice appears; he has nausea, vomiting, great thirst, loss of appetite; he complains of burning heat in the epigastrium, and there is some tenderness on pressure over the region of the stomach and duodenum. His tongue is foul, his bowels costive, his urine loaded; he has considerable prostration of strength, complains of vertigo and lowness of spirits, and is constantly sighing. There is always more or less febrile disturbance; in some cases the fever is ephemeral, and goes off in a day or two; in others it continues for a much longer period. When this fever continues beyond the second or third day, it is to be looked upon as an unfavourable sign, and you may expect that the case will be unmanageable and dangerous. There is another remarkable symptom on which I have had reason generally to found an unfavourable prognosis, *and this is a variation in the intensity of the yellowness*. In some cases, you will find that to-day the countenance and skin are much less yellow, and this is always noticed by the patient, whose spirits are generally raised by the decline of the jaundiced tint, but in a day or two it becomes as deep as ever, and it may go on in this way, alternating from a faint to a deep tinge, and vice versa. This is an unfavourable symptom; it appears to indicate the repetition of inflammatory action in the intestinal tube, because each increase in the depth of the yellow tinge is accompanied by an increase of the epigastric symptoms. In such cases as this, the patient does not, as under other circumstances, shake off the disease and return to his usual habits; he lies in bed, and though he complains of no pain, except when you make firm pressure on the epigastrium, still he is not at all improving; he tells

you he is better, but he is still languid, and his appetite does not return. The stools are generally clay-coloured, but this is not a necessary consequence of jaundice; they are sometimes yellow, and I have seen them of a perfectly healthy appearance. The pulse, in most cases where the fever is ephemeral, returns in a few days to its natural standard; in some instances it is remarkably slow, and this state of pulse is to be regarded as an unfavourable symptom. Sometimes there is a slight degree of subsultus tendinum and delirium; and I must observe that you are never to forget that the early supervention of nervous symptoms, in any form of this disease, is always to be looked upon with suspicion. One of the most alarming complications, however, of this gastro-duodenal jaundice, is the occurrence of coma during its progress, a symptom to which the attention of the profession was first strongly directed by Dr. Marsh. He has given several cases of jaundice, characterised by this symptom, the majority of which resisted all the ordinary resources of medicine, and terminated fatally. I must confess, too, that I have never seen a case, in which the coma was distinctly established, terminate favourably. You should, therefore, when called to treat a case of jaundice, be always on the alert, and never allow any bad symptom like this to steal upon you; and it is gratifying to think, that if you take this symptom in time, you will, in all probability, be able to overcome it.

An extremely interesting paper on this coma, occurring in jaundice, will appear in the forthcoming number of the Dublin Medical and Chemical Journal, from the pen of Dr. Griffin, of Limerick. He gives the details of some extraordinary cases, which you will find well worthy of an attentive perusal. Out of four cases *in one family*, which he attended, two died, who had become comatose at an early period; in the other two, the affection of the brain was relieved by bleeding and other active measures. From this it would appear, that the mere supervention of coma is not necessarily followed by death, but that it is an exceedingly dangerous symptom when it comes on at an early period of the disease. It is very difficult to give a satisfactory explanation of this. Some persons think that it is attributable to the action of the bile on the blood which is circulating in the brain. This explanation would answer very well if coma was a symptom of common occurrence; this, however, is not the case, and we must seek for some better reason. It is stated, by some, that coma may be one of the consequences of the close sympathy which exists between the brain and liver. Dr. Griffin draws an analogy between the effects of suppression of bile in jaundice and suppression of urine in diseases of the kidneys, and thinks that the affection of the brain is of common occurrence in one as well as in the other. This analogy, however, is incomplete, for we have no case of complete suppression of urine without fever and other violent symptoms, but we have many cases of complete suppression of bile with very slight and almost inappreciable disturbance of the economy. It is very difficult, in the present state of medical science, to explain the coma of jaundice; all we know is,

that it sometimes occurs, that it is a bad symptom, and must be met with great activity. I may mention one fact which seems to be strongly opposed to the analogy of Dr. Griffin. It will be proper to observe here, that Dr. Griffin does not advance this as an opinion, or advocate it as a theory; he merely offers it as a hint or suggestion, leaving it to others to decide the question. We are not, therefore, in examining this analogy, reasoning against any opinion of his. But with respect to this matter, the fact to which I allude is this—one of the worst cases of coma I ever witnessed, occurred in a patient who had no suppression or retention of bile; the bile flowed freely into the intestines, the dejections were distinctly tinged with it, and yet this man had deep jaundice and intense coma. We are still in want of a number of facts on this point; it is a subject which affords a large field for interesting enquiry, and Dr. Griffin deserves great credit for the philosophical and impartial manner in which he has brought his cases before the medical public.

When a patient dies of jaundice, accompanied by this comatose affection, you are naturally anxious to ascertain the cause of death. Now what you will generally find is this: on opening the head you examine the brain accurately, but cannot detect any lesion of its substance or membranes; you then go to the stomach, and discover there marks of vascularity; you open the duodenum, and find it in a state of intense inflammation. I have seen many cases of this disease in which the mucous membrane of the duodenum was highly engorged and almost black. It is said that this inflammation extends from the duodenum along the common biliary duct to the liver. I am not possessed of facts to confirm this assertion, but I have little doubt that, in the majority of cases, the jaundice is more the result of a mere lesion of innervation of the liver, than proceeding from any spread of inflammation along the ducts into its substance. Unless we can demonstrate this inflammation, it is idle to assume its occurrence. When you examine the liver, gall-bladder, and biliary ducts, you generally find them in the normal state. In a few cases, the ducts have been found impervious from adhesive mucus; you will see in John Hunter's works a case of this kind, which occurred in a consumptive patient. You will find a great number of important facts, relating to the pathology of jaundice, in the commentaries upon his own pathological propositions by M. Broussais. I would also advise you to peruse Dr. Marsh's excellent paper in the Dublin Hospital Reports.

We come now to the diagnosis of jaundice depending upon gastro-duodenal inflammation. In the first place, we learn from the history of the case that the exciting cause has been some excitant of inflammation in the mucous surface, the ingestion of indigestible aliment, or taking cold water into the stomach while the body has been overheated. The next thing is the supervention of fever with gastric symptoms, and these being followed, in two or three days, by an attack of jaundice, *without any of the ordinary signs of hepatitis*. Here we have a disease excited by taking cold water while the body is heated, or by indigestible food, preceded by febrile

disturbance with gastric symptoms, and unaccompanied by the symptoms or signs of hepatitis. When this combination of circumstances occurs, you make your diagnosis with great certainty, and set it down as jaundice depending on inflammation of the stomach and duodenum, and treat it accordingly. There are but two forms of jaundice accompanied by symptomatic fever; the one under consideration, and that which is the consequence of hepatic inflammation, or other disease. It might be supposed that the tenderness of the epigastrium was caused by an affection of the liver, but by making an accurate examination you will be generally able to discriminate with certainty. You will find that the pain is less than that of acute hepatitis, that strong pressure gives pain, not in the region of the liver, but in that of the duodenum; you can ascertain by a manual examination, and by the pleximeter, that there is no enlargement of the liver, that there is no remarkable dulness on percussion at the lower part of the chest on the right side, and when the fever is ephemeral, this will furnish you with much valuable assistance towards forming a correct diagnosis.

With respect to the treatment of this form of jaundice, in mild cases, where there is little or no fever (for fever is to be taken as a test of the severity of the disease), the patient very often gets well without any treatment, and the jaundice, after lasting a few days or weeks, goes off spontaneously. In all such cases, a regulation of diet, keeping the bowels open by mild laxatives, and prohibiting wine, spirits, and other stimulants, will be found, in general, sufficient to remove all the symptoms. I wish, however, to impress upon you that it is of *the utmost importance to cut short this disease as soon as possible*. There is no use in letting it get ahead of you; and in every case where the symptoms are in any degree acute, and there is a degree of fulness and tenderness over the epigastrium, you will be culpable, if you omit to apply leeches over the stomach and duodenum, and prescribe iced water, and every other means calculated to remove inflammation. If you allow it to go on to a certain length, if you allow fever to progress, and coma to supervene, you will not be able to manage the case so easily. Never, then, omit the application of leeches the moment you have ascertained the existence of decided inflammation. Keep your patient's bowels open by enemata, or by mild saline laxatives, regulate his diet carefully, prohibit all stimulants, and he will generally do well.

Many persons are in the habit of prescribing mercury in this disease. From my own experience I cannot say whether this is right or wrong; but I can state that I have seen a great many cases get well without it. But in cases where the symptoms are obstinate, and the stools continue white, I think you would be justified in giving mercury, even so far as to produce salivation. I must remark to you, however, that I have seen two cases in which it was found impossible to produce the free action of mercury in patients labouring under this disease. The exhibition of small doses of cream of tartar, two or three times a day, made into an

electuary with some mild confection, I have found to be an excellent remedy in the treatment of this affection. In my lecture on dysentery, I mentioned some facts which go to prove that this remedy seems to have great power in bringing down bilious discharges. In this form of jaundice I found cream of tartar extremely useful, and its exhibition is unattended with danger.

Now suppose you should meet with a case in which coma appears as an early symptom, what should your line of treatment be? Here you have to deal with a very threatening symptom, which, if neglected for any time, will, in all probability, bring on a fatal termination. You should, therefore, on its first appearance, meet it with a corresponding activity; you should immediately have the head shaved, apply leeches behind the ears, blister the nape of the neck, and act smartly on the bowels by laxatives. It was by such treatment as this that Dr. Griffin saved his patients.

I wish here to make some observations on a very remarkable form of gastro-duodenitis, which was almost epidemic in this country some years ago, at least it occurred during the existence of an epidemic fever, and we had at that time a great many cases of it in the hospital. It is a curious fact that the majority of these seemed to bear a distinct resemblance to the yellow fever of warm climates. This will appear somewhat extraordinary; but, when you have heard a statement of the facts, you will be inclined to think that these cases were nothing more or less than so many instances of the malignant yellow fever of the tropics. I shall read for you an account of the symptoms, as they were observed in numerous cases under the care of my colleague, Dr. Graves, and myself, in the Meath Hospital.

In the great majority of cases this disease was preceded by fever; in fact, all the patients who exhibited this form of jaundice had been admitted as fever patients. After a longer or shorter period, without any premonitory indications, symptoms of intense irritation of the digestive tube set in, and advanced with a fatal rapidity. Most of the patients vomited frequently; there was great tenderness of the epigastrium, and over the region of the small intestine; the tongue became black and parched; there was a violent pain in the belly, and a spasmodic affection of the abdominal muscles, which felt hard and knotted, and to which the nurses gave the name of *twisting of the guts*, a name which singularly agreed with the numerous intussusceptions found along the course of the small intestine after death. This state of suffering continued from one to four hours, and then the body became all over suddenly jaundiced. Then came another train of symptoms. With intense and universal jaundice, the patients exhibited also extreme restlessness, tossing their arms about, and regarding their attendants with a look at once expressive of nervous suffering and despair. Some raved, had trembling and convulsive fits, and were totally unconscious of every thing passing around them; others preserved their intellect to the last, but they had depicted in their countenances an agony and a despair which I shall never forget. General spasms were

frequently observed; and many, on attempting to swallow, had spasms like those of hydrophobia. There was great irritability of the stomach; many vomited frequently, and in some cases the matter ejected bore an exact resemblance to coffee grounds. The pulse became low and fluttering, the extremities cold, the face pale and shrunken, and in some the nose assumed a purple colour, giving to the patient a truly horrible appearance. This change in the colour of the nose was preceded by extreme paleness; the part, at first, appeared as if it had been frost-bitten. Broad patches of a wax-like whiteness, elevated a little above the level of the skin, and somewhat resembling urticaria, having the same temperature as the rest of the body, were found on the following day to assume a reddish colour; and on the third day the redness was converted into dark purple. The toes were affected in a similar way; and in some of these cases the parts so affected sloughed and were thrown off. There is at present in this city a woman who lost the ala of the nose, and one of the toes, in this manner.

The phenomena observed on dissection were equally remarkable. Though the tenderness of the epigastrium was very great, there was no trace of peritoneal inflammation; *neither was there, in any case, inflammation of the liver, and the gall ducts were found to be pervious in every instance.* The mucous surface of the stomach, and duodenum, and ileum, were found in every case to present intense marks of inflammation; there were numerous intussusceptions along the course of the ileum, and the spleen was found to be large, soft, and pultaceous. There was no evidence of inflammation of the brain; but in the ventricles, and at the base of the brain, there was in some cases an effusion of yellowish fluid, and the membranes had a faint tinge of yellowness. In one case I found a remarkably dry state of the arachnoid. In one severe case there was a good deal of a substance resembling coffee grounds in the stomach, and the mucous membrane was soft and disorganised.

All the phenomena of this disease, the gastro-intestinal inflammation, the yellowness of skin, the enlargement and softening of the spleen, the rapid fatality and excessive prostration, seem to point out a strong analogy between it and the yellow fever of warm climates. In the writings of Rush and Lawrence, you will find that their description of the phenomena, observed on dissection, would in a great degree answer for those of the cases which I have detailed. I may mention here, too, that in our cases the mortality was severe. We lost the first sixteen cases; and it was not until we fully ascertained the nature of the disease by dissection, that we began to save these patients. Then, by free depletions, copious applications of leeches to the abdomen, and the bold use of calomel and opium, we succeeded in a great number of cases. In some cases death took place in four, in others in six hours; in a few it was more prolonged. There is no epidemic on record in this city in which the same symptoms, and the same rapid fatality, were observed.

With respect to the analogy between this disease and yellow fever,

it appears that in the latter affection the yellow colour depends upon the presence of bile in the blood. This is one point. Again, from the most accurate descriptions which have been given of the morbid appearances of yellow fever, it appears that in the majority of cases the liver has been found healthy; here is another point. In yellow fever, also, inflammation of the stomach, duodenum, and intestines, is a matter of almost universal occurrence, as you will find by examining the works on yellow fever. In our cases we had all these circumstances; we had extreme tenderness of the epigastrium, and inflammation of the stomach, duodenum, and intestines; and in one severe case we had black vomit. All these circumstances, combined with the fatality, seem to prove that the cases which were under treatment in the Meath Hospital, during the epidemic of 1826-27, bore a very striking resemblance to that species of fever which is supposed to exist only in warm climates. It is probable that if yellow fever should appear in temperate countries, it would exhibit itself in the form of gastric fever, with some cases only of yellowness. Indeed, it seems to be now very generally admitted, that yellow fever has nothing peculiar in it; that it is the maximum of bilious or gastric fevers. We find that in proportion as we approach the warm latitudes, the digestive mucous membrane appears to take on a greater susceptibility of disease. Between the tropics it would seem as if morbid actions were chiefly thrown upon the viscera of the abdomen. Europeans, who have resided there for any length of time, acquire a yellow tinge, and many of them suffer from intestinal and hepatic inflammations. If we go northward, we find the case to be the reverse; as we approach the colder latitudes, we find the mucous membrane of the digestive tube acquires a greater degree of tone and vigour, that it is less susceptible of disease, and can bear much greater stimulation. The inhabitants of warm climates use a large proportion of vegetable food; they seldom indulge in the use of animal food or spirits. The Hindoo lives on rice, the Arab on dates and milk. But, if we go northward, we find the natives habitually using stimulating food and drink with impunity; indeed, it is wonderful to think what vast quantities of flesh, animal oil, and other stimulants, the stomach of an Esquimaux or Kamschatkan will bear without injury. There is no doubt that warm climates predispose to inflammatory affections of the digestive apparatus, and this seems to connect yellow fever with the ordinary form of gastro-duodenitis, accompanied with jaundice, or, in other words, a little more extent, a greater degree of intensity, and we may have the jaundice of this country converted into yellow fever. And it is fair to conclude that the *typhus icterodes* of temperate countries owes its danger not to the mere circumstance of jaundice existing, but to the greater degree of secondary gastro-enteritis which has produced that jaundice.

I shall now draw your attention to some other forms of jaundice. One of the most important of these is, that which arises from the obstruction of the biliary ducts by calculi. It would be foreign to

my purpose to enter into any discussion with respect to the formation of gall stones in a course of lectures like this ; I shall therefore refer you, for information as to their history and composition, to the various treatises on animal chemistry. What we have to consider at present, are the symptoms of the disease, the habit of body in which it is found to occur, and its mode of treatment. You see on the table numerous preparations of the various forms of this disease.

Gall stones are more commonly observed after the age of forty or fifty than before these periods ; they are very frequently met with in persons of sedentary habits, and hence women are more subject to them than men. They are also liable to occur in persons who eat highly-seasoned indigestible meats, and take little or no exercise. It is stated that in England five sixths of the cases of gall stones occur in females. I do not know whether this proportion be exact, but the fact is established that they are more common in females than men. Biliary calculi may be found in three different situations, either in the substance of the liver, or plugging up the biliary ducts, or filling the gall-bladder. Here is a preparation, exhibiting the gall-bladder almost obliterated by the pressure of a number of those calculi within its cavity. Here is another specimen. You see the gall-bladder is contracted, and nearly filled up with biliary calculi ; it also appears to be atrophied and reduced in size. Here is a remarkable specimen. You observe the gall-bladder, which is rather large, is completely filled with a vast calculus ; its coats are also thickened, probably the result of inflammation. Here is another preparation of the gall-bladder, containing two moderately sized calculi.

Gall stones, when lodged in the substance of the liver, or in the gall-bladder, may remain for a long time, and accumulate prodigiously, without producing jaundice. This has been frequently proved by the fact, that on opening the bodies of persons who have not had during life the slightest symptom of jaundice, the gall-bladder has been found completely filled up with these productions. But when any cause determines the passage of one of these bodies into the ducts, and that it is too large to pass freely, then the symptoms of icterus begin to make their appearance. We do not know what it is that produces the attempt to discharge small biliary calculi through the ducts, but it is during this process that the dreadful symptoms of what has been by some called *hepatic colic* are observed, and, supervening on these, the rapid occurrence of jaundice. Under such circumstances, a train of phenomena presents itself, very different from that which characterises the jaundice depending on inflammation of the stomach and duodenum. The patient is suddenly attacked with violent pain in the epigastrium and right hypochondrium. The stomach sympathises, and we have nausea, cardialgia, and vomiting ; the patient's sufferings are dreadful, and he refers his pain to the region of the gall-bladder. The abdominal muscles are thrown into spasmodic contractions, there are often convulsions and fainting fits, the extremities are cold, the

body is bathed in perspiration, and the pulse is often hard and contracted, but seldom accelerated. This is a very remarkable symptom. Heberden says, that the pulse not being in quickness above the standard of health, with a sudden attack of pain in the region of the epigastrium, are diagnostics of this affection. "I have seen," says he, "a patient in this disease rolling on the floor in a state of violent agony, which I could not allay with nine grains of opium, and yet the pulse was as tranquil as if he was in a calm sleep." I can confirm the truth of this observation from my own experience. Here are the diagnostics; the pain is more intense than that which attends any form of inflammation, and yet the pulse is perfectly quiet; it occurs in persons not generally subject to spasmodic attacks; it is not preceded by constitutional symptoms; and is rapidly followed by jaundice, and absence of bile in the stools. Under these circumstances you may make a certain diagnosis.

Sometimes a tumour is formed in the right hypochondrium, which rises above the edge of the liver, and gives a feeling of distinct fluctuation, marking the situation of the distended gall-bladder. In such cases as these, the calculus is in the common duct, and the bile descends into the gall-bladder, from which it cannot escape, thus causing the distension of that organ. This may go on until the distension becomes so great as to increase the size of the gall-bladder to such a degree, that, in some cases, it has been known to contain a pint of fluid; and cases have occurred in which it has burst, and effused its contents into the peritoneum, causing violent peritonitis and death. This termination, however, is fortunately of very rare occurrence. I believe that some of the cases in which rupture occurred, were those in which an emetic was given; and hence it is that many practitioners are afraid to give an emetic where this state of the gall-bladder has been ascertained, or is strongly suspected.

LECTURE XIV.

Diagnosis of jaundice from biliary calculi—Proof of the passage of the calculus—Indications of treatment—Rupture of the gall-bladder after the use of emetics—Spasmodic jaundice—Treatment of spasmodic jaundice—Discharges of fatty matter—Researches of Drs. Bright and Elliotson—Connection with malignant disease examined—Source of fatty matter.

We were occupied at our last meeting, in considering the symptoms of that disease in which there is a formation of what are termed biliary calculi; the passage of these into the common biliary duct; the possible strangulation of the duct for some time, and the consequent production of jaundice. I described the symptoms of this disease as consisting in a sudden and violent attack of pain in the

region of the gall-bladder, succeeded sooner or later by the phenomena of jaundice, and in the generality of cases *occurring without fever*. Between these violent attacks the patient sometimes has intervals of complete ease; at other times a gnawing sensation continues in the original situation of the pain. It is remarkable, however, that a patient may have an interval of perfect ease between the fits, somewhat similar to the calm which occurs during the pains of labour. The occurrence of this cessation of intense suffering has been attributed to the passing of the stone into the duodenum; this, however, is by no means certain. The idea generally entertained upon this matter is that each attack of pain corresponds with the passage of a stone. How far this notion may be true I cannot decide; but this I shall impress upon your attention, that the mere subsidence of pain is no proof of the removal of the disease, *unless bile is discharged by stool or by vomiting*; but when such a discharge coincides with the cessation of pain, you may be sure that the obstruction has been overcome for the time. I need not remark to you that the smaller the calculus is, the greater the facility with which it will be discharged. You will find in some cases that the efforts which nature makes to remove one of these concretions are quite unavailing; it lies in the gall-bladder or duct, and there remains impacted. Here its presence sometimes excites inflammation, lymph is thrown out, and the duct becomes permanently closed; in other cases it has been found to make its way into the duodenum by ulcerative absorption, and is thus discharged.

The size of biliary calculi is various. Generally speaking, their dimensions are similar to those which you see before you; but there are many cases on record of very large ones having been discharged. In the twelfth number of the *Medico-Chirurgical Transactions*, Dr. Brayne gives an instance of one passed, which was three inches long and three and a quarter in circumference. I may however mention, that there is a source of doubt connected with this case. It is possible that the calculus in this instance was nothing more than one of those fatty covered secretions which are found in the intestinal tube, and which have nothing to do with the gall-bladder or its ducts. As it is my intention to return to this subject, I shall here only observe, that fatty matter has been frequently discharged in hard as well as soft masses, that it sometimes cuts like a biliary calculus, and that it may be difficult for a mere physiologist to distinguish concrete masses of this kind from gall stones.

The passage of a biliary calculus does not of necessity imply the occurrence of jaundice; if it passes without difficulty there is none; if it happens to become impacted, then jaundice is sure to follow. It is a curious fact, that of this form of jaundice cases have occurred in which the flow of bile into the digestive tube has been obstructed for more than a year, and yet a recovery took place.

Permit me now to rehearse the diagnosis of jaundice from biliary calculi. Sudden and violent pain in the region of the gall ducts, increased by pressure, but generally unaccompanied by acceleration of pulse or fever, coming on in a person not subject to spasmodic

attacks, and speedily followed by jaundice. This is the diagnosis. In most of the cases described in books, and, I believe, in the majority of instances, you will find the disease to exist without febrile symptoms; but it is also true that it may be complicated with febrile disturbance, and under such circumstances you should be apprehensive of inflammation in the biliary ducts or duodenum. The importance of this will appear when you come to consider the treatment.

Now, suppose you are called to attend a case of this kind. A person of sedentary habit, who indulges in highly seasoned food and takes no exercise, gets a sudden attack; he lies, perhaps on the floor, writhing in agony; he is beginning to exhibit the yellow tinge of jaundice; he refers his pain to the region of the gall-bladder; his pulse, however, is quiet, and he has no evident symptoms of fever. Here the nature of the disease is manifest, and the first thing you have to consider is, what are the indications of treatment. These are obviously threefold. The first is to guard against inflammation; for you are aware that inflammation may take place, and besides, the higher the irritation and (if I may so term it) the spasm of the gall ducts are, the greater will be the difficulty in passing the stone. The next thing is to allay spasmodic pain. We know that this pain is principally spasmodic, or nervous, because it is always more sudden and violent than that which attends common inflammatory action, and, moreover, it is commonly uncomplicated with symptoms of inflammation. The third indication is to adopt measures to favour the passage of the stone. Now these three indications, but more particularly the second and third, are, as you may perceive, reducible to one form of treatment. Whatever will relieve pain and spasm will assist in favouring the passage of the stone. If, then, you happen to meet with a case of this affection in a strong robust constitution, where the pain is violent and is aggravated by pressure, and particularly where there is any sign of febrile disturbance in the system, I would advise you to bleed such a person immediately. Not that you have to combat actual inflammation, but because you have to prevent the liability to it, and because, in using the lancet, you are employing a most powerful antispasmodic. The next thing of importance, in severe cases, is the application of leeches over the region of the gall-bladder, and the same remarks apply to leeching as to venesection. You are not to suppose that the application of leeches will cure the disease; but you may be sure that it will assist materially in allaying spasm, and favouring the passage of the calculus. The bowels should be freely acted on by purgatives and enemata; you may give a brisk purgative by the mouth, and at the same time a purgative enema. After the bowels have been opened, the only thing which you can rely upon for giving relief is opium, and that in full doses. I have seen several patients labouring under this disease who appeared to me to be maltreated. The different measures for procuring relief were certainly put into practice, but not in a regular or proper manner. They first got a dose of opium, then a purgative, and lastly were bled.

If you have a case of this kind to treat, bleed first, then leech, next employ purgatives, and when you have emptied the bowels, have recourse to opium. I have never employed the anodyne injection in this disease; but, reasoning from analogy, I am inclined to think that it would prove serviceable, and I am aware that it has been employed with effect in that form of jaundice which depends upon hysteria. The tobacco injection also seems to have strong claims to our notice, and in this disease must prove extremely useful, from its powerful effect in reducing spasm.

There is a difference of opinion with respect to the employment of emetics. The object of their exhibition is to force the calculus through the ducts, by the shock given by the sudden and violent contraction of the abdominal muscles, and also to relieve spasm, by their subsequent relaxing effect. Some practitioners of high authority, however, state that this practice is not unattended with danger, and give cases of rupture of the gall-bladder after the exhibition of an emetic. Such an accident as this would be very likely to injure for ever the character of a professional man. I am sure the practice, in some cases at least, is dangerous. A distinguished medical friend of mine has related to me the particulars of a case of this kind, in which the exhibition of an emetic was followed by rupture of the gall-bladder and fatal peritonitis. In this instance the case was not so deplorable, so far as the patient was concerned; he was labouring under extensive disease of the liver, and only exchanged a lingering for a sudden death; but this furnishes no excuse for a medical practitioner. If I were to hazard a conjecture, I would say *that emetics can be employed with safety only in the early stage of the disease, when there is no obstruction from organic disease*; for the longer the jaundice has lasted, the greater is the chance of obstruction from organic disease. Again, you should never use them *where there is evidence of a distended gall-bladder*. If you can feel the tumour formed by the distended gall-bladder, in the right hypochondrium, you may be sure something has been going on for a long time, and you should be cautious in giving an emetic. Never use it then where you can feel a tumour in the region of the gall-bladder. If you give it at all, give it in the early stage, and after premising venesection, leeching, and the use of the tobacco injection. I had almost forgot to mention that very signal advantages accrue from the use of the warm hip bath in this disease. I have seen cases in which the most extraordinary relief was obtained by applying twelve leeches over the region of the gall-bladder, and then placing the patient in a hip bath.

Sometimes it happens that the symptoms return again and again. Here you cannot repeat the venesection; you must employ leeches, the hip bath, warm fomentations, opium, and every thing calculated to relieve pain and spasm. Watch your patient carefully, guard against inflammation, and if any inflammatory symptoms of the duodenum arise (but this is rare) take proper measures to obviate them.

A few words now with respect to what has been termed spasmodic jaundice. This form of the disease occurs independent of inflammation of the stomach or duodenum, and independent of disease of the ileum, brain, or liver. It appears to be an essentially spasmodic disease, but the situation of the spasm has not as yet been accurately determined. It is supposed to exist, either in the gall-bladder, or in the biliary ducts, or in the duodenum. If the biliary ducts and gall-bladder do not possess muscular fibres, we must place it in the duodenum; but whatever may be its seat, it presents the characters of a spasmodic disease. It seems to be excited by the same cause, and yields to the same treatment as other spasmodic affections. It generally occurs in hysterical females, and in hypochondriac and nervous persons, and disappears under treatment calculated to allay nervous excitement. Its exciting causes seem to be chiefly sudden and violent mental emotions, or the taking of a quantity of indigestible food; and it frequently terminates by the discharges of flatus upwards and downwards. It resembles, in a certain extent, the last-mentioned form of jaundice, but differs in two particulars; first, the pain is relieved by pressure, which generally increases it in the former species. Dr. Pemberton, in his *Treatise on the Diseases of the Abdominal Viscera*, dwells strongly on this point. The second peculiarity is, that in this disease the attack is more sudden. In the case of jaundice from gall-stones, the patient has some degree of pain and uneasiness before the violent symptoms appear; but in this form they exhibit themselves in a sudden and unexpected manner. The disease, too, is accompanied with hysterical or convulsive symptoms, and there is sometimes a copious flow of limpid urine. All these circumstances are important in forming a correct diagnosis.

The best treatment for this spasmodic jaundice is, after acting on the bowels by warm purgatives, to use fetid enemata, and prescribe a mixture composed of ether, castor, and ammoniated tincture of valerian and opium, which are of the greatest use when the bowels have been opened. In this form, as well as that which we have been lately considering, the fact is, that if you expect any good from opium, you must not give it until the bowels have been opened. Opium and antispasmodics have, I am convinced, often lost their character for utility, from being given at a time when the exciting causes of disease are still present in full energy; and the failure of these powerful auxiliaries is to be attributed to the neglect of proper measures for reducing intense irritation. In the spasmodic jaundice, tobacco injections would be likely to produce beneficial effects. Generally speaking, however, you will not find it necessary to have recourse to such a vigorous remedy, as the disease is most commonly observed in delicate females, and yields readily to milder treatment. Indeed, it will often disappear spontaneously, and without any apparent cause.

The last form of this disease which we have to consider, is jaundice connected with an affection of the brain; and this is a very interesting and curious subject. I shall not, however, enter upon

it at present, as I intend to reserve my observations on this point until we come to treat of diseases of the nervous system. I have alluded to this variety on a former occasion, and referred you to Dr. Marsh's paper on jaundice in the Dublin Hospital Reports, in which you will find several cases of it which came on as the result of disease in the head. Broussais admits that it is dependent on and secondary to cerebral disease; but he thinks there is another link in the chain of connection, and that this is duodenitis. He believes that we have irritation, first in the brain, next in the duodenum, and then jaundice. Several practitioners of great authority, on the other hand, assert that the cerebral affection produces jaundice at once, without the intervention of duodenal inflammation. In the present state of medical science we cannot determine this point.

A few observations now with respect to the discharge of fatty matter from the bowels. The reason why I introduce the subject here is, because it has been frequently observed in connection with jaundice and disease of the upper portion of the digestive tube. In the last number of the *Medico-Chirurgical Transactions*, a great mass of interesting matter has been published on this subject by Dr. Bright, Dr. Elliotson, and Mr. Lloyd. I shall give you a short analysis of these papers; and I wish to impress this upon your recollection, that when you go into practice the study of this affection would form a subject worthy of your investigations; and that any attempts on your part to clear up the difficulties which complicate this singular form of disease will be advantageous to the cause of science.

Dr. Bright gives three interesting cases of this disease. In these the discharge was in the form of oil or semi-concrete matter—it floated on the top of the *fæces*, and had a fetid odour. There was also in these three cases a remarkable similarity in the pathological phenomena. The first case exhibited symptoms of jaundice, diabetes, enlarged liver, and discharge of fatty matter: on dissection, the liver, pancreas and duodenum were found diseased. The second presented symptoms of jaundice and disease of the liver, in addition to the fatty discharge; on dissection the liver was found healthy, but there was a similarly diseased condition of the duodenum and pancreas; there was malignant disease in both. Nearly the same symptoms were observed in the third case, and after death disease was found in the pancreas and small intestine, and the pylorus was in a state of extensive ulceration. In all there was chronic disease of the pancreas and duodenum terminating in jaundice, from obstruction of the gall duct, and accompanied by discharges of fatty matter from the bowels. Here are three cases in which there is an extraordinary similarity in the symptoms and pathological appearances. Dr. Bright is inclined to think that these discharges may be connected with disease of the pylorus and duodenum, but particularly with malignant affections of the pancreas, and gives the particulars of some cases, in which disease of the pancreas was suspected, and in which, from the absence of this symptom, he was

induced to give a contrary opinion, which, on dissection, turned out to be correct.

Mr. Lloyd's case resembles those detailed by Dr. Bright, inasmuch as it presented the phenomena of jaundice with obstruction of the gall ducts, disease of the head of the pancreas, and contraction of the duodenum. So that you see we have here four cases in which there was disease of the duodenum and disease of the pancreas, together with the occurrence of jaundice. I may, however, mention one fact, which you should be acquainted with; in Mr. Lloyd's case the pancreatic duct was found to be obstructed by calculi.

Dr. Elliotson commences his paper by alluding to that peculiar substance called ambergris, which is frequently washed ashore by the tide in several countries, and which is supposed to be a morbid production from the intestinal canal of the *physeter macrocephalus*, or spermaceti whale. The quantity found in the intestinal canal of this animal is said to be enormous, and instances are mentioned, in which this substance was found to amount to 182 lbs. in the body of one of these animals. Dr. Elliotson proceeds to give cases from the records of medicine and from his own experience, in which a fatty discharge took place in the human subject. Of this he quotes cases from Mællenbrochus and Mæbius in the *Ephemerides*, but one in particular from the works of Fabricius Hildanus, which I shall briefly recount. "A pious matron of Hilden had been for a long time subject to severe pain in the stomach, which became at length much worse, when one day the pain extended all over the abdomen, and after very severe pain and suffering, she discharged about three pounds of fat, which was of a pure quality, had no smell, and was preserved by her for many years." This woman recovered perfectly. Dr. Scott, of Howick, mentions the case of a servant girl who had been treated with purgatives and injections, under the supposition that her disease was colic, and who, after two or three days' suffering, discharged a quantity of fatty substances, about the size of nuts, beans, and peas, which burned like fat when thrown into the fire; this patient also recovered. Dr. Babington gives another case, which had been mentioned to him by Sir E. Home, in which we find that a lady who had been suffering, as it was supposed, from gall stones, happening to take castor oil draughts to open her bowels, passed a quantity of fatty matter. Another case is detailed by Mr. Howship, where a lady who had been attacked with pain, jaundice, and fever, passed a quantity of this substance with the subsidence of those symptoms. The fatty matter in this case was discharged after the lady had taken a pint of olive oil, upon the recommendation of Dr. Simpson of New Malton. Dr. Turner, of St. Thomas's Hospital, mentions the case of a female who laboured under an hysterical distension of the belly, and who passed quantities of this substance, specimens of which are preserved in the Hunterian Museum.

Sometimes these fatty discharges are found in the concrete, some-

times in the semi-fluid form. Dr. Elliotson mentions the case of a patient who had phthisis, diabetes, and discharge of fatty matter; thus he was at the same time passing fatty substance, large quantities of saccharine urine, and spitting up pus and softened tubercular matter. Between all these, and the agonising pain which he suffered, he became in a short time completely exhausted and sank rapidly. The fatty matter discharged in this case was shown to Dr. Prout and Mr. Faraday, and Dr. Prout stated he could not distinguish it from human fat when heated. Tulpius is quoted by Dr. Elliotson as relating a case where *fat was discharged from the bowels and bladder*. Here is the quotation:—"But what do we say of Margaret Appelmania, an innkeeper, who, in her seventieth year, passed precisely the same fat, both from the intestines and the bladder, and likewise without fever, emaciation, or colliquative excretion. Towards the close of the disease, however, she did become feverish, and, in consequence, so emaciated, that death found her little else than a juiceless dried up corpse." A case similar to this was communicated by Mr. Pearson to Dr. Elliotson. The symptoms were suppression of the biliary secretion, and a copious discharge of *oil from the bowels and bladder*, which, it is stated, formed good soap when mixed with alkali. Dr. Prout has observed fatty matter passed with the urine, and considers this symptom as an indication of the probable supervention of malignant disease of the kidneys and bladder. The last case is from the *Annali Universali*, which is quoted by Dr. Johnson in the *Medico-Chirurgical Review* for July. In this case the patient, after fasting for a considerable time, took a quantity of indigestible food. On the evening of the same day he had an attack of vomiting; at first blood was thrown up, and then he ejected this fatty substance to the enormous amount of thirty pounds. There was, in this instance, a sudden and extraordinary emaciation; the patient was so reduced in the space of a few hours, that the skin hung in loose folds about him. He recovered in twenty days; but with great loss of bulk.

Let us enquire now what is the nature of this symptom. Is this fatty matter a morbid secretion from the liver, from the pancreas, from the mucous membrane of the stomach, or from the intestines? There are facts to show, that in certain cases this disease cannot be explained by a reference to any of these circumstances. It seems plain, too, that Dr. Bright's suggestion of referring it to malignant disease of the duodenum and pancreas, and the diagnosis which he would seem to found upon it, cannot stand here; for the symptom upon which he attempts to establish a diagnosis—a discharge of fatty matter—occurs in persons who have recovered from the disease. We cannot suppose that they have been labouring under malignant disease of the duodenum and pancreas when they have recovered; and that a recovery may take place is proved by Dr. Elliotson's cases. It is quite probable, however, that if the irritation, or whatever it be that produces this discharge, should continue, it may bring on fungoid and malignant disease; but that

the discharge of fatty matter is significant of the actual existence of such a condition is not borne out by these facts. Well, are we to look upon this discharge as a secretion from the liver? I think we cannot, because we have seen that in Dr. Bright's three cases the biliary duct was obstructed by disease of the duodenum and pancreas. I may mention, too, that in some cases where a dissection was made, the liver was found perfectly healthy, and the gall-bladder in its normal condition, full of pure bile. Taking this and the foregoing fact into consideration, we have proofs that this fatty substance, in some cases at least, cannot come from the liver. Does it proceed from the pancreas? It would more naturally come from the liver than the pancreas, for the liver does actually secrete a certain quantity of fatty matter; but there is no substance of this kind found in the secretion of the pancreas, which is considered to bear a strong analogy to that of the salivary glands. Besides, in the case mentioned by Mr. Lloyd, where the duct of the pancreas was obstructed by calculous secretions, this fatty matter has been discharged; and hence we cannot, I think, refer it to the pancreas. Whence, then, does it come? Is it a secretion from the surface of the intestines? This is a question which it is hard to determine. We do not yet know, nor have we ever met with that state in which lesion of structure in the mucous membrane of the intestinal canal has been followed by a discharge of fatty matter. We have discharges of serum, lymph, blood, and pus, from the surface of the intestines, according to the nature of the disease; but we know of no pathological condition as the result of which fatty matter may be produced. Again; cases of every known form of disease in the liver, pancreas, and intestinal canals, occur without this discharge at all. In the present state of medicine, the probability is that this discharge is the result of a sort of metastasis of the secretion of fat from the other parts of the body in which it is usually deposited, to the surface of the digestive tube, where it is poured out somewhat in the same way as in cholera; the fluids of the body are rapidly absorbed and eliminated by the intestinal canal. This supposition, without attempting to bring it forward as the true solution, furnishes us with the best explanation of the case. In the case of the patient who discharged this substance by stool and with the urine, the emaciation came on rapidly, as if all the fat of the body had been absorbed and carried out of the system; here, too, the fat was discharged from another mucous surface. In the other remarkable case, where a vast quantity of this substance was thrown up by vomiting, the emaciation was so great that the patient's skin hung in loose folds about him. When we reflect, too, that there is no recognised disease of the intestines, liver, or pancreas, to which this discharge can be referred, we cannot help believing that it is the result of a metastasis in the secretion of fat.

The next point in this matter which we have to consider is, what is the best mode of treatment? This question, I believe, cannot be answered at present; nor can our practice be any thing but empirical until we have more light thrown upon the subject. With a

view to increasing our knowledge, I beg of you to make this disease the subject of your practical investigations, and to have a look out for this discharge, because I believe it often occurs unnoticed, from our neglecting to inspect the evacuations.

LECTURE XV.

Acute and chronic hepatitis—Pathological differences—Effect of climate—General and local symptoms—Character of fever—Pain of shoulder—Use of pleximeter—Complication with jaundice—Resolution—Abscess—Various openings of the latter—Cicatrisation.

I propose to-day to draw your attention to the subject of inflammation of the liver. This is the disease which you meet with in books under the general name of hepatitis; but it is of great importance to distinguish between acute and chronic hepatitis for this reason—acute hepatitis implies something specific, an organic change, the nature of which is well known and accurately defined; but chronic hepatitis implies nothing of this certainty of the nature of organic change, inasmuch as there is no single one of the recognised disorganisations of the liver, which may not, and have not occurred, with chronic hepatitis as an existing cause, or a prominent symptom. When we speak of acute hepatic inflammation, we speak of a disease, of which the structural lesions are sufficiently understood; but when we treat of chronic hepatitis, we treat of a disease in which there may be a great variety of organic changes. Chronic irritation of the liver may in one patient be followed by the development of hydatids; in another by cancer, or tubercle; in a third, by hypertrophy of one or both of its elementary tissues; in a fourth, by atrophy; and in a fifth, by abscess; so that, under the chronic form of hepatitis, we may have many different lesions comprised. Under the acute form, we have only vascularity, softening, yellow degeneration, and suppuration. These, which are the ordinary results of acute hepatic inflammation, are the same as the results of active inflammation of other parenchymatous organs.

It is an interesting fact, and connected with the predisposition to acute diseases of the abdominal viscera in warm climates, that acute hepatitis is much more prevalent in those countries than it is here, and this is particularly true with respect to the East Indies. You recollect, in one of my lectures, I alluded to the greater susceptibility to disease, the extraordinary nervous excitability of the digestive mucous membrane in warm latitudes, and hence that a large proportion of the diseases of those climates was characterised by the predominance of inflammation in the stomach and intestines. The same thing occurs with respect to the organs which are connected with the digestive tube; and hence it is that diseases of the liver and spleen are so frequently met with between the tropics.

A very remarkable fact, bearing on this point, has been mentioned to me by Staff-Surgeon Blest. He states that, in the East Indies, hepatic disease in animals is no unusual occurrence; that *animals brought to India from more temperate climates are peculiarly subject to it*; and that in them it is a common cause of death. He has seen many cases of hepatic abscess in dromedaries and horses, under these circumstances; a fact of great interest, when considered with the liability to *tubercle* in animals brought from *warm climates* to these countries. In these countries, acute hepatitis in its highest degree is a rare disease; in fact, so rare, that it is only in our own time that any thing like a series of cases, by which you could compare the disease in these countries with a similar affection in others, have been published. A series of cases by Louis, and another by Dr. Graves and myself, published some time since, are all that we have on the subject. It is, somewhat extraordinary that a sort of epidemic tendency to acute hepatic inflammation, and the formation of abscess, occurred in the countries about the middle of the year 1828. Up to this period, abscess of the liver was looked upon as a very rare disease in Ireland; a case of it was met with in hospital once perhaps in twelve months or two years; but at the period to which I allude, almost every great hospital in Dublin had several cases; and in the Meath alone we had a great number, out of which seven or eight proved fatal.

We have now to consider this acute inflammation of the liver; and first, with respect to the symptoms. Were I lecturing on pathology merely, I would commence with the organic changes; but as I have chiefly kept in view, during my present course, the practice of medicine, I shall begin by detailing the symptoms. You will get a good idea of the symptoms of acute hepatic inflammation by dividing them into local and general; by doing this, you will simplify the matter, and acquire accurate and defined notions of the disease. Now, the local symptoms are, pain in the region of the liver, tenderness over the affected organ, and a degree of tumefaction perceptible to the touch; pain, tenderness, swelling—here are the local symptoms. What are the general? Inflammatory fever, and lesion of the digestive function; and, in addition to this, if the case be severe, you have functional derangement of the respiratory and cerebral systems. You have, then, in a case of acute hepatitis, the general symptoms of inflammatory fever, with lesion of the digestive function; and if the case be severe, of the respiratory and even cerebral systems, the local symptoms being pain, tenderness, and tumefaction.

Now, with respect to the character of the fever which accompanies this disease, it is in all cases nearly the same; and here we come to an interesting and curious fact. You recollect that, in speaking of gastro-enteric inflammation, I alluded to the nature of the accompanying fever, and stated that it was (commonly) of a low character, and that there were no local inflammations in which the fever was so often typhoid as in the affections of the gastrointestinal surface. This, I believe, has been one great cause of the

ignorance of medical practitioners with respect to gastric and enteric inflammations; they have been most commonly looked upon as cases of typhus, and treated accordingly. In acute hepatitis, however, we do not observe this typhoid prostration. Though closely connected with the gastro-intestinal system, the liver does not, in its acute inflammatory state, produce the same manifest depression of the vital powers. On the contrary, we have, in the early period of the disease in this country, high inflammatory fever, hot skin, and full bounding pulse; a state in which few would be afraid to employ the lancet with boldness. Patients labouring under acute inflammation of the liver, generally have high sympathetic fever, a full, strong, and accelerated pulse, with the local symptoms above described; and, in addition to these, we frequently observe bilious vomiting, considerable thirst, derangement of the bowels, and scanty high-coloured urine. The tumefaction is more or less evident, and when this is accompanied by severe pain, there is considerable difficulty of breathing, a circumstance which sometimes occasions this disease to be mistaken for pleurisy. There are two remarks to be made on this subject. In the first place, it sometimes happens that acute inflammation of the liver and of the lower part of the lung occur at the same time, particularly where inflammation attacks the diaphragmatic surface of the liver. Here you frequently have an extension of the inflammatory process to the corresponding surface of the pleura, or the two diseases co-exist from the first. Under such circumstances, disputes, as to which organ is engaged, are often unnecessary. Again, in the early period, and when the attack is acute, the diagnosis of inflammation of the diaphragmatic surfaces of the liver, or pleura, is comparatively of little consequence, as both demand the use of calomel and opium, leeches and the lancet; and, in the early stages at least, both are amenable to the same treatment. But it is not so in the chronic stage of either. Here the diagnosis is of great importance; and when I come to treat of pleuritis, I shall draw your attention to some researches of mine on this subject, which I hope have set this question at rest.

The pain which accompanies acute hepatitis varies much in situation. Sometimes it is felt in the shoulder, sometimes under the short ribs, sometimes in the loins, and frequently in the epigastrium. You have all heard of pain at the top of the shoulder as a common symptom of liver disease; in fact, so common as to be looked upon by some as a pathognomonic symptom. I believe that a great deal too much stress has been laid on this circumstance. It is now discovered, that so far from being a constant, or even a common, symptom, it is one which is of exceedingly rare occurrence. I have never seen a case of acute hepatitis with pain in the shoulder; I have sometimes observed it in chronic, but never, to my recollection, in acute cases. Andral states that it is very seldom met with; Dr. Mackintosh says the same, and, if I recollect aright, looks upon it as a symptom not worth enquiring about. Now, I have seen some medical men who considered this pain in the

shoulder as a diagnostic of such value, that if it happened to be absent they concluded there was no hepatic disease. The fact is, that it is any thing but constant. You may have it in some cases, particularly of chronic hepatitis, and not in others; besides, it frequently depends upon other causes—for instance, upon pneumonia of the top of the right lung, or it may be caused by incipient phthisis, aneurism of the arteria innominata, or right subclavian artery, and other diseases. It is of very little consequence whether it be absent or present; and the only reason why I dwell upon it is, to show you its real value as a symptom.

There is one remarkable circumstance connected with the pain of an acute hepatitis. In one case, you will find that the pain is very acute and constant, in another, that little or none is felt; and when you come to investigate the cause of this after death, it generally happens that, in cases where the pain was violent, the inflammation existed on the surface of the liver, and in those where little suffering was experienced, deep in the substance of that organ. This is a curious fact; but it may be looked upon as an illustration of a general law, *that if we consider inflammatory affections of the solid viscera, we shall find that the more superficial the inflammation the more painful it is; and, on the other hand, the more deep-seated it is the more is it latent, so far as pain is concerned.* Thus: if you take a case of inflammation of the substance or central parts of the brain, you will find that the disease is to be recognised often not by pain, but by the lesions of the sentient and locomotive powers; whereas, in inflammations of the membranes, on the surface of the same organ, one of the most prominent symptoms is agonising headache. In the next place, go to the lung; take a case of deep-seated pneumonia, and contrast its almost painless character with the lancinating torture of an acute pleuro-pneumony. In pneumonia the pain is dull, and scarcely complained of; but pleuritis unaccompanied by acute suffering is extremely rare; in fact, where you have the signs of inflammation of the parenchymatous tissue of the lung, with sharp pains in the chest, you may very safely make the diagnosis of pleuro-pneumony. The same absence of pain is by no means unusual in inflammatory affections of the mucous membrane of the intestines; but if the inflammation should chance to extend to its peritoneal investment, you will have this state rapidly exchanged for one of intense suffering. So it is with respect to the liver; disease on the surface of that organ is attended with severe pain; but enormous destruction of its deep-seated parts may take place, and your patient complain merely of a sense of uneasiness.

A late author on hepatic affections, Dr. Bell, who has written a treatise on the diseases of India, describes two forms of acute hepatic inflammation, which are different as to their seat and character. In one of these, which he terms *sero-hepatitis*, the disease is on the surface of the liver; in the other, which he terms *puro-hepatitis*, it exists in the centre. In the sero-hepatitis, he states that the patient is attacked with sudden pain in the region of the liver, and

this is so severe that even the weight of the bed-clothes is insupportable; the patient cannot bear to turn or lie on his left side, from the pressure exerted in that position on the inflamed organ. But the deep-seated, or puro-hepatitis, may go on in such a latent manner, that the first symptoms you have of the existence of liver disease are those which mark the occurrence of suppuration. Neither the patient nor his medical attendant will have reason to suspect inflammation of the liver, until the constitutional and local symptoms of the suppurative process direct attention to that organ. Such are the statements of Dr. Bell, which I believe to be correct, as they are supported by the concurrent testimony of many persons who have practised in India, with whom I have conversed on this subject. Mr. Annesly makes the same assertion; and such was our experience in the succession of cases of hepatic abscess which were under treatment in the Meath Hospital during the year 1828.

The next symptom which we have to consider, is the tumefaction of the liver, and this is one of considerable importance. In order, however, to estimate the extent of this tumefaction with any degree of accuracy, you must take one preliminary step, and that is, to have the bowels fully evacuated. If the intestines are filled with feculent matter or gas, you cannot do this in a proper manner. A few hours before you make your examination, give the patient a full purgative draught, assisted, if necessary, by a strong purgative enema. In this way, you empty the belly of collections of feculent matter and aeriform fluid, and then you can with certainty and satisfaction ascertain the extent of the swelling. You will then be able (when your patient is laid in bed), perhaps, to see at once the extent of the tumefaction, particularly where the parietes are not thick or loaded with fat; at all events, you will be able to feel it with your hand, and in every case you can ascertain it by mediate percussion with the pleximeter. I do not know any more important adjuvant, in making out the diagnosis of an enlarged liver, than the use of mediate percussion. For instance, suppose you have a patient labouring under acute hepatitis, and that the tenderness of the organ is so great that he cannot allow you to make the requisite degree of pressure to ascertain the extent of the swelling; take the top of your stethoscope, apply it over the region of the liver, make use of light percussion, and you will find, with the greatest accuracy, how far the tumefaction of the liver extends, by the dulness of sound heard over the inflamed organ, and exactly limited to it. In this way, you can make a most satisfactory examination, without giving your patient any pain; and this is a matter of some importance, as you will meet with many cases in which there is exquisite tenderness, and where the patient will not bear the slightest pressure. I would advise you, therefore, to practise this mode; it gives little or no pain, it is exceedingly simple, and I have not the slightest doubt of its accuracy. Now, the value of this tumefaction, as a sign of the existence of hepatic inflammation, depends very much on the recent nature of the attack. If a man, who was in perfect health a few days back, complains of pain in his right side, and

has a tumour in that situation, it is to be presumed that this tumour does not depend upon the presence of a collection of fluid in the pleura, and, consequently, that the tumefaction is not produced by an empyema. Then, if, in connection with fever, and pain in the right side, you can ascertain the existence of a tumour in the region of the liver, and that it has occurred within a short space of time, you may be pretty sure that it is not an empyema, but an inflamed and enlarged liver.

Jaundice has also been considered as a symptom of hepatic inflammation, but it is one which is by no means constant. Again, you may have most extensive hepatitis, with slight jaundice, and universal and intense jaundice, with trifling or no hepatitis; and, what is equally singular, you may have very little perceptible disease of the liver with scanty secretion of bile; and, on the other hand, the liver may be burrowed with abscesses, and at the same time you find bilious stools, and after death the gall-bladder may be found filled with pure healthy bile. I thought, at one time, that I could explain the presence or absence of jaundice in cases of hepatitis, by supposing that, where it occurred, the jaundice was the result of inflammation of the gastro-duodenal mucous membrane; and to prove this, I drew up a table of cases, of which one half were complicated with jaundice, and the other not. I found, however, that in a great number of cases, where the tube was free from disease, the hepatitis was complicated with jaundice; and in a similar number of cases, where the same circumstances were observed, the tube was in a state of disease. So that we may have, as you perceive, hepatitis and jaundice, with and without disease of the intestinal tube; and whether we look to the cases of hepatic inflammation, unaccompanied or complicated with jaundice, the state of the gastro-intestinal mucous membrane throws, as yet, no light on the subject. It appears, then, that the occurrence or non-occurrence of gastro-duodenitis does not explain why it is, that in one case of hepatic inflammation jaundice is a prominent symptom, and in another is completely absent.

In some cases of acute inflammation of the liver, the natural secretion of that organ seems to be totally annihilated. A curious case of this kind occurred under the care of Dr. Graves, in the Meath Hospital, where the slightest trace of bile did not exist in the gall-bladder, which was filled with a transparent mucus. In some instances you will find plenty of bile discharged, in others none; in some patients the stools are observed to be clay-coloured, or very faintly tinged with bile; in others they are healthy, and natural in colour, as well as consistence. From our own experience, and from studying the series of cases published by Louis, we have come to the conclusion, that neither the presence nor the absence of bile in the stools affords any positive or useful information as to the different stages of this disease, its progress or termination.

Acute hepatitis terminates in a variety of modes. It may terminate by resolution—here the organ returns to its former healthy state, without any appreciable change of structure or function; it

may terminate by the formation of matter—here we have suppuration and abscess; it may terminate in gangrene; and, lastly, it may, without the occurrence of suppuration or gangrene, pass into chronic hepatitis, of which the result may be a variety of morbid changes in the organ itself. When the patient is so fortunate as to meet with the first of these terminations, the fever, pain, and tumefaction, gradually disappear. On making an examination with the pleximeter, you will find that part of the belly which was rendered dull by the tumefied liver becomes clear on percussion; you will find, also, that the dulness of the lower part of the chest, on the right side, is removed, the patient can breathe without any difficulty, and lies on the affected side without inconvenience. But when the disease passes into the suppurative stage, the train of phenomena exhibits a marked difference. What we generally observe under such circumstances in this country is, that there is a change in the constitutional symptoms; the fever, which has been hitherto inflammatory, now becomes hectic. The pulse continues quick, but is diminished in strength and volume; the countenance becomes pale and collapsed, the patient feels languid, restless, and disposed to sweat, and his perspiration has a sour smell. He may also have a miliary eruption, and this continues for some time, with an increase or persistence in the size of the hepatic tumour. When these symptoms appear, there is every probability that matter is forming, or has been already formed. The patient then begins to complain of increased weight in the region of the liver, and in some cases the integuments over that organ are swollen, and slightly discoloured. I have observed that, in some instances, the pain concentrated itself in one point, and in this situation it was afterwards found that abscess had formed. These are the ordinary symptoms which usher in, or accompany, the suppurative stage of hepatic inflammation; but there are also cases, even in this climate, where this marked change of symptoms is not seen, and where abscess forms rapidly, and with symptoms which might be supposed to belong to the early period of the disease. This, however, is particularly true with respect to hepatic abscess in the East Indies.

I believe I mentioned in a former lecture a very curious fact, namely, that it has been often found impossible to salivate persons labouring under hepatic abscess, so that the presence of matter or not, in the liver, may be determined by the circumstance of the patient being susceptible or not of the full effect of mercury. The liver, in this case, seems to illustrate that pathological law which I alluded to in speaking of dysentery; that the more intense an inflammation, the greater is the difficulty of producing ptyalism. My friend, Staff-Surgeon Marshall, and also Mr. Annesly, agree in stating, that it is exceedingly rare to find a case of hepatic abscess in which the salivary glands have been affected by mercury, and our experience of the disease in this country exactly coincides with their opinion. It has been also observed, that hepatic abscess may form in an insidious and latent manner, when it happens to be complicated with disease of other organs. This affords us an illustra-

tion of a law already laid down, that the more complicated an affection is, the more obscure is its character. Again, we may, as the result of acute hepatitis, have one or two vast cavities formed in the substance of the liver, or we may have a number of very small abscesses. I recollect a case which occurred some time ago near this city; the patient exhibited the symptoms, and was, in fact, supposed to labour under intermittent fever. After some time, death took place, and, on dissection, a number of small abscesses were found in the liver, of which, during life, there was no symptom, except that which I have just mentioned.

When an hepatic abscess attains a certain magnitude, it has a tendency to burst and discharge its contents. If it escapes externally, it makes its way in a great variety of directions, sometimes in the epigastric, sometimes in the hypochondriac, sometimes in the lumbar region, and there are cases on record, in which the matter has burst in the right axilla, by a sinuous passage beneath the integuments of the chest. When it bursts internally, it sometimes perforates the diaphragm, and gets into the cavity of the pleura, or, what is more commonly the case, into the substance of the lung. The matter of an hepatic abscess very rarely gets into the pleural sac, and hence we very seldom have an empyema as the result of this occurrence, because the pleura being extremely liable to adhesion as a consequence of the inflammatory process, and the passage of matter being always preceded by inflammation, the opposed surfaces of the pleura become glued together by coagulable lymph, which prevents the hepatic pus from getting into the pleura, at the same time that it favours its passage into the lung. The opening into the lung is one of ordinary occurrence; many cases of it are on record; and serious as the lesion may appear, it is, perhaps, one of the best modes in which hepatic abscess may terminate by internal opening. Many persons have recovered after such a termination; and I have seen myself three cases in which it was certain, and a fourth in which it was probable, that the matter had been expectorated by the mouth, with a favourable issue. We are, then, as far as the records of medicine and our experience in the Meath Hospital go, warranted in looking on this termination as a favourable one. Hepatic abscess may also open into the pericardium; but this is very rare, there being only one case of this kind, which is given by an American author. It may open into various parts of the intestinal canal, the stomach, duodenum, and colon; it may also discharge its contents into the right kidney, into the vena cava, or into the peritoneum, and thus cause violent peritonitis and death.

The diagnosis of these different openings of an hepatic abscess is easy, and founded on the same principle, the occurrence of new and extraordinary symptoms, connected with the adjacent viscera, which were not before diseased—symptoms of a sudden discharge of pus from, or into, these organs. Suppose you have a case of hepatic abscess, and that, during the progress of the disease, the patient has sudden and enormous expectoration of purulent matter, without any preceding signs of inflammation of the lung, it is probable that

the abscess has opened into the lung; or suppose that, during an attack of acute hepatic disease, your patient is all at once seized with nausea, and vomits a quantity of purulent matter, and, immediately after this, you perceive that the tumefaction of the liver subsides. Here the matter has been discharged into the stomach; in other cases you have it discharged into the duodenum or colon. Again, you may have instances where the matter gets into the peritoneum; here you may observe the occurrence of rapid peritonitis. So that, in all cases of this kind, the diagnosis is founded on the same principle, *the occurrence of discharge of pus from, or into, organs which previously had been considered to be in a healthy state, and this coinciding with a subsidence of the original tumour.*

In persons who, under such circumstances, recover, it is natural to expect that cicatrifications should exist in the liver. Louis states that he has never seen this; with respect to our cases of hepatitis, we can only say that the fatality of the disease has afforded us no opportunity of investigating this point of morbid anatomy. Mr. Annesly, however, in his work on the diseases of India, has given drawings exhibiting this appearance. I recollect one case of a man in the Meath Hospital, who had been a soldier in the East India Company's service, and had been treated for liver disease; this man died of phthisis, and, on dissection, the surface of the right lobe of the liver was found puckered, forming a hollow with a cartilaginous basis, strongly resembling what we might suppose to be the cicatrix of an abscess.

LECTURE XVI.

Diagnosis of the rupture of hepatic abscess—Pulmonary openings—Case of double opening—Puncture of the gall-bladder—Gangrene of the liver—Its connection with hepatic apoplexy—Diagnosis of distended gall-bladder—Its causes—Inflammation of the parietes over the liver—Sympathy of the integuments.

I broke off at my last lecture while engaged in considering the phenomena of hepatic abscess, and you will recollect I spoke of the various modes in which these abscesses may open internally, and stated that the diagnosis in all cases was founded on the same principle, which is this—that during the prevalence of symptoms indicating the existence of suppuration of the liver, some new organ becomes *suddenly affected*, the nature of the affection being what would be produced by the sudden rupture of an hepatic abscess and a discharge of pus into some of the neighbouring viscera, and this coinciding with the disappearance, more or less, of the original tumour. Now, when we consider the various internal openings of an hepatic abscess, we find that they admit of being divided into two classes, first, those in which the matter is effused into cavities

having a communication with the exterior of the body, as the lung, digestive tube, and kidney. Here, in addition to the symptoms already alluded to, we have a sudden discharge of pus from the stomach or bowels, from the lungs, or by the urinary passages. But we may also have the matter discharged into shut cavities having no external communication, as where the contents of the abscess open into the peritoneum, pleura, or pericardium. You will readily perceive that of these two classes of openings, those in which the matter escapes into cavities having no communication with the exterior are the most unfavourable. The confined pus excites violent and generally fatal inflammation, and we have a dangerous empyema, a rapid peritoneal inflammation, or intense pericarditis.

I stated, that of the internal openings of an hepatic abscess, one of the most favourable is that in which the matter is discharged into the right lung, and I described briefly the mechanism of this curious process. We are warranted, I think, in declaring this to be a fortunate termination, because there are many instances on record of persons having recovered under such circumstances. A very near relative of mine presented an example of this. He was attacked with symptoms of acute hepatitis, for which he was attended by some of the most eminent physicians in Dublin. His treatment was bold and vigorous; he had free bleeding, both general and local, mercury, and every other means calculated to remove inflammation, but all proved ineffectual. His pulse became rapid; he began to sweat; the hepatic tumour increased in size, and presented a distinct sense of fluctuation; there could be no doubt of the existence of suppuration in the substance of the liver. One morning he was suddenly seized with a violent fit of coughing, and during the course of the day expectorated more than a large tea-cupful of pus; towards evening this increased, and on examination it was found that the tumour was remarkably diminished. The expectoration continued during the whole night, and in the morning it was observed that there was scarcely any appearance of the hepatic swelling. It was singular, and tends to confirm the idea that the matter had been discharged into the lung, that in the erect position this gentleman had scarcely any expectoration, but in the horizontal it was always extremely copious; a circumstance which you can easily understand by considering, that in the recumbent posture the purulent matter would find a more easy passage into the lung. In this case, it would appear that the communication between the liver and lung was very free, for I remember that on one occasion by making pressure over the liver, he said I was forcing the matter into his chest, and the pressure was followed by an instantaneous and copious expectoration. This frequently occurred. A medical friend of mine residing in Dublin, mentioned to me some time since the case of a large robust drayman, addicted to whiskey drinking, whom he attended for an attack of acute hepatitis. At a time when the liver was very much increased in size, and well-marked symptoms of suppuration present, he observed that sudden expectoration of pus took place, which conti-

nued for several days, with manifest subsidence of the hepatic tumour and complete recovery. Three cases of this kind came under my notice in the Meath Hospital. One of the patients had symptoms such as I have before described as exhibiting a striking similarity to yellow fever, from which he recovered, and was discharged, with no other remarkable symptom but quick pulse. Shortly afterwards he returned, complaining of pain in the right hypochondrium, with rapid pulse, profuse night sweats, and a slight cough. At first his appearance struck me as being characteristic of phthisis, and under this impression I repeatedly examined the chest by the stethoscope and percussion, but could not detect any lesion. The man had only a slight cough, and this was totally insufficient to account for his symptoms. The nature of the case was soon manifest: one morning the patient stated that he felt as if something had given way in his chest during the night, and he was from that time expectorating *large quantities* of purulent matter. On examining the lower portion of the left side, I found that it sounded completely dull on percussion, and that the physical signs of an accumulation of fluid in the bronchial tubes were extremely distinct. That this dulness was the result of the effusion in question is proved by the previously healthy state of the lung. The very day before I had carefully examined this part of the chest, and found it quite healthy. There was not the slightest resonance of voice in this portion after the accident, because the tubes were so completely filled; so that in this case the return to health was accompanied by *increase of broncophonia*, a fact that sets the question of the nature of the accident at rest. It may appear strange that in this case the puriform matter entered the left lung instead of the right; but this is sometimes the case, particularly when the abscess forms in the left lobe of the liver.

I shall now draw your attention to the particulars of a case which I look upon as almost unique, and which derives additional interest from the accuracy of the diagnosis. It is of great importance that you should have clear ideas on the subject of hepatic abscess, for, though the disease is not of common occurrence in this country, still, if called on to pronounce an opinion on a case of this kind, the least difference in the quantity of your information may be of consequence. The patient, who was the subject of this disease, was admitted into the wards of the Meath Hospital in August, 1828. The history of his case was, that he had been labouring, some time previously, under obscure symptoms of an hepatic affection, accompanied by slight fever and jaundice, which had gradually subsided. Three weeks before admission he stated that he had irregular fits of shivering, followed by sweating, and when he came to the hospital he complained of sickness of stomach, but particularly of cough and difficulty of breathing, which were extremely harassing, and said that he came in chiefly to be cured of his cough. He was considerably emaciated, and looked pale and low, but his stools had a natural appearance. On considering the history of his case and the symptoms then present, it struck me that it was either hepatitis

with suppuration, or empyema of the right side with irritation of the liver. At that time I had not made my researches on the diagnosis of empyema, and I must confess that I experienced a great deal of difficulty in determining the nature of the case. I found the right side considerably dilated, with dulness on percussion over its inferior half, but the intercostal spaces were not distended, and preserved their natural appearance. The case went on in this way for some time. Permit me to draw your attention for a moment to this point. Dilatation of the right side may result from the pressure exercised upon it by a solid or by a fluid mass. If the mass be solid it will push the ribs outwards, but the intercostal spaces will still preserve their natural appearance. But if the protrusion of the side be the result of pressure by a fluid mass, the intercostal spaces will be acted on even more than the ribs, and the sulci, which mark their situation, will be effaced. Now, in this case the intercostal spaces were evident, and from this circumstance I determined that it was a liver disease. The patient continued for a fortnight without exhibiting signs of any material change, and then the tumour increased very much in size, but there was no appearance of pointing. At this time the patient was visited and examined by a number of medical men, and all agreed that it was a case of deep-seated suppuration of the liver. Under these circumstances it was thought advisable to make an incision through the integuments down to the peritoneum, as recommended by Dr. Graves, and to keep the wound open by filling it with lint. This operation was performed, and the wound kept open for several days, but no matter came. On the sixth day the patient began to sink, his face became hippocratic, his extremities cold, and every one thought he was dying. During the course of the day it was observed that there was a circumscribed tumour, with a distinct sense of fluctuation, situated close to the wound, and towards the right side of the mesial line. Here is an important stage of the case;—a man presenting evidence of suppuration in the liver has an operation performed on him to favour the exit of pus externally, and some time after this we find a circumscribed fluctuating tumour, nearly in the situation of the wound. We concluded that the hepatic abscess was pointing in that situation, and it was determined to pass a lancet cautiously into the tumour. This was done, but to our astonishment, instead of pus pure bile escaped through the incision. It was clear that we had mistaken a distended gall-bladder for an abscess, and this I need not tell you was a serious error. It is singular, however, that the accident was not followed by any bad consequences. About two hours after the operation the patient went to stool, and passed two large evacuations, consisting chiefly of a vast quantity of purulent matter. Next morning he was surprisingly well, *and the hepatic tumour had considerably diminished*. His countenance recovered its natural expression, his spirits were quite elated, his pulse had become tranquil, and the liver was manifestly returning to its ordinary dimensions. He began to sit up, was put upon generous diet, could walk about the

ward, and was talking of leaving the hospital. From the period, however, at which the discharge of pus took place he had an obstinate diarrhœa, and though he took a great deal of nourishment he was still pale and emaciated. Twenty-two days after the subsidence of the tumour, another swelling began to make its appearance in the epigastrium, which increased daily, and it was obvious that another abscess was forming in the left lobe. About a fortnight after this he was suddenly seized with excruciating pain in the epigastrium, followed by symptoms of peritonitis. The tumour in the epigastrium subsided, but the patient sank in a few days of the peritoneal inflammation. Let me recall the circumstances of this case. First, we have obscure signs of the existence of abscess, then the sudden escape of matter from the bowels, accompanied with subsidence of the hepatic tumour; in the next place a persistence of diarrhœa and emaciation, and, lastly, we have a new tumour in the epigastric region, disappearing on the supervention of symptoms of acute peritonitis. From a consideration of all these circumstances I stated to the class that I should expect to find evidences of the abscess in the right lobe, which was the first affection, and I ventured to say, that the opening between it and the intestinal tube was still pervious. I was led to form this opinion from observing the persistence of the diarrhœa, to check which all the ordinary remedial means had failed. This was the first part of the diagnosis. In the next place I stated my belief that the gall-bladder had been punctured, but could not explain why the bile had not escaped into the peritoneum. Thirdly, I said that an abscess had formed in the left lobe, which had discharged its contents into the peritoneal cavity. All this was stated publicly, and on consideration you will find that there was no great difficulty in making the diagnosis. On dissection, we found a cavity in the right lobe with a small quantity of matter in it, and having a free communication with the duodenum. The fundus of the gall-bladder was found adhering to the parietal layer of the peritoneum, and the mark of a lancet wound in it was evident. A recent abscess was discovered in the substance of the left lobe of the liver, from which the matter had escaped into the peritoneum by a passage capable of admitting a small quill. Every part, therefore, of the diagnosis of this case was perfect, and borne out by the necroscopic appearances. You will see the details of this very interesting case in a paper published by Dr. Graves and myself, in the fifth volume of the Dublin Hospital Reports.

This case is exceedingly interesting, because it illustrates two remarkable terminations of hepatic abscess: in one instance, by opening into a cavity which had an external communication, in the other, into a shut sac. The patient recovered from the first abscess, and would have done so effectually if the fistula had closed (no uncommon event); but he could scarcely have recovered from the second, because, where the matter escapes into the peritoneum or pleura, the patient almost invariably dies of acute inflammation of these cavities. This case derives additional interest from the

circumstance of the gall-bladder having been opened. I believe this is the only case on record in which an opening made into the gall-bladder has not been followed by fatal consequences. I might detail many other cases of hepatic abscess, but I must at present refer you to the paper already alluded to, in which we have published the results of our experience on the subject.

Some authors have mentioned gangrene, or mortification of the liver, as one of the modes in which acute hepatic inflammation may terminate. It is now however agreed, that this is one of the rarest terminations we can meet with; in fact, that there is hardly any organic disease which so seldom occurs. Mr. Annesley states, that in all his dissections (and these were very numerous) he never met with a case of gangrene of the liver. Andral, who has examined some thousands of bodies, has only met with a single case: this, with another which was under the care of Dr. Graves, and appears to have been a genuine example of mortification of the liver, are almost the only cases of which I have any distinct recollection. The case under Dr. Graves was that of a patient in Sir Patrick Dun's Hospital, who laboured under chronic inflammation of the liver, with ascites, jaundice, swelling of the lower extremities, and an incapability of lying on the left side. After this man had been about eleven days in the hospital he began to complain of tenderness and pain of the belly; he was next seized with vomiting, and threw up a large quantity of fetid matter. Soon after this he sank, and, on dissection, numerous marks of chronic disease were found in various parts of the substance of the liver; but in the left lobe there was a cavity which was distinctly gangrenous, and had in the centre of it a large mass of slough. I think that there can be no doubt that in this case the disease was actual gangrene of the liver. I think, too, it may be very fairly doubted whether gangrene of the liver is the result of inflammation, properly so called, in any case; and I believe it would be a very interesting subject for enquiry, to consider how far this disease may be the result of hepatic apoplexy, or effusion of blood into the substance of the liver. This is an accident to which the liver, as well as every other parenchymatous organ, is subject; and though effusions of blood into its substance are by no means so common as similar occurrences in the brain and lungs, still it does not enjoy any thing like immunity from such lesions. We have good reason to believe, that in many cases blood effused into the substance of parenchymatous organs may, under certain circumstances, either undergo putrefactive decomposition and form a gangrenous abscess, or that, although no longer circulating in its vessels and effused into the parenchyma of an organ, it may still retain its vitality to a certain extent, and, being modified by the powers of life, may give rise to the formation of various morbid products. In this way it is thought that various tumours—cancerous, steatomatous, melanotic, and encephaloid—may originate. I am inclined to think that this sometimes occurs in the brain and lungs, and it is probable that it may happen in the case of the liver also. Further researches,

however, are necessary, with respect to the elucidation of this matter, before our opinions on it can possess a higher character than that of verisimilitude.

While on the subject of hepatic abscess, it will be necessary to allude to one of its occasional complications—distended gall-bladder—because this may be mistaken for the pointing of an abscess, and an operation be performed, and that this has happened more than once is a positive fact. A distended gall-bladder has been mistaken for the tumour formed by the pointing of an hepatic abscess, an opening has been made into it under this supposition, bile has escaped instead of pus, and this getting into the cavity of the peritoneum, has given rise to rapid and fatal peritonitis. A remarkable case of this kind has been detailed with great candour by the late Mr. Todd, in one of the early numbers of the Dublin Hospital Reports. He was called suddenly to visit a girl, whom on his arrival he found to be in a dying state, labouring under great distension of the belly, almost insensible, moaning constantly with her jaw fixed, and presenting a distinct tumour in the hypochondriac region, which, from the history of her case, he was led to consider as an hepatic abscess pointing externally. He divided the integuments and muscles down to the peritoneum, and having introduced a trochar, drew off nearly three pints of *bile*, with apparent relief. Shortly afterwards violent peritonitis came on and the patient sank rapidly. After death the liver was found to be healthy, and the tumour to have been formed by a distended gall-bladder of enormous size. From this, after the operation, the bile had escaped into the peritoneum, causing intense and universal peritonitis. In making a diagnosis in such a case as this, every thing will depend upon your knowledge of the history and previous symptoms. The circumstances which produce distension of the gall-bladder, you will find upon examination do not bear any distinct resemblance to those which precede or accompany inflammation of the substance of the liver. We may have it from the obstruction caused by biliary calculi, and here you can make a tolerably sure diagnosis. We may have it from disease of the duodenum, or of the head of the pancreas, or from the pressure of aneurismal tumours in the vicinity. Abscess of the liver is generally accompanied by symptoms of inflammation of that organ, but distension of the gall-bladder does not present any corresponding train of phenomena. There may be some exceptions to this rule, but in making the diagnosis we must strike a balance of probabilities. The first part of our diagnosis then is this—the occurrence of a tumour in the hypochondriac region, not preceded or accompanied by any of the symptoms which characterise hepatic inflammation. Another important diagnostic, and which I think will apply in several cases, is this. In a case where abscess was formed in the liver, the fluctuation, which is a sign of the existence of fluid, is often preceded by a condition of the part in which there is no sign of the presence of fluid; we have first induration and swelling, and *then the signs of fluctuation*; but this is not the order of succession in the phenomena which charac-

terise distension of the gall-bladder. In abscess we have a hard tumour which gradually softens; in case of distended gall-bladder we have the tumour soft and fluctuating from the commencement. If, then, we have a tumour in the hypochondriac region, not preceded or accompanied by symptoms of hepatic inflammation, accompanied by jaundice, with a sense of fluctuation from the beginning, and unattended by hectic, the chances are indeed very great that it is not an hepatic abscess, but a distended gall-bladder.

You will perhaps be surprised, that, in treating of the diagnosis of distended gall-bladder, I do not lay any particular stress upon position. The reason of this is, that the situations in which a distended gall-bladder may be felt are extremely various. First, we may have it appearing in different parts of the hypochondrium, under the cartilages of the ribs. In the next place, we may have it between the cartilages of the ribs and the spine of the ileum. It has been observed by Andral in the iliac fossa, and he has seen it in the epigastric region. In a case which occurred in the Meath Hospital, it presented itself in the epigastrium, a little to the right of the mesial line. Again, in severe cases you may have the whole of the liver filled with bile, *and having a distinct fluctuating feel, not produced by the existence of pus in that organ, but from the enlargement of its ducts, which are gorged with bile.* In one case mentioned in the Medico-Chirurgical Transactions, this curious circumstance occurred. So far, then, as diagnosis is concerned, position appears to be of very little consequence; but when we have this, in addition to the other circumstances mentioned, it will tend to give additional certainty to our diagnosis. In all cases on record where there was distended gall-bladder, the patient laboured under jaundice, except in that which I have detailed in the early part of this lecture; but perhaps if our patient had lived longer, he would also have had jaundice.

There is one disease more which may be, and I believe has been, confounded with acute hepatitis and abscess of the liver. This affection, which has not been sufficiently noticed by authors, is inflammation and abscess of the abdominal parietes over the hepatic region; and this is a very singular disease. It is sometimes trifling, but I have seen a patient die of it. With the original nature of this disease I confess that I am not at all well acquainted; nor can I say whether the inflammation first attacks merely external parts, or whether it is a primary affection of the liver, and that the external parts take on diseased action from sympathetic irritation. In such cases we frequently observe many of the symptoms of inflammation of the liver, as pain, tenderness, biliary derangement, foul tongue, and morbid stools, with a tumefied state of the integuments. After these symptoms have continued for some time, the tumour increases in size, becomes softer, and matter forms. You give exit to the pus by opening the abscess with a lancet, and the patient gets well. This occurrence I have frequently witnessed. From a consideration of all the circumstances, it strikes me that in this disease the first morbid action in all probability commences in the liver itself,

and that the external inflammation is an example of the strong sympathy which subsists between disease of deep-seated parts and integuments which cover them. Of this fact you have several illustrative instances. In pleuritis we frequently find the integuments of the chest remarkably tender on pressure; and in cases of inflammation of the brain the integuments of the scalp have their sensibility much increased. The same thing occurs in hepatitis; and in this disease one of the first distinct symptoms is this tenderness of the superincumbent skin. Now, you can conceive that, if this morbid sensibility of the investing parts should increase, in place of having some pain and tenderness, accompanied by swelling, we may have suppurative inflammation set up in these parts; and that, under such circumstances, the inflammation may leave the internal organ where it first existed, and be thrown upon the external parts in its vicinity. It strikes me that this is not unfrequently the case in this curious affection. In the case of this disease which I have seen prove fatal, the following circumstances were observed:—evident symptoms of inflammatory fever; pain and tenderness in the region of the liver, followed by the appearance of a tumour; which became fluctuating, was opened, and a quantity of matter discharged with considerable relief to the patient. She left the hospital, but returned again in about a fortnight or three weeks, with an enormous tumour in the same place, which was again opened, and a vast quantity of purulent matter evacuated. Though the matter continued to flow out freely, she did not recover strength; and on enquiry it was found that before her second admission she had spit up some blood. One day, while dressing the abscess, the gentleman who attended her observed that when she coughed air passed out through the wound, proving the existence of a fistulous communication with the lung. On examination after death we found an abscess, the base of which rested upon the peritoneal surface of the liver, without engaging its substance. From this the matter had made for itself a double passage, one externally, the other through the diaphragm and pleura into the substance of the lung. This was the only case in which I have seen this disease prove fatal; and in it death appears to have been caused by the extent of the disease, and by the abscess opening into the pleura and lung.

LECTURE XVII.

Aneurism of the hepatic artery—Distension of the liver with bile—Treatment of hepatitis—Employment of mercury—Symptoms of suppuration—Dr. Graves's operation for giving exit to matter in hepatic abscess—Rupture into the peritoneum—Chronic hepatitis—Complication with disease of the heart—Embryonary state of the liver.

You may remember, in one of my past lectures I alluded to a case of aneurism of the hepatic artery, of which I had procured a preparation: to-day I shall be able to exhibit to you the morbid appearances in this very remarkable case. It would appear that aneurism of the hepatic artery is an exceedingly rare circumstance. At a late meeting of the Academy of Medicine of Paris, a specimen of aneurism of the hepatic artery was presented to the society; and that celebrated pathologist, Cruveilhier, stated that it was the first of the kind he had ever seen. I wish to bring this preparation before you, not merely from the interest which its rarity excites, but also because the disease, in this instance, produced that distended condition of the gall-bladder to which I drew your attention on a former occasion, and which, in this case, was recognised before death. The gall-bladder formed a distinct pyriform tumour, situated a little above the iliac fossa, and the patient was deeply jaundiced. I shall state, from recollection, what I know of the details of this case. The patient was brought into the Meath Hospital, labouring under jaundice, which he stated to be of some days' standing. He was thin and weak, and when questioned respecting his age, he said he was thirty-five, but he appeared to be upwards of fifty. His habits he described as being uniformly temperate and regular. Some years before he had suffered from an attack of apoplexy, but after this had enjoyed good health, until the occurrence of the present illness, which began with vomiting of blood, and which continued for some days and then yielded to medical treatment. He now experienced a loss of appetite, became quite dyspeptic and constipated; he also began to lose flesh, and under these circumstances applied at a dispensary, where he got various remedies without any benefit. Some time after this he observed, on getting up one morning, that his arms and legs looked rather yellow; on the following day he had a decidedly bilious tinge with yellow vision, and in this state he entered the Meath Hospital. On admission he presented symptoms of general jaundice; the urinary secretion was deeply coloured; the skin, eyes, and nails yellow; the stools white and without any trace of bile. On examining the abdomen, the liver was apparently greatly increased in size; in the epigastric region there was a tumour of considerable dimensions; and in the iliac fossa we observed a separate pyriform tumour, which could be traced up to the edge of the enlarged liver. I mentioned at that time to the class, that there was something about the case which I could not understand. The disease was of inconsiderable standing; the patient had, a short time previously, been in a state of good health, and yet, reasoning

from analogy, this hepatic tumour could only have occurred as the result of chronic disease. It must have been the consequence of disease more or less chronic, and yet the history of the case was at variance with the idea of its chronicity. After some time the patient got miliary eruption, then petechial spots; he continued in a low and weak state, and nothing did him any good. On the morning of the day of his death he did not appear worse than usual; he answered our enquiries respecting his health in his ordinary manner; in the evening he sat up in bed gasping for breath, with a look of extreme distress; he then leaned back on his pillow and expired.

On opening the peritoneum we found a vast quantity of blood effused into its cavity, and my first impression was that it was aneurism of the abdominal aorta. On closer inspection, the aorta proved healthy, and the aneurismal tumour was found to be connected with the hepatic artery; this had ruptured close to the gall-bladder, and its contents had been effused into the cavity of the peritoneum. We now found that the cause of the jaundice had been the pressure which this tumour had exercised on the biliary ducts. In consequence of the obstruction to the flow of bile, the ducts of the liver were dilated to an enormous extent; some of them were capable of admitting the largest sized finger. This dilation affected not only the larger trunks, but even extended to their most minute ramifications, even up to the surface of the liver; and here we found that the biliary tubes were dilated into sacs, some of which were as large as a hazel-nut. When these pouches were punctured the bile gushed out freely. A similar condition of the ducts has been noticed by Mr. Lloyd as existing in connection with obstruction of the biliary duct, from disease of the head of the pancreas, in his paper on Discharges of Fatty Matter from the Bowels. (See *Med. Chir. Trans.*) I have got the preparation of this singular disease before me, and I regret that in one respect it is defective, inasmuch as it does not show satisfactorily the condition of the biliary ducts. A portion of the preparation which exhibits this appearance I gave to Dr. Houston, the curator of the Museum at the College of Surgeons, and I am sure that he will give admission to any gentleman who is anxious to examine it. This preparation, gentlemen, is too large to send round. It exhibits the hepatic artery with its aneurismal tumour, and the opening by which the artery communicates with the aneurismal sac. Here is the place in which the rupture took place, and here is the gall-bladder greatly extended and thickened in its coats.

Here, then, we have a new cause of jaundice, where the disease is the result of the pressure of an aneurismal tumour of the hepatic artery—a cause which has hitherto been unnoticed by writers on jaundice. The great interest of this case consists in this, that dissection explained the difficulty which I felt in making the diagnosis at first, for it showed that the hepatic tumour was formed, not by an hypertrophied, but by a distended and displaced liver. It proved that it was formed, not by a process of chronic growth, but

by the rapid formation of an aneurismal swelling and the consequent obstruction of the gall-bladder, accompanied by distension of the liver itself. With recent symptoms, then, we had, in this case, *an enormously large liver, not the product of acute inflammation, but of distension of all the biliary ducts up to their most minute ramifications, and arising from mechanical obstruction.* As far as it goes, this case appears to me to be perfectly unique.

Let us turn now to the treatment of acute hepatitis. It is unnecessary for me to say, that in all cases of acute visceral inflammation, in the healthy subject, the first consideration is blood-letting, either general or local. In the early period of acute hepatitis, all authors have agreed in strongly recommending the use of the lancet; and there can be no doubt that when the disease is in its early stage, and the patient robust, the practitioner who omits employing these measures must be culpably negligent. It should always be borne in mind that the liver is an organ of paramount importance to life. There are two circumstances, also, which are in favour of bleeding in the case of an acute hepatitis—there is less chance of its being complicated with typhus fever, and general bleeding exercises a powerful influence over the acute inflammations of parenchymatous organs. Hence we bleed with greater advantage in a case of acute hepatitis than in the inflammation of mucous membranes. Our first bleeding should be large, and such as will make a decided impression, and it will frequently be necessary to bleed a second and even a third time if the disease be very acute and the constitution strong, taking care to diminish the quantity at each successive bleeding, and to watch its effects. I have here to make one remark—that general bleeding is not the same heroic remedy, nor has it the same decided influence in arresting acute hepatic inflammation, as in checking pneumonia. A copious detraction of blood has, under favourable circumstances, often succeeded in completely removing an attack of pneumonia, and the patient has recovered without the employment of any other remedial measure; but acute hepatitis is seldom or never cut short in this way. Still venesection is of the greatest importance; and if it were performed merely with a view of preparing the patient for leeching and other depletive measures, its advantages would be unquestionable. I would recommend you, therefore, when you meet with a case of hepatitis in the early period, first to bleed freely, or in such a manner as to make a decided impression on the symptoms; next, to empty the bowels by prescribing a purgative draught, assisted by an enema; and, lastly, to cover the region of the liver with leeches. You will find great advantage in employing your therapeutic means in this order; for if you begin with leeches before you have had recourse to venesection, or the use of purgatives, your practice will not be so scientific, nor will your success be so complete. Bleeding, purgation, leeches, and the application of cupping glasses over the leech-bites (if necessary) will give you breathing time; and, after the lapse of twelve or fourteen hours, you will find that all symptoms of urgent danger

will have passed away. During the progress of the case, the remedy which I should principally rely upon is local bleeding, frequently repeated. If you apply thirty leeches to-day, I would not have you repeat them to the same amount to-morrow; but you might, perhaps, apply fifteen or eighteen, and the next day ten or twelve. By proceeding in this way you will find a great abatement in your patient's symptoms; and I know of no circumstance which, taken singly, proves the value and benefit of your treatment so well as the diminution of the hepatic tumour, which you can accurately and satisfactorily ascertain by means of the pleximeter. When you find a gradual subsidence of swelling, I think you may be pretty sure that, even though the other symptoms exhibit little or no improvement, the hepatitis is on the decline, and will soon be removed entirely.

You have all, I am convinced, heard a great deal of the use of mercury in hepatitis; and there appears to be in the minds of most medical men a strong connection between mercury and all diseases of the liver. So far has this impression gone abroad, that to some practitioners it would appear perfectly heterodoxical to think of attempting to cure an hepatic inflammation without this accredited panacea. I must however confess that it is my belief that several cases of hepatic inflammation may be cured without it; and, if this be true, as I am convinced you will find by experience, it is so much the better for the patient. I do not mean to depreciate the value of this powerful remedy in making this assertion;—it is undoubtedly a useful adjuvant, but it is only an adjuvant. It is decidedly secondary and inferior to general and local antiphlogistics, followed by counter-irritation; and you should always bear in mind, that if you wish to bring about the full action of mercury on the system, you must precede its employment by means calculated to reduce the intensity of local inflammation. By premising general bleeding, leeching, and purgatives, you give the mercury an opportunity of exerting a decided influence on the salivary glands; and in such cases it is that the most unequivocal advantage is derived from it; for, as I have observed in a former lecture, salivation appears often to be the *result* of the reduction of inflammation to a certain degree, and not its cause.

In all cases of hepatitis occurring in delicate females, but particularly in persons of low, scrofulous constitutions, endeavour to dispense with the use of mercury if possible. You will have considerable difficulty in divesting yourselves of early prejudices, and combating those of others; but when you have an opportunity of acting for yourselves, I would have you make trial, and you will find that many cases are curable without mercury. If, after having regularly and carefully employed the means recommended, you perceive that two or three days pass without any improvement in your patient's symptoms, and that the hepatic tumour remains undiminished, then indeed you may have recourse to mercury. But if you have been so fortunate as to have struck a decided blow in the commencement, and that the case is going on well, I would

ask, why should you expose your patient to the misery and danger of salivation? I am not by any means opposed to the employment of mercury in cases of liver disease; on the contrary, if we compare inflammation of the lungs, brain, and liver, with respect to the power which it has over each, I believe that it is much more applicable to cases of hepatic inflammation than it is to either pneumouia or cerebritis.

There is nothing more common than a complication of disease of the liver with disease of the upper part of the digestive tube; and here you will find that calomel will frequently cause great irritation of the bowels, vomiting, and increase of fever. Under such circumstances, you must omit the internal use of mercury, and have recourse to frictions, directing your patient to rub in a dram of camphorated mercurial ointment every six or eight hours until the gums are affected. A very good auxiliary means is to place a dram of the mercurial ointment in the patient's axilla, and leave it there; the action of the arm will, to a certain extent, answer all the purposes of friction. Dr. Graves is much attached to this mode. Where you have employed blisters, you may cut off the cuticle, and dress the raw surface with mercurial ointment. This also will contribute materially to produce the intended effect on the system. With respect to blisters, the same rules are to regulate their application as I have mentioned before, when speaking of the treatment of gastro-enteritis, namely—that they are not to be used until active antiphlogistic treatment has been employed; for it is then, and then only, that the stimulus of a blister can be useful. I believe it is seldom necessary, or even safe, to apply a blister before the third or fourth day in cases of acute inflammation of the liver. The physician who purges to-day, and blisters to-morrow, and bleeds next-day, is a very injudicious practitioner indeed; he should bleed first, then purge; and having by these means reduced the symptoms of active inflammation, he may proceed to the use of blisters with advantage.

It is unnecessary for me to remind you that you must enjoin a strict antiphlogistic diet in all cases of acute hepatitis. Recollect the powerful influence which all dietetic stimulants exercise, not only over the digestive canal and general system, but also over the liver; bearing this in mind, you will, for the first few days, keep your patient on a water and slop diet, and then on mild farinacious food and chicken-broth.

But suppose that after all this, after having employed all the resources of the science and art of medicine, your patient becomes gradually weaker, his face pale and expressive of much constitutional suffering, his skin flaccid and bedewed with perspiration, his pulse small, rapid, and compressible; that the hepatic tumour increases in size, and when you throw aside his bed-clothes, the whole of the right side appears manifestly enlarged; and, if the bowels are empty, you see the hepatic tumour extending far downwards into the abdomen; in addition to these symptoms, suppose the patient has had shivering fits, not only once but repeatedly;

that his perspirations are profuse, and have a sour smell; that his tongue is dry and glazed; that his cheeks are hollow, and sometimes present a circumscribed flush; and that he is low, weak, and restless. Under these circumstances you may be sure that suppuration is commencing, or has been already established; and the question is, what are you to do? You must change your hand, you must give up antiphlogistics, you must omit the employment of all measures which have a tendency to reduce strength, you must prescribe a light nutritious diet, and anodynes to relieve irritation. When suppuration is fully established, the next consideration is, in what direction the contents of the abscess may escape; and here I need not remind you that it is much better that the abscess should open externally, through the integuments of the abdomen, or into some cavity having an external communication, rather than into a shut sac, as in the latter case it is almost certain, and often immediate death. At this period of the case it will be proper to support your patient's strength by allowing him wine, increasing the quantity if the hectic symptoms threaten to run him down, and taking care that his diet be nutritious and of easy digestion. You will also take care to relieve his sufferings, and irritation attendant on the disease, by the judicious employment of opiates.

When after some time the tumour becomes more elevated and distinct, the pain concentrated in one particular part of the liver, and the abscess is evidently pointing towards the surface, the question then is, whether we shall open it and give exit to the matter, and how this may be best accomplished. That the contents of the abscess should be evacuated as speedily as possible is true, but the consideration is, how far it can be done with safety. Now, I beg your attention to this point, as it has not been sufficiently attended to in works on the practice of medicine. Recollect what the anatomical condition of the parts is under such circumstances, and that, in order to get at the matter, you have to pass through a serous cavity. It is obvious that if you make an incision into the tumour through the peritoneum, and if this be in a state of health, and without any adhesions between its layers in the situation of your incision, you run the risk of having the contents of the abscess effused into the peritoneal sac, and you know that this is almost of necessity fatal. The condition then for success is, *the circumstance of adhesion taking place so as to prevent the matter from getting into the peritoneum.*

Well, it seems to be a very simple thing to give exit to the matter of an hepatic abscess which presents a distinct pointing. Persons will say, adhesion has formed long since, the integuments are swollen and painful, the matter has crossed the peritoneum and lies close under the skin. Here, however, is a curious fact; of all the serous membranes in the body the peritoneum is that which is least liable to general or partial adhesions, and it is well known with respect to hepatitis with suppuration, that you may often have abscess so large as to form a distinct tumour on the surface, which

shall be fluctuating, discoloured, and painful, and with all these conditions, so favourable to the notion of matter being actually under the skin, the patient dies, and on dissection we find not the slightest trace of adhesion. If you plunged a trochar or abscess-lancet into the tumour, what would be the consequence?—death by peritonitis. Dr. Graves and I, in our report of the cases of hepatic abscess which occurred in the Meath Hospital, were the first who drew the attention of the profession to this interesting pathological fact, and, subsequently to this, Mr. Annesly, who has vast experience in hepatic abscess, stated that in his practice he found that the existence of adhesion between the layers of the peritoneum in the vicinity of the abscess, even after swelling, tenderness, and discoloration of the integuments, is by no means a necessary consequence.

It appears then to be quite certain, that the opening of an hepatic abscess is a matter of considerable nicety, and requiring a great deal of caution. The best mode of proceeding which can be adopted is, in my opinion, that which has been recommended by Dr. Graves, and which is founded on the most accurate pathological views. He makes an incision through the integuments, over the most prominent part of the tumour, and carries it through the cellular substance, fat, and muscular tissue, until the peritoneum is nearly laid bare, and there he stops. The wound is then kept open by plugging it up with lint, and after some time the abscess bursts in this situation with perfect safety to the patient. This operation was performed under his direction, for the first time, in a case of abscess where there was no distinct pointing. It was the first operation of the kind, and every one who witnessed it waited with anxiety for the result. Five or six days passed away without any appearance of matter; but about this period the abscess began to point, shortly afterwards there was a large gush of matter through the wound, and the patient recovered perfectly in three weeks. Since that time the operation has been performed on two patients with success and safety. In the case of one patient it was performed twice at no very considerable interval.

Now, I believe you are all aware that in cases of deep-seated collections of pus, it is of the greatest importance to remove the obstruction to its exit externally, and that matter will always point towards the place where there is the least resistance. The performance of this operation not only tends to remove the resistance, but also has this advantage, that the existence of irritation in the neighbourhood of the abscess, and immediately over the peritoneum, has a strong tendency to produce adhesion at this point; a circumstance which I was able to verify in a fatal case, in which the abscess had pointed, but never burst. In this case we found on dissection six or seven small tumours near the surface of the liver, without any traces of adhesive inflammation in the peritoneum over them, but over the situation of the tumour, in the direction of which the incision had been made, there was a considerable quantity of organised lymph, and the two layers of the peritoneum were closely

adherent. That this effusion of lymph had not been accidental, is rendered probable by the rarity of its occurrence, from not being observed in other cases in which an operation had not been performed, and lastly from the success of the operation in those cases in which it had been employed. I would advise you, therefore, in all cases of hepatic abscess showing a tendency to point, but particularly if this pointing be distinctly towards the surface, to make an incision down to the peritoneum, fill up the wound with lint, and you will often succeed in causing the abscess to break externally, and without any danger to your patient.

With respect to the bursting of an hepatic abscess into the cavity of the peritoneum, I have stated before to you, that it is almost necessarily fatal. I say almost, because I have seen two cases of this termination, of which one recovered completely from the peritonitis, and the other lived eight or nine days after the discharge of matter into the peritoneum, and on dissection it was found that a process of cure had been going on. The first of these cases was that of a young woman who had a vast chronic abscess. An attempt was made to make this open externally, by destroying the soft parts over it with caustic, but this not succeeding, a lancet was introduced through the eschar made by the caustic. The patient was immediately afterwards attacked with severe pain in the abdomen, and distinct symptoms of peritonitis. As she was very weak and emaciated, Dr. Graves, under whose care she was, gave her opium in full and repeated doses, allowing her the free use of wine and porter; no blood was drawn, no depleting measures of any kind used, but every thing done to support strength and relieve irritation. Under these circumstances (wonderful to relate) she recovered from the peritonitis. She afterwards sunk from the abscess, and on dissection we found that the peritoneal cavity was obliterated, just as the serous investment of the testicle has its opposed surfaces glued together after an operation for the radical cure of hydrocele. In the other case, the patient lived eight or nine days after the occurrence of symptoms of peritoneal inflammation. On dissection, we found a large quantity of transparent lymph effused on the surface of the peritoneum, in the substance of which several large blood-vessels had been developed.

The principles of treatment in a case of this dreadful accident is to support strength and remove irritation, laying aside all antiphlogistics. I am sure that, under such circumstances, the ordinary modes of treating peritonitis are inapplicable and useless. As I shall return to this subject when I come to speak of peritonitis, I shall here merely state, that the treatment of such a case as this is to be conducted upon the same principles as peritonitis, produced by rupture of the intestine, or a perforating ulcer.

Gentlemen, I shall occupy your time briefly in treating of chronic hepatitis. You will find a full description of the symptoms of this disease in almost every book on the practice of medicine, and it is unnecessary for me to detain you with details of this

kind. If we are to judge from British practice, chronic hepatitis is a very common disease, and, if we look to the practice, it is an affection under which half the community labour. I believe, indeed, that the chronic form of this disease is much more frequently observed in this country than the acute, but still I think it is any thing but a disease of universal prevalence.

I shall not, as I said before, take up your time in stating what you will find in any medical work; I shall merely mention that in chronic hepatitis we have generally derangement of the bowels, chiefly affecting the stomach and upper part of the digestive tube, and in addition to this we have more or less pain, tenderness, and swelling in the region of the liver, and often dulness of sound over the lower part of the right side. When we meet with this train of phenomena, we say that the patient has the symptoms of chronic hepatitis. But no one under such circumstances could undertake to say whether the patient will die of hypertrophy or atrophy, of cancer or hydatids, of tubercles, or of fatty discharge, or of any peculiar disease of the liver. There is another point, too, of which I am anxious you should be aware. Chronic hepatitis is a disease which has been, and is, frequently confounded with various other affections;—with scirrhus of the pylorus, with chronic disease of the duodenum, with chronic disease of the pleura, and empyema of the right side. There is one circumstance which you should bear in mind when you are in doubt with respect to a chronic hepatitis, that one, two, or three of these affections may occur in connection with chronic inflammation of the liver. For instance, a patient labouring under chronic hepatitis may have also at the same time empyema and disease of the duodenum. I believe the subject of disease produced, as it is said, by contiguity in separate organs, has not as yet been sufficiently investigated, and that our knowledge on this important point is extremely scanty.

There are two circumstances connected with this part of the subject, on which I shall say a few words. One common error is that of confounding affections of the heart with those of the liver, and this I regret to say is an error of very serious consequence, and one which is frequently observed in the consultations of medical practitioners. A patient complains of palpitations, a physician is called in, and pronounces the disease to be hypertrophy of the heart; another is called in, and gives it as his opinion that the liver is affected; a third is summoned, and says that both the liver and heart are diseased. In such cases you should always make a careful examination, and weigh well the circumstances of the case in your mind before you venture to pronounce an opinion. In the first place, you are to recollect that organic disease of the heart may produce disease of the liver. Secondly, that disease of the liver (though not so often) frequently brings on morbid affections of the heart and nervous palpitations. Thirdly, that these affections act to one another reciprocally as cause and effect. If a person has disease of the heart, the current of the circulation through that organ is obstructed, and you may have disease of the liver, not as

the result of any original affection of that organ, but as the effect of chronic obstruction to the passage of blood through the heart. The consequent congestion and disease of the liver may, in such a case, be reflected on the digestive tube, and this in turn may re-act on the heart. The heart sympathises then with the irritation of the digestive tube; we have nervous palpitations, and if these continue for a length of time, we have the disease of the heart increased. Again, suppose a patient has chronic disease of the liver, causing more or less obstruction to the circulation; the heart begins to sympathise, palpitations commence, go on increasing, and finally terminate in hypertrophy of the heart. The mischief does not stop here; the effects of obstruction extend to the vena cava hepatica, this in turn re-acts on the liver, and we have in this way a curious train of phenomena; first liver disease, then heart disease, and lastly liver disease again. Let me once more impress upon you that, under such circumstances, you cannot be too diligent in making an examination, or too cautious in pronouncing an opinion.

There is another thing connected with hepatic disease which you should be aware of. A patient, labouring under the following train of symptoms, comes to consult you;—he has pain in the right hypochondrium, loss of appetite, deranged bowels, morbid stools, a dirty bilious hue of countenance, and, in fact, all the symptoms of diseased liver. You examine the liver and find it very much tumefied, in fact, its size is so much increased that you would at once be inclined to say that it was extensively diseased. Now, there are some cases of great tumefaction of the liver accompanied with more or less of the symptoms of hepatic derangement, and yet in such cases you may have no disease of the liver at all, at least none of the ordinary forms of hepatitis: these are cases in which there exists, in adults, a persistence of the embryonary condition of the liver. If we compare the condition of this organ in the infant and in the adult, we find many essential points of difference. In the infant it is comparatively large, and, as it were, hypertrophied; it descends far below the margin of the ribs, and occupies a large portion of the abdominal cavity. On the other hand, if we examine its state in the adult, we find that it has shrunk beneath the short ribs, and that its size and dimensions are comparatively much reduced. Now this physiological atrophy of the liver is a natural and healthy process. There are *certain individuals, however, in whom this change does not take place, and who grow up with the liver bearing the same proportion to the other organs as it did in the fetal condition.* This curious condition is one of the varieties of arrest of development, and is, in almost every instance, observed in those persons whose constitutions present that train of phenomena to which the term scrofula has been applied, and which (if I have time) I shall show you is explained, or at least great light is thrown upon it, by the theory of arrest of development. *In such subjects the tumefaction of the liver is by no means a measure of actually existing disease.* If

you were to suppose this tumefaction of the liver to be the product of actual recent disease, and proceed to treat the patient in the same way as you would treat a case of hepatitis in the healthy subject, you would not only do no good, but, in all probability, a great deal of mischief. I know the case of a gentleman, in the enjoyment of good health, who has this tumefaction of the liver to a very great degree. He is of a thin spare habit of body, with a full, round, and prominent belly; he is pursuing the avocations of an active profession, and yet you will hardly credit me when I say that his liver extends below the umbilicus, and close to the anterior superior spine of the ileum; yet he is very active, and to all appearance a healthy man. You will often meet with this condition of the liver in children who are attacked at an early age with symptoms of *tabes mesenterica*.

At the next lecture I hope I shall be able to finish diseases of the liver, and proceed to the consideration of other affections of the system.

LECTURE XVIII.

Treatment of chronic hepatitis—Neuralgia of the liver succeeding hepatitis—Connection of hepatic with gastro-intestinal disease—Modes of transmission of disease from the mucous surface of the liver—Phlebitis of the vena porta—Obstruction of this vein—Case of pulmonary, hepatic, and intestinal fistulæ—Hepatic neuralgia.

We now come to the consideration of the treatment of chronic hepatitis. It is of great importance, in a case of this kind, to place your patient under such circumstances as will ensure the full and favourable action of the remedies employed. The use of wine, spirits, and all kinds of exciting food, must be laid aside; the patient must not use any thing capable of producing fever during the process of digestion. So long as any kind of food or drink produces uneasiness and sensations of heat and fulness, you may be sure that it will do more harm than good. Give him what will support his strength without exciting the vascular or nervous systems during the process of digestion.

You must next prevail on your patient to give up the use of active purgatives by the mouth. This is a point which you should strongly and firmly insist upon, as, in consequence of the ordinary costive state of the bowels which accompanies chronic inflammation of the liver, the patient is generally in the habit of having recourse to those temporary and hurtful remedies. It is the same thing in cases of chronic hepatitis as it is in chronic gastritis; you will find the subjects of these diseases taking different purgatives every day. Break your patient of this practice, if possible; you will have some difficulty in doing so, for he has been long habituated to it, and you must exercise all your authority in putting a stop to the pernicious habit. Instead of purgatives by the mouth, make him use every

day an emollient injection. You may, if necessary, give, occasionally, mild laxatives by the mouth, as Rochelle salts, manna, castor oil, or something equally mild, and in this way you will be able to secure a regular alvine discharge, once in the twenty-four hours at least. But where there is considerable pain and tenderness in the region of the liver, this plan alone will not be sufficient; you must apply relays of leeches, a practice which has a most admirable effect in chronic hepatitis. I would advise you to apply cupping-glasses over the leech bites; by doing this, you get as much blood as you wish, and you will generally save your patient from the annoyance of an oozing hemorrhage. When piles exist, it will be useful to apply leeches to the anus, followed by the hip bath. But I have no hesitation in saying, that, as a general mode of relieving hepatic disease, the application of leeches to the right hypochondrium is far preferable in every point of view. You may, in the next place, have recourse to blisters; and I have frequently employed blisters, alternately with leeches, with the best results. Tartar emetic ointment, in the form which I have already mentioned, croton oil frictions, and other modes of counter-irritation, will assist materially in bringing about a successful termination. But these must be continued long, and used over an extensive surface.

In this way, by regulating your patient's diet, keeping his bowels open by enemata, or the mildest laxatives, by small and repeated local bleedings, with counter-irritation, you will frequently succeed in removing all the symptoms of chronic hepatitis without the use of mercury. But if, after having carefully employed all these measures, the symptoms manifest a degree of persistence, if your patient has not already taken a large quantity of mercury (which is not likely to be the case in this country), and if he be not of a scrofulous habit, I see no reason why you should not have recourse to mild doses of mercury. For this purpose, nothing answers better than to prescribe, once or twice a day, a pill composed of hydrarg. c. creta, blue pill, or a small quantity of calomel, combined with rhubarb, extract of hyosciamus, and taraxacum. It will be seldom necessary to bring on actual salivation; but if the pain continues to be severe, the swelling undiminished, the symptoms obstinate, and no contra-indication existing, you may bring him under the influence of mercury, and keep him so for a short time. The best mode of doing this is to direct him to rub in a dram of the camphorated mercurial ointment every day; and if you have employed blisters, you can assist the frictions by dressing the blistered surface with mercurial ointment.

Some practitioners are in the habit of substituting the nitro-muriatic acid for the mercurial treatment, and there appears to be evidence that it is an advantageous mode of practice in these cases. The best mode of using this remedy seems to be the endermic; and, hence, bathing the feet, or sponging the right hypochondrium with the acid, are most recommended in chronic affections of the liver. As it is convenient to have a formula for making the nitro-muriatic solution, I shall give you the following. Take of strong nitric and

muriatic acids of each four ounces, and add to these eight ounces of pure water. Here you have a sixteen ounce mixture; of this combination you may take from two to five ounces, and mix them with three gallons of warm water. This, I believe, is the form recommended by Mr. Annesly. Having placed this solution in a foot bath, or tub, you should direct your patient to keep his feet in it for twenty minutes or half an hour. If the bath be of proper strength, it will communicate to the skin a prickling sensation; if not, you may increase its strength by adding an ounce or two more of your mixture. The same solution will answer for sponging over the liver.

There is no doubt that, in certain cases of chronic hepatitis, this remedy has been found decidedly useful, and as its employment is unattended with any dangerous or disagreeable consequences, it has strong claims to our notice. The cases of chronic hepatitis to which it seems to be peculiarly adapted, are, first, those where mercury has been used irregularly, or for a long time without any benefit, and, secondly, where the patient is of a broken down constitution, and where you are anxious to dispense with the use of mercury, if possible. Here the intro-muriatic treatment is of decided value. I need scarcely remark to you that this acid frequently acts upon the system somewhat like mercury, producing tenderness of the gums and ptyalism. Such an effect as this, furnishes us with an example of these cases, in which we find other remedies, as well as mercury, producing a decided effect on the salivary glands, and exercising a very powerful influence over hepatic and syphilitic affections. An interesting fact, bearing on this point, is related by Mr. Cox, in his account of his residence on the Columbia river. Several of his party, who used a strong decoction of the fresh sarsaparilla, were salivated.

There is one circumstance, connected with the treatment of chronic hepatitis, which I believe has not been sufficiently dwelt on. You may have a case in which there was distinct evidence of chronic inflammation, and where, under the influence of judicious treatment, the signs of inflammation and organic derangements subsided, but where severe pain still continues to be felt in the region of the liver. The nature of this pain is often mistaken; *it is supposed to depend upon a continuance of inflammation, while it is, in reality, nothing more than a mere neuralgic affection—a remnant or successor of the former disease, to which the antiphlogistic treatment is totally inapplicable.* Under such circumstances, the patient goes from one practitioner to another, taking different medicines, and submitting to repetitions of the usual modes of treatment, but with little or no benefit. Now I have seen, in several cases, this symptom yield completely to treatment calculated to remove purely neuralgic affections. In a case, lately under my care, of a gentleman who had been attacked with enteritis and hepatitis in India, and who had taken enormous doses of calomel "for the liver," and of croton oil "for the bowels," this circumstance occurred. When first I saw him, he was emaciated, the skin

yellow, the urine high-coloured, with thirst, costive bowels, and great tumefaction in the region of the liver. These symptoms completely subsided under treatment, but a violent pain, running at intervals, continued obstinate. This was rapidly removed by a course of the carbonate of iron, and the use of the belladonna plaster.

It is of great importance, in the treatment of chronic hepatitis, to bear in mind the state of the gastro-intestinal mucous membrane. You are aware that the disciples of Broussais are of opinion that almost all cases of hepatic inflammation are secondary to a gastro-enteritis; that the first morbid action is on the surface of the intestinal tube, and that it is transmitted from this to the liver. I have taken a considerable share of pains in investigating this subject, and have examined very carefully the question as to the complication of hepatic inflammation with disease of the gastro-intestinal surface, and the conclusions to which I have come, are the following:—In the first place, that most cases, whether of acute or chronic inflammation of the liver, present the complication, more or less, with disease of the intestinal mucous surface, and that in the majority of instances there is some degree of actual disease of the digestive tube. It would appear, also, from observation of different cases of hepatitis, that in a great many the affection of the liver has been secondary, and that symptoms of disease of the digestive tube have preceded those of hepatic irritation. But, on the other hand, we must admit that the hepatic affection may be primary; that the liver has the irritative, and that disease has been subsequently propagated to the gastro-intestinal mucous surface. Lastly, we may have hepatitis, both acute and chronic, quite independent of any disease of the mucous coat of the stomach and bowels. This, I believe, is the rarest case; still it does occur. You observe, therefore, that the doctrine of the physiological school, that all hepatic inflammations are secondary to a gastro-enteritis, is not supported by the authority of facts. It is therefore wrong to say that every case of acute or chronic hepatitis is preceded by gastro-intestinal inflammation. Facts have been brought forward to show that not only has inflammation of the liver been observed in the simple state, and independent of any complication with intestinal disease, but that the affection of the liver has distinctly preceded the symptoms of gastro-enteric disease. On the other hand, however, I am free to admit that these are the exceptions rather than the rule, and that, in the majority of cases, hepatitis is either secondary or complicated with disease of the gastro-intestinal surface.

Now, a very interesting question comes to be considered, and this is, how does the disease come from the gastro-intestinal surface to the liver? Pathology informs us that irritation may be transmitted from one organ to another in three different modes. First, sympathetically, as through the medium of the nerves. Thus, long-continued stimulation of the stomach is reflected upon the liver, the liver sympathises with the suffering organ in its vicinity, and finally becomes diseased itself. It is in this way that many

chronic affections of the liver and stomach terminate in affections of the neighbouring viscera and dropsy. The first mode, then, in which disease may come to affect the liver from the gastro-intestinal surface, is by sympathetic irritation. The next mode is supposed to be the actual transmission of disease along the biliary duct from the duodenum to the liver. Inflammation commences in the duodenum; this creeps along the ducts until it reaches the liver, which takes on the inflammatory action in its turn. Several persons of high authority have supported this view of the question, and assert that they can actually demonstrate the passage of inflammation along the ducts. Without denying the possibility of this, yet I feel convinced that it is rare. I have never been able to discover this mode of propagation of inflammation from the duodenum to the liver; and it must be remembered that, in the great majority of cases of duodenitis, we cannot detect inflammation in the liver or its appendages. The last mode by which disease may be transmitted, is the propagation of inflammation along the course of the veins belonging to the portal system, that is to say, there is phlebitis of the portal system, and the inflammation travels along the veins until it arrives at and attacks the liver. That this has occurred, is proved. But we may suppose that, in certain cases, disease of the liver may result from a phlebitis of the minute mesenteric veins, without a continuous spread of inflammation to the larger trunks; just as the lung is affected in cases of phlebitis of the extremities, not by actual spread of inflammation, but rather, as Mr. Arnott has shown, by the transmission of the products of that inflammation.

Inflammation of the portal veins is a circumstance which possesses great interest in a pathological and practical point of view; it is a curious process, and there are some singularities connected with it which have a claim on our attention. In the *Clinique Médicale* of Andral, there is a case given of a patient who, after labouring for some time under symptoms of fever and gastro-enteritis, was attacked with pain and tension in the region of the liver, followed by jaundice. On dissection, marks of inflammation were found in the stomach and ileum; there was also some disease in the colon, and the liver was found to be enlarged, and presenting the ordinary marks of inflammatory action. On a more minute examination, nearly all the mesenteric veins, and the trunk of the porta, were discovered to be in a state of intense inflammation; while, on the other hand, the lining membrane of the vena cava was found to be in its normal and healthy condition. Here we have a very remarkable coincidence between disease of the liver and of the portal system. First, the patient had fever, with gastro-enteric inflammation, and then pain and tension in the region of the liver, followed by jaundice. On dissection, the mesenteric veins and the trunk of the porta are found inflamed; this condition extends to the liver, the substance of which is found tumefied, red, and friable. I believe there can be no doubt that disease of the liver may be brought on by disease of the abdominal veins, particularly those of the portal system. It is a very curious fact, that with symptoms such as many prac-

titioners would not hesitate to call chronic hepatitis, we may have phlebitis, terminating in obliteration of the porta, and even of the vena cava. In such cases, nature generally makes an effort to keep up the venous circulation; in consequence of the obliteration of the internal abdominal veins, the external ones become enlarged, and produce a supplementary circulation to a certain extent, and in this way life is prolonged. This drawing, which represents the appearance of a patient labouring under this form of disease, will give you some idea of the matter. You observe the patient's belly is enlarged and prominent, his extremities œdematous; and here you see those enormous veins passing along the surface of the belly, and keeping up a collateral venous circulation. In the patient, from whom this drawing was taken, the porta and cava were obliterated. These are the epigastric and other superficial abdominal veins which ascend to anastomose with the thoracic, intercostal, and axillary veins.

I shall now relate, as briefly as possible, the particulars of this very remarkable case. The patient, who was the subject of it, laboured for more than twelve months under jaundice, accompanied by wasting of flesh and prostration of strength, but for the first eight months he had not been confined to bed. He suffered, however, very considerably even at this period, from constant pain in the epigastrium and swelling of his feet. Now, in this country, we would be very apt, under such circumstances, to say that he was labouring under chronic hepatitis. At the end of the eight months he became bed-ridden, and the large veins, which you here see, began to make their appearance. Although he was wasting in flesh, still he had a canine appetite, and was always complaining that he had not enough to eat. This is an interesting fact. It has been observed in other cases, and tends to throw some light on the share the mesenteric and other abdominal veins have in the process of absorption. In *tabes mesenterica* it has been often remarked, that the little patients have generally enormous appetites; and, as it would appear from the same cause, a deficiency of nutritious absorption, with this difference merely, that in the disease before us it is the veins that are diseased, whereas in *tabes mesenterica* it is supposed to be the lymphatics. But to return to our case. This patient had, as I remarked, a very voracious appetite, by indulging which, he brought on repeated attacks of constipation and colic. He then got diarrhœa and dropsy, for which he was tapped twice without any benefit. From observing that there was in this case an extraordinary supplemental circulation, leading to the inference that there was obstruction of the deep-seated veins; from remembering that the appearance of the patient, and the more prominent symptoms, coincided with those of a former case, in which obliteration of the porta had been discovered after death; from these circumstances, and the remarkable voracious appetite, M. Reynaud, under whose care the patient was, came to the diagnosis of phlebitis of the portal system, extending to and affecting the liver; and this diagnosis was subsequently confirmed by dissection. He was,

however, unable before death to explain one symptom which was present, namely, infiltration of the lower extremities. You are aware, that when the general venous circulation is obstructed either in the chest or belly, we have anasarca of the lower extremities, but when the obstruction affects only the portal system, then we have ascites as the first phenomenon. If you had two cases of dropsical effusion, in one of which there was, *first*, œdema of the lower extremities, in the other, *first*, ascites, you could thus determine where the primary obstruction existed. M. Reynaud was at a loss to account for this symptom in the present case, as he had not observed it before in the other case, and as the swelling of the feet had preceded that of the belly. On dissection, it was found that the right branch of the porta had been obliterated by the growth of a yellow substance, somewhat like the middle coat of arteries; the same was found to exist in the corresponding hepatic veins, and the inferior cava was found obliterated to the distance of three inches from the left auricle. The left branch of the porta was pervious, the corresponding hepatic veins much enlarged, and the superficial epigastric veins inosculated freely with the intercostal and axillary veins.

The vena azygos was very much dilated; and, what is extremely curious, a large vein was seen to arise from the union of the sub-peritoneal branches on the convex surface of the liver; this passed through the diaphragm, and emptied itself into the cava close to its termination. Here we have an entirely new vein. It was also observed, that the sub-diaphragmatic veins were much increased in size, and apparently varicose; these passed through the diaphragm, and inosculated with the pericardial and superficial thoracic veins. Some of them ran up and opened into the great coronary vein of the heart, which was as large as the crural vein. The remaining peculiarities of this curious case were inflammation of the duodenum and gall-bladder. The cavity of the latter was half filled with purulent fluid.

I am fully convinced that I have seen instances of this disease, although I was not so fortunate as to have an opportunity of verifying the diagnosis by dissection. I have seen patients who had wasting of flesh, pain and tension in the region of the liver, and jaundice, with this singularly varicose state of the external abdominal veins; some of them had ascites; and I recollect distinctly, that in one case the appetite was very great, and the patient had a tendency to diarrhœa. I am satisfied that in such cases you would be fully justified in making the diagnosis of obstruction of the portal system; and if, in addition, there was infiltration of the lower extremities, there would be a probability that the disease had extended to the cava itself.

Before I proceed to the consideration of a subject to which I have already alluded—hepatic neuralgia—it may not be amiss to exhibit some specimens of organic lesions of the liver. Here is an example of abscess of the liver:—you perceive the softened yellow degeneration of the substance of the organ; and here is the cavity of the

abscess, in which you may observe a loose slough suspended. This portion which surrounds the abscess may be looked upon as a fair specimen of the yellow softening of the liver, before its substance breaks down into a purulent mass. Here is another specimen exhibiting the same phenomena. Here is a very curious example of hepatic abscess, which perforated the diaphragm, and made its way into the substance of the lung. I regret that the whole of this preparation has not been preserved. The rest of the preparations before me illustrate chronic disease of the liver. Here is an example of the disease which has been called cancer of the liver. Time will not permit me to enter into a detail of the pathological circumstances of this case. The patient was a female, who had cancer of the breast, scirrhus of the pylorus, and aneurism of the aorta, with this disease disseminated through the substance of the liver. Here is another preparation of what would be called by many persons pure cancer; the patient, a female, had cancer of the mamma. This, and the preparation on the other side, exhibiting a mass of white, firm, semi-cartilaginous substances, are examples of what has been called tubercle of the liver. Here is an example of the disease which has been termed whiskey liver, a disease which is said to be ordinarily found in persons who indulge in the use of ardent spirits. This, however, is a term which has been often abused and misapplied; for persons indulging in the use of whiskey may have every form of disease of the liver, and the appearance before you may be detected in the livers of persons of the most temperate habits. On the label of this preparation is written—"A Specimen of Whiskey Liver," but this you will not mind. There is a very remarkable fact, however, respecting this kind of liver, verified by Professor Carswell, namely, that this condition of the liver is always accompanied with more or less ascites. I may add, that I have never met with this disease without ascites.

I remember a most remarkable case of disease of the liver, which occurred during my stay in Edinburgh. My lamented friend and instructor, the late Dr. William Cullen, whose loss to pathological medicine was irreparable, and whose splendid attainments and high character justly and rapidly raised him to an elevated rank in his profession, brought me to see a patient. One of the most curious circumstances connected with this case was, that when the patient sat up in bed, a fluid of a serous character was poured out in considerable quantity from the anus; but while he remained in the horizontal posture this did not occur. The patient died shortly afterwards; and, on dissection, it was found that he had a gangrenous abscess of the right lung, communicating with the pleural cavity, which contained a quantity of a sero-purulent fluid, and a mass of hydatids, some broken down, others perfect and entire. On continuing the dissection, it was found that the cavity of the pleura communicated with the right lobe of the liver through the diaphragm. In the right lobe of the liver the same kind of sero-purulent fluid, and a quantity of hydatids, were discovered; and, what was still more extraordinary, the cavity in the liver was found to communi-

cate with the colon by a distinct opening. There was, then, in this very remarkable case, a direct communication between the bronchial tubes and the colon, through the pleura and liver. We can thus see that, when the patient assumed the erect position, the fluid would immediately pour into the colon.

As I am anxious to finish the subject of hepatic disease to-day, I shall now draw your attention to one of the last points connected with this subject, namely—neuralgia of the liver. It is a singular fact that a patient may labour under severe and harassing pain in the region of the liver; that this pain may last for months and years; that he may die of some other affection; and that, on examination after death, we may find the liver without the slightest trace of disorganisation; and, also, that the organs in its vicinity present no appearance of any organic disease. Many cases of this kind have been observed; and it is the opinion of the best pathologists that they are examples of neuralgia, the seat of pain being the hepatic plexus. It is a disease of no very unusual occurrence, and is often found in females of a nervous and hysteric habit. It is constantly mistaken for hepatitis, and there is no greater mistake than this, or one which is likely to entail more misery on the patient. The persons who are subject to this affection are, as I remarked before, generally of a nervous and hysteric habit; they complain of pain in the right side, of more or less constant occurrence, and this pain, during its exacerbations, is often most excruciating. Now, this circumstance furnishes us with a sort of key to diagnosis; for with this dreadful pain, and, in some cases, exquisite tenderness in the region of the liver, we have the skin cool, the pulse tranquil, no fever, no permanent derangement of the bowels, no tumefaction of the liver. If this were the pain of acute inflammatory disease, a fatal result would be produced; or if it belonged to a chronic affection, it would terminate in organic derangement; and yet we find it existing with a clear colour of the skin and eye, healthy fæces, calm pulse, and absence of swelling in the region of the liver. Add to this, that the disease may have lasted for a considerable time, and that it occurs in a person of hysteric and nervous habit. Moreover, if the patient has been treated for hepatitis unsuccessfully, you may make up your mind to the diagnosis of hepatic neuralgia. Here is the diagnosis; pain in the region of the liver, with occasional violent exacerbations, and accompanied by tenderness of the integuments, but without swelling, symptoms of fever, or abdominal derangement; the disease being of long standing in a person of nervous habit, and having resisted bleeding, mercury, and even counter-irritation, or being made worse by those measures.

Now, it is no uncommon thing to see this disease mistaken for acute hepatitis; and I need not tell you how ruinous to the patient's health such an error must be. When you are in practice, you will meet instances of females labouring under this affection, who have gone through a variety of treatment. When you recollect that the disease occurs generally in hysteric females, and that such persons

are injured by depletion, you can conceive how much mischief may be done by repeated bleedings and courses of mercury. Some of the most deplorable cases I have witnessed, were those in which neuralgia of the liver had been mistaken for hepatic inflammation, by a number of practitioners, and the patient subjected to such modes of treatment as gave her constitution a shock from which it never recovered.

The treatment of this disease must be both general and local, but by no means what you would call antiphlogistic. You will have some difficulty in preventing the patient from getting herself blooded; for though the lancet is inadmissible, yet its employment gives a temporary relief, and this encourages the patient to have recourse to it again. What I would advise you to do in this disease is, first to pay attention to the general condition of the patient. You must pursue a general anti-hysterical plan of treatment, remove every source of irritation and excitement, and take measures to improve the general health by exercise, regimen, moral improvement, and the judicious employment of tonic medicines. With respect to the pain, one of the most powerful means of arresting and removing it, appears to be the use of the carbonate of iron in full doses; and this is an interesting circumstance, when we recollect the power which it possesses in removing pain in other nervous diseases. I would advise you to try this after having premised the use of purgatives, and continue it for some time, for you will often find that it will not only cure the pain, but also improve your patient's strength and appetite. While you are giving it, order your patient to take some mild purgative, as compound rhubarb pill, to prevent constipation. When you are about to prescribe a course of carbonate of iron, you should prepare your patient to find the stools coloured. I have known this circumstance taken hold of and turned to their own advantage by quacks. The patient is told that his complaints arise from the existence of morbid and dark-coloured matters in his bowels. Preparations of iron are given, and the black matter begins to come away, greatly to the credit of the empiric. After a time, the medicine is omitted, and some purgative substituted; the stools become natural, and the trick is complete. During the paroxysms of pain, a mustard plaster, or anodyne stupes, and anodyne enemata, will give relief; and, in the intervals, I would advise you to use the belladonna plaster, after the following formula:—'Take of extract of belladonna three parts, of gum ammoniac and soap plaster each one part; spread these on a piece of leather with an adhesive margin, and make the patient wear it over the region of the liver. If there be any tenderness over the lower dorsal vetebræ, you may apply a few leeches, followed by narcotic stupes, or counter-irritation.

I have seen this hepatic neuralgia without any hysteric complication. I remember the case of a lady who had three or four healthy children, and had never been subject to hysteria. This lady came up to Dublin to be treated for liver disease—in fact, to be salivated; but happening to fall into the hands of a judicious

friend of mine, who recognised the true nature of her complaint, she was treated with carbonate of iron, and cured effectually. I knew another case of a young gentleman, in whom (after being treated for symptoms of chronic hepatitis) this pain continued for a considerable time, and was at length removed by carbonate of iron, and the use of the belladonna plaster.

LECTURE XIX.

Gastritis, with delirium tremens—Varieties of intestinal worms—Organisation and origin of—Occurrence in the fetuses of various animals—Formation—Pathology of—Perforation of the intestines by—Worms in tumours and abscesses.

You may recollect that, when treating of acute gastritis, I alluded to the great importance of being aware of its complication with delirium tremens; and stated, that in the form of delirium tremens, which is the result of an excessive debauch, and where the stomach has been subjected to powerful stimulation, we have reason to believe that there is more or less of gastric inflammation. I have it in my power, to-day, to exhibit to you a very accurate drawing of the stomach of a patient who laboured under this form of disease, and whom I had an opportunity of examining several times before death. You will remember, also, I mentioned that in cases where symptoms of delirium tremens had arisen from excess, and not from a want of the customary stimulus, the ordinary routine treatment of giving wine, brandy, and other spirits, was extremely improper; and that where it was persevered in, and the patient died, you commonly found, on dissection, evident marks of inflammation in the brain and stomach. On that occasion, too, I quoted this as an example of the latency of gastric symptoms when complicated with an affection of the nervous centre. I have now to exhibit this drawing, which represents the stomach of a man who died of delirium tremens, supervening on a severe debauch. This patient was treated entirely on the stimulant plan; he got wine, porter, brandy, and opium, but their exhibition was not attended with the slightest benefit. Under their use his symptoms changed, and assumed a decided cerebral character; he had hot skin, quick pulse, great thirst, and general symptoms of fever, accompanied by a comatose condition. Previously to opening the body, I gave it as my opinion that the stomach would be found to exhibit marks of inflammation. Here is an accurate drawing of the stomach, and, from its appearance, you will be able to judge for yourselves. (*Here Dr. Stokes exhibited the drawing to the class, representing the stomach in a state of intense vascularity.*) Observe the generally diffused dark red colour of the whole organ, and the excess of inflammation towards its cardiac orifice. The brain, in this case, was but slightly vascular.

I propose to devote this day's lecture to the consideration of an interesting subject in practical medicine—intestinal worms. There are few subjects possessing so much interest, in a physiological and pathological point of view, as this; and, in order to have correct notions, it will be necessary for you to be acquainted with the investigations of modern science on this subject. You are well aware that worms are found in most classes of animals. They occur in reptiles, fishes, birds, in the different classes of quadrupeds, and in man. In man they do not exist in such abundance, nor so frequently, as they do in birds and fishes. With respect to their places of habitation, we find them, first, in cavities which have an external communication, and next, in the parenchymatous substance of organs; and we generally observe, that those which inhabit the cavities are different from those met with in parenchymatous parts. We observe, also, that the species existing in the different organs and cavities are not only different in their nature, but that there is a difference between the worms which inhabit separate portions of the same organ or cavity. In one part of a cavity or organ we find one species, in another a different, and this occurs almost invariably, as if it was regulated by a fixed law of the economy. A peculiar species of worm, occurring in man, called the *distoma hepaticum*, is never found except in the liver or gall-bladder. If this animal had been introduced from without, it would certainly be detected in some part of the intestinal canal, but this is never the case. Rudolphi states, that the *strongylus horridus* is to be met with only in the œsophagus of aquatic birds, and the *ascaris obtusa* in the stomach of mice.

Generally speaking, worms are of three different forms—cylindrical, riband-shaped, and vesicular. Their organisation varies from the lowest scale, in which we can scarcely trace, as it were, the rudiments of an animal; beginning with the tape-worm, which presents little more than a cellulo-gelatinous mass, we ascend gradually until we arrive at a high degree of organisation, where we find well-developed muscles, a difference of sex, generative organs, and, according to some anatomists, a tolerably perfect nervous system.

Now, to remove all sources of doubt and error on this interesting subject, and to establish proper principles of treatment, let us examine into the origin of these animals. I shall confine myself to the consideration of the origin of those worms which inhabit the human intestines, as they are the only species which we have to do with as practical physicians.

You will at once perceive that worms must be derived from one of two sources; either as introduced from without, or formed originally within the bodies of man and other animals. It is maintained by those who are in favour of the first supposition, namely, that they are introduced from without, that similar animals are to be found in the external world, and that they are introduced either in the form of ova, or in a state of perfect development, with the food or drink, or by the respiration of the animal. Observe, this

doctrine is founded on the validity of the assertion as to whether animals similar to intestinal worms are to be met with in external nature. Linnæus states, that he found the tape-worm, and the small ascarides, a species now called *oxyuris vermicularis*, in a marsh in Lapland; but Müller, a much more accurate helminthologist, has since shown, most satisfactorily, that Linnæus was completely mistaken, and that those he had observed are never found to exist within any animal whatever. There are many observations on record similar to those of Linnæus; but as they were made at a time when natural history was in its infancy, and as they have been disproved by the researches of modern zoologists, I shall not notice them. I believe there is no well-authenticated instance on record of tape-worms, lumbrici, or ascarides, being found living in any situation external to the animal body. Every one of you have seen worms in the intestinal canal, or recently discharged by stool or vomiting; but I will venture to say that not one has ever observed them in any article of food, in earth, or in water. Bremser, who is a high authority, makes a very pertinent remark on this subject. "We find," says he, "all animals most abundant in that situation which has been assigned to them by nature. Now, if these animals were accidentally introduced from without, we ought to find them more abundant in the earth, water, &c.; but the contrary we have seen to be the fact."

But it is contended that these animals may have been introduced from without, and that in consequence of a change in situation, nutriment, and other circumstances, their forms may be altered; and it is argued, in support of this hypothesis, that external circumstances will and have been observed to change the forms of plants and animals in a very remarkable degree. In addition to this, it may be said that an alteration in the nature of its food may even produce an actual change in the function of the animal. It is a singular fact that neuter bees may be made prolific by changing their food; it is shown that when a queen bee dies or is lost, the neuter bees take a grub of their own species in place of her, and, by feeding it in a particular manner, it becomes capable of laying eggs.

Now, supposing that intestinal worms are introduced in the form of ova into the human body, there is no reason why this sudden, remarkable, and complete change should take place. We see nothing similar to it in nature. The plant which springs from any particular seed will resemble that from which it derives its origin; the egg of any particular bird, no matter in what way it may be hatched, will produce an organised being similar to its parent. The form and character of the animal are given during the act of generation, and remain unchanged. Again, admitting that a difference in circumstances and nutrition might produce a total change in form, it should be in our power to demonstrate the individual in the process of transition; we should find those animals in a state half between what they were and what they are, and this state we should observe of very frequent occurrence. No such thing, however, has been ever demonstrated. Out of a vast

number, Bremser did not find a single one in any stage of transition, nor has it been demonstrated by any zoologist. He also states expressly, that after having diligently examined fifteen thousand specimens of worms in the cabinet at Vienna, he never was for one moment at a loss to say which were intestinal worms and which were not. If there was any such transition, it would have been discovered, but no such thing has ever been observed.

It appears, then, obvious that there is no direct evidence to prove that these animals have been introduced into the body from without, either in the form of ova, or in a state of perfect development. We have nothing, then, I think, but to come to the other conclusion, that they originate within the body, and this seems to be the opinion of the best physiologists and pathologists. This doctrine appears to be almost brought to a demonstration by the following facts. First, it appears that the worms which have been found in man and animals have a peculiar structure and organisation, differing materially from that of the worms which inhabit the external world. This is a point admitted by almost every modern writer on natural history. In the next place, we find that the worms of certain animals present peculiarities differing from those of the same species in others. Thus the bothriocephalus and tœnia solium, in man, differ from those of other animals. You are not, however, to conclude from this that every animal has its peculiar worms, for such is not the case. Thus the lumbricus and small ascarides of man are found to exist in various animals, both carnivorous and graminivorous.

It appears obvious, that if worms were introduced from without, we should not find peculiar worms in the bodies of certain animals; yet taking a certain number of different animals, living on the same food and in the same situation, we find a difference in the nature of the worms which are met with in the bodies of each. Another important fact is, that worms are to be found not only in the intestinal canal, but in almost every part of the body. We find them in the cellular tissue, in the liver, gall-bladder, lungs, and trachea; in the brain, heart, kidneys, and spleen. They have been met with in the air-bladders of fishes; and Treutter states that he has found the polystoma pingucicola in the ovaries of a woman which were steatomatous, and the strongylus in an aneurism of the mesenteric artery of the horse. These animals have been observed in the anterior chamber of the eye in birds and horses, and there are innumerable examples of their occurrence in situations equally strange and anomalous. Another circumstance already mentioned, and which must be coupled with the fact just alluded to, is that there are certain species of worms which occur only in the same organs, and are never met with in any other situation.

Now, observe the importance of these facts—we find that worms not only exist in the digestive tube, and parts having an external communication, but also in the very substance of deep-seated viscera, and that the worms which are found in the various cavities and organs are peculiar to them. In one case, we find a worm in

the digestive tube, in another in the brain, in a third in the liver, in a fourth in the pulmonary apparatus, but no one has ever been able to demonstrate the trajet of a worm from one of these cavities or organs to another. It would be ideal and absurd to say, in the case of worms found in the substance of viscera, that they had been introduced from without, or came from the intestinal canal. The distoma hepaticus, which is found in the liver and gall-bladder, might be supposed to arrive at those situations by passing along the ductus communis choledochus; but in the various cases in which it has been found, it has never been detected in the intestinal canal; and this, I think, would not have been the case, if the digestive tube had been its original situation. One of the most important facts which have been stated is, that certain forms of these animals are found invariably in certain situations; and this has been observed not only in man, and other animals of the class mammalia, but also in reptiles and fishes. In man, we generally find the lumbricus inhabiting the stomach and small intestine, the tricocephalus in the cœcum, and the small oxyuris, or thread-worm, in the rectum. The preparation before me exhibits a specimen of the rarest form of worms which inhabit the intestinal canal, the tricocephalus. Here is the cœcum filled with these singular worms. The males are distinguished from the females by the whirl of the tail. If these little animals, or the oxyuris, had been introduced from without, we should expect to find them in various parts of the intestinal canal; but we find, on the contrary, that their situation is separate and distinct.

Lastly, *intestinal worms have been found in the fœtus, both of man and other animals.* Kerkring describes a fœtus, the intestinal canal of which contained a vast quantity of small worms; and another of six months, in whose stomach a large lumbricus was found. Rudolphi, Blumenbach, and others of nearly equal authority, have recorded abundance of examples of worms existing in the fœtuses of various quadrupeds, *and also in those of birds which had just broken the shell.* Those who are obstinately attached to the doctrine that worms are introduced from without, have gone so far as to assert, that the ova of the worms have been transmitted at the moment of generation, a doctrine so absurd that it is unnecessary for me to enter into any refutation of it.

With respect, then, to the formation of worms in animals, we cannot help coming to the conclusion that they are originally formed within the body, and that, in fact, there is an original generation of these animals, the result of one organisation taking place within another—the production, in fact, of a distinct being. This idea does not appear so difficult of conception when you recollect that circumstances analogous to it are extremely familiar and of almost constant occurrence. There is not much more difficulty in conceiving the formation of a living worm within the body than there is of conceiving the organisation of a portion of lymph thrown out upon the surface of a serous membrane. What occurs in both cases is, that, under the influence of the vital principle of the original

animal, a portion of matter, previously inorganic, assumes the properties of life, presents distinct traces of organisation, vascularity, and sensibility. The only difference between them is, that in one case the organised mass remains adherent to the matrix, and in the other it is cast off, and forms a separate being. In the present state of our knowledge, all speculation on the mechanism of the formation of worms must of necessity be nothing more than mere hypothesis. The idea which Bremser entertained on this subject is, that *intestinal worms* are formed by the presence of semi-assimilated nutritious matter in the digestive tube. Food, taken into the system under ordinary circumstances, is converted into a substance fitted for the purposes of absorption and nutrition; but when the process is not perfected, it is not taken up by the absorbents, and is then, according to Bremser, converted into an animal substance. This appears to be but a crude idea, unsupported by any facts; and it would be more philosophical to say that we know nothing about the matter. Besides, worms occur in various parts of the body as well as the digestive tube; and to suppose the presence of unassimilated matter in such situations would be only supposing an absurdity. Bremser brings forward, in support of his theory, that worms are of very frequent occurrence in cases where the assimilating powers are weak or deranged, and says that nothing is more common than to meet with an abundance of these animals in scrofulous persons, in those who have great appetites and bad digestion, and in children labouring under disease of the mesenteric glands. On the other hand, there are abundant instances of worms existing without the slightest apparent injury to the general health. In certain countries almost all the inhabitants have worms. But I believe all that we can affirm on this subject is this, that they are not introduced from without, and that they are formed within the body by a process, the nature of which is exceedingly obscure.

Now, to come to the pathology of this subject, can we connect the formation of intestinal worms with any known pathological condition of the intestinal canal? This is a question of no ordinary importance; for if we were able to connect their formation with an inflammatory or any other state of the digestive tube, it would furnish us with a key to correct and successful treatment. The school of Broussais are of opinion that worms are the result of an acute or chronic inflammation of the gastro-intestinal surface. This doctrine is by no means supported by the evidence of facts, for it has been established *that worms are found to exist not only in connection with every possible pathological condition of the intestinal canal, but also where the tube presented the appearance of perfect health.* We cannot, then, safely affirm that intestinal worms are connected with an inflammatory or non-inflammatory condition of the digestive tube. Andral states that he has found them in all conditions of the intestine, whether red or pale, dry or covered with mucus. They are most commonly, he says, enveloped in a quantity of mucus, and there is some redness in the place where they are lodged; but this appears to be rather the effect of

their presence than the cause. I believe it to be the fact, that persons in excellent health, and with the intestinal canal in the normal state, may have worms. Dogs, who are killed while in a state of apparently perfect health, are often found to have a large quantity of tape-worm in their intestines. It is idle and hypothetic to say, that the formation of worms depends upon an inflammatory or non-inflammatory, an asthenic or sthenic condition of the digestive tube; their formation is owing to some modification of the vital power, the nature of which is unknown. I again repeat, that nothing can be stronger against the supposition that worms depend upon inflammation than the fact of their being observed in considerable quantities in healthy individuals.

A very curious point, connected with this subject, is the question of perforation of the intestines by worms. This question, which is an interesting one in many points of view, has been lately the subject of medico-legal discussion, and therefore demands a share of our attention. Of the different kinds of intestinal worms, the only one which is supposed to be capable of perforating the coats of the digestive tube, and escaping into the peritoneum, or some adjoining organ, is the lumbricus, which is remarkable for its vigour, and for the sharp and pointed shape of its head and tail. Many of the most eminent pathologists of modern times, and, among the rest, Andral, Rudolphi, and Carswell, are of opinion that these worms are totally incapable of perforating the intestinal tunics. Andral states that there is no well-authenticated instance of this occurrence on record; and Rudolphi declares that they have no apparatus for effecting a passage through any continuous tissue. On the other side of the question, however, there are some curious facts and cases given, which, supposing that worms are incapable of perforating, are very difficult to explain. Dr. Fischer, of Vienna, gives the case of a female, in whom the following circumstances were observed on dissection. Two circular orifices were found in the colon, communicating with the cavity of the peritoneum; in one of these openings a worm was discovered, one half of which lay in the peritoneal sac, the other in the intestine. No other worms were found in the digestive tube; but a second worm, like the former, was found in the peritoneum. Here we have a very remarkable coincidence of perforation of a portion of the gut, with the existence of one worm in the cavity of the peritoneum, and another of a similar description, as it would appear, in the act of making its way in the same direction. These circumstances, together with the existence of a double perforation, seem to be in favour of the idea that the openings had been made by the corresponding worms. Another case is mentioned in the *Elements of Pathological Anatomy*, by Andral, and he quotes the case, not as one of perforation merely, but to show that the symptoms of effusion of matter into the peritoneum may, under certain circumstances, be nearly latent. The subject of this case, a young man, labouring under phthisis, had a tumour near the umbilicus, which increased rapidly in size and presented a distinct fluctuation.

Soon afterwards, the integuments gave way, and a large quantity of matter was discharged, together with a lumbricus. During the progress of this disease, there was some tympanitis, but little or no pain had been complained of. On dissection, there was a considerable number of worms, and a quantity of matter, found in the peritoneum, and a perforation in the arch of the colon, corresponding with the extravasated matter. Bremser gives a curious instance of this kind, as occurring in a species of fish. In this case, the fish died; and it would appear, says Bremser, that the worm, finding some extraordinary change had taken place, was determined to take a peep and see what was the matter, for it had perforated not only the intestinal tube, but actually made a passage for itself through the whole body of the fish, until it reached the water in which it had been lying. Here, finding that its world extended no further, it stopped, and began to make its way back again to its original situation by a new opening, so that when it was observed by Bremser, the two ends were in the intestinal tube of the fish, and the middle portion external. This, however, does not resolve the question, as to whether lumbrici are capable of perforating the intestinal canal or not. My own impression on the subject is, that we have not, as yet, any distinct and unquestionable evidence of these worms being possessed of any perforating power; but it is a fact, that there are a great many cases on record of worms being discharged in considerable quantities from openings in the intestinal tube, and where it would appear that the openings had been formed, not so much by the action of the worms themselves, as in consequence of their exciting an irritation in some portion of the intestine, followed by inflammation, ulceration, and escape of the contents of the tube into the peritoneum. There are many instances of this kind. An interesting case is mentioned of a female, who was attacked with pain in the groin, followed by the appearance of a tumour, which she was directed to poultice by her medical attendant. After some time, the integuments gave way, a quantity of matter was discharged, followed by a large lumbricus, and during the progress of the case about one hundred of these animals were discharged through the opening. This is a well-authenticated case. Another case is mentioned of a patient who had been subject to constipation and violent attacks of colic. A tumour began to appear in the right hypochondrium, followed by pointing and ulceration of the integuments, and a discharge of matter. A number of worms (I believe twenty-four) were discharged through the opening, which remained pervious, and the patient lived for many years afterwards with an artificial anus. This case appears to be not an example of direct perforation from worms, but of the accumulation of a mass of these animals in a particular portion of the intestine, giving rise to irritation, which terminates in ulcerative absorption of its tunics, and escape of its contents. Inflammation is set up in some part of the intestine, this goes on until the coats are all destroyed, and the matter and worms escape into the peritoneal cavity; but if adhesion should prevent

this, an opening will be formed in some part of the integuments covering the belly. In both cases, the opening is produced not by an exertion of the worms, but by an ulcerative and vital process. In support of this view, it has been observed that worms have come out through these apertures not head foremost; the centre portion appears first, and you can draw it out like a loop. Such cases as the foregoing, then, cannot be fairly given as cases of perforation from worms, but as cases in which these animals, acting somewhat like foreign bodies, produced irritation, inflammation, and ulcerative absorption. There is a very curious case on record, of a patient labouring under abscess of the liver, which burst externally, and a lumbricus was discharged with the matter. The patient died; and, on dissection, it was found that the cavity of the abscess had a communication with the stomach, through which it was conceived that the lumbricus had got into the liver.

The worms which inhabit the intestinal canal in man are the following:—first, the lumbricus, or common round worm; next, we have the tape-worm, of which two varieties have been described; thirdly, we have the very curious worm, of which there is a specimen before me—it inhabits the cœcum, and is called *tricocephalus*; lastly, we have the thread-worm, to which the name of *oxyuris vermicularis* has been lately given. The lumbricus generally inhabits some portion of the small intestine, but is also frequently found in the stomach. Persons have often vomited them, and they have been known to have crept out by the mouth. They have been found also in the pharynx, œsophagus, and large intestine. There is an interesting case mentioned by Andral, of a child who, in a state of apparently good health, was suddenly seized with symptoms of suffocation, and died. On dissection, it was found that a large lumbricus, which had come up from the stomach, had, when it arrived at the glottis, turned into its orifice, and, by irritating the larynx, produced spasmodic closure of that organ, and suffocation.

The lumbricus presents very marked appearances of an advanced state of development. The male has a peculiarly formed penis; the female has her generative organs well developed; and both have an extensive alimentary canal. The *tricocephalus* is about an inch in length, terminating in a point; the sexes are different, and the male is distinguished from the female by the circular whirl of his tail—it is always found in the cœcum. The small thread-worms, with which you are all acquainted, are almost exclusively found in the rectum. These worms are found in vast numbers in some children; and it is said that the quantities of them which are discharged by the West Indian negroes are extraordinary.

The tenia, or tape-worm, is generally found in the small intestine; but it has also been observed in the stomach, colon, and rectum. The length to which this animal sometimes attains is almost incredible. Bremser mentions a case in which a tape-worm one hundred and fifty feet in length was discharged by stool. Another case is given, in which the tenia had the enormous length

of three hundred feet. I have myself seen a large wash-hand basin filled by a mass of tape-worm, discharged after a strong dose of castor oil and turpentine. Still more extraordinary instances are recorded. Thus, in the Copenhagen Transactions, we read of a tape-worm eight hundred ells in length. But, in all probability, there has been an error in these measurements, and many worms have been taken for one. This is rendered probable by the fact observed by Robinus, who found in the body of a man, who had before death discharged fragments of tape-worm, a tape-worm extending from the pylorus to within six inches of the anus. The length of this single worm was scarcely thirty feet. One interesting circumstance connected with this animal is, that it is inferior in its organisation to every other species of worm. It appears to be nearly a simple, homogeneous, cellulo-gelatinous mass, without any division of sexes, and without a nervous system, or generative organs. It is said, also, to occur principally in persons whose powers of life are low; and if this be the case, as I believe it is in many instances, it furnishes us with a very curious and interesting fact. The other better developed kinds are found in persons of healthy, good constitutions; but the tape-worms, though sometimes met with in such persons, are generally found to occur in persons of low and weak diathesis. Here we see a curious connection between the product and the producing cause.

With respect to the exciting causes of worms, a vast number of circumstances have been mentioned by authors, as giving rise to their formation. Foul air, low, damp situations, bad diet, the constant use of milk, cheese, sugar, vegetables, have been reckoned among their exciting causes. I believe we are not well acquainted with these causes. They appear often to be connected with some morbid influence produced upon the system by bad diet, and other circumstances; but what the nature of this influence is, we know not.

LECTURE XX.

Symptoms of intestinal worms—Sympathetic irritations—Affections of the nervous and respiratory systems—Various diseases mistaken for worms—Exciting causes of worms—Farinaceous and milk diet—Vermineous fever—Treatment of worms—Specific and mechanical purgatives; calomel, turpentine, &c. &c.—Remedies for each species of worms—Preventive measures.

Let us proceed with the consideration of intestinal worms. At my last lecture you will recollect that I spoke of the different kinds of worms, and stated that there was a difference between the worms which are found in various parts of the body; that I examined the question as to the origin of these animals, and came to the conclusion that they are formed originally within the bodies of man and

other animals. I mentioned the various kinds of worms which inhabit the digestive tube in man, and examined at some length the question of perforation of the intestinal canal by lumbrici. We come now to the investigation of the symptoms.

With respect to the symptoms of worms, it is a singular fact, that we have not one single pathognomic sign of their existence, except the circumstance of their being occasionally passed by stool, or vomited; almost all their symptoms are referable to irritation of the gastro-intestinal surface, and its sympathetic relations. Persons, who are much subject to worms in these countries, are generally of a pale complexion, with a bluish circle round the eyes; the belly is more or less prominent, and there are various signs of irritation of the digestive tube, with itching at the nose and anus; headache; foul breath and tongue; irregular and sometimes canine appetite, nausea, hiccup, borborygmi, tenesmus, diarrhœa, and constipation. Though the patients take abundance of nutriment, they are generally thin and pale; and in such cases there is either one or two very large worms, or a great number of smaller ones, or their presence is complicated with disease of the intestinal canal. Such persons are also observed to be of an indolent and languid habit; they have perspirations, disturbed sleep, with grinding of the teeth, and irregularity of pulse.

The sympathetic irritations produced by worms are numerous and extraordinary. The genital organs may be excited, and we may have priapism and seminal emissions in the male, and irritation amounting to nymphomania in the female. There is a very singular case on record of a female, aged seventy, being seized with a violent attack of nymphomania from this cause. The nervous affections produced by worms are so Protean and so numerous, that it would be almost impossible to detail them; in fact, there is not a single nervous disorder which may not be simulated by the sympathetic irritation of worms. Epilepsy, hysteria, convulsions, dilatation of the pupil, amaurosis, symptoms of hydrocephalus, and even mania, are among the affections of the nervous centres or their immediate connections, which, in repeated instances, have been found to depend on the presence of worms. Kraus gives an extraordinary case of a man, who, at a very advanced age, became subject from this cause to fits of continued and inordinate laughter.

There is another case on record of convulsions depending on worms, which, like those from the bite of the tarantula, are said to have been soothed and relieved by music. Hufeland, in his journal, mentions a case of yellow vision from the same cause; and there are several instances of aphonia and mania on record, which have yielded to treatment which had removed intestinal worms. A case is mentioned of a person who got violent spasmodic action of the muscles of the eye, producing inversion of that organ to such a degree that the eyeball appeared to be nothing more than a mass of red flesh. A case is recorded by Serres, in which the symptoms strongly resemble those of hydrophobia; and

it is probable that some of the cases of hydrophobia, said to have been treated successfully, were nothing more than this extraordinary irritation of the nervous system produced by worms. I saw, myself, a case in which two eminent physicians made the diagnosis of hydrocephalus; it was that of a child, who was certainly, to all appearance, labouring under cerebral disease—for he had convulsions, coma, and dilated pupils. It was remarkable, however, in this case, that the treatment directed to the head, though early and well applied, proved totally inefficacious. A large dose of calomel was given, and some lumbrici passed; in the space of two or three hours there was an evident improvement, and the child quickly recovered.

During the course of practice I have met with several examples of affections of the respiratory organs, depending upon the irritation of worms. This affection has been long known. I recollect the case of a boy who was brought to me with an extraordinary affection of the chest. He was of a gross habit of body, of a flabby scrofulous appearance, and labouring under disease of the elbow-joint; but his chief complaint was, that he passed the night in great distress from incessant cough and wheezing. On examining the chest, I found the respiration healthy, and no other symptom of pulmonary derangement except a very slight bronchitic *râle*. On expressing my opinion of the case to the mother, she said that he was easy during the day, but that his condition was very different at night. To ascertain the truth, I took the child into the hospital, and found that her statement was substantially correct; for, from four o'clock in the afternoon until next morning, he was in a state of perfect orthopnoea, with loud, ringing, incessant cough. During the rest of the day he was free from cough, and tolerably quiet. The case was treated with calomel and ipecacuanha, tartar emetic, and other similar remedies, but the disease was rather exasperated than improved. The boy had swelled belly and constipation, and for this he was ordered to take a dose of turpentine and castor oil. He passed some worms with relief to the existing symptoms; and from the consideration of this, and the failure of the treatment for bronchitis, we were determined to persevere in the use of anthelmintic medicines, and for this purpose put the child on syrup of couchage, to be followed by castor oil draughts. He passed vast quantities of thread worms in the course of a few days, and when they had been all removed the cough disappeared altogether; but, as long as any of them remained, the symptoms of pulmonary irritation continued. There could be no doubt that this was a case of intermittent bronchial irritation from worms, for their evacuation was immediately followed by a complete cessation of cough and dyspnoea. I have also, since the foregoing, met with many other instances of a similar description. A young girl came into the Meath Hospital with chronic bronchitis, and some degree of hepatisation at the lower part of the left lung. Having heard from her friends that she was extremely subject to worms, I determined to try what would result from the use of anthelmintic

medicines, and put her on the syrup of couhage with aloetic pills. Under this treatment the cough was quickly removed, and the lower portion of the lung recovered its permeability. Here it was remarkable, that not only irritation of the bronchial mucous membrane, but even solidification of the lung, were cured by treatment calculated to remove worms. Mr. Ramsay, in his paper published in the *Medico-Chirurgical Transactions*, gives several cases of hæmoptysis from this cause. I think I have seen several cases of phthisis, where the original source of pulmonary irritation seemed to be the existence of intestinal worms.

Let me here, however, remind you that we should be cautious in attributing too much to worms as the causes of morbid symptoms. There are several reasons why you should be on your guard in this respect, one of the most obvious of which is this: it does not follow, in the first place, that the symptoms in any particular case are produced by worms; because the same cause, which may have predisposed to the formation of worms, may have produced the symptoms in question, and there may be merely a coincidence of worms and of these symptoms. Even if we look to the results of treatment, there is a great deal of doubt and difficulty. There are many cases on record which are described as cases of epilepsy from worms, and where all the symptoms have subsided under the use of anthelmintic medicines. In many of these cases we find the medicine chiefly employed has been oil of turpentine, and I need not tell you that this is an excellent remedy in many cases of epilepsy totally uncomplicated with worms. The results of such cases do not necessarily prove that worms were the source of irritation. Again, immense injury is frequently done to children in persisting in the anthelmintic treatment for the supposed existence of worms. Recollect, the prominent phenomena of worms in the intestines are irritations of the digestive system and of other functions. Now, it is very well known that these symptoms may occur with or without worms. If, then, you have a case where these phenomena are present without the co-existence of worms—and if, under a mistaken impression, you treat it with anthelmintic medicines—you inflict a double injury: you exasperate the original disease by the drastic and irritating medicines which are ordinarily used for the removal of worms, and you do an indirect injury by neglecting to adopt proper means of treatment. There is nothing more common than to see children labouring under some irritation of the digestive tube, which is mistaken for worms, purged again and again, until they get incurable enteritis or *tabes mesenterica*. When a child has foul tongue and breath, picking of the nose, diarrhœa, and turbid urine, it is a common notion that he is labouring under worms. If he gets feverish, it is said to be worm-fever, and the anthelmintic treatment is pursued with unabated vigour. Now, I believe that a great majority of such cases are, in reality, disease of the mucous surface of the intestine, and that the consequent feverishness is dependent on this state. Another reason why you should be cautious is this: in persons of an hypochondriac

habit, there is nothing more injurious than their getting the idea that they have a worm in their bowels. When once this notion gets into the head of an hypochondriac, it is generally impossible to eradicate it. Some of the most melancholy and fixed cases of hypochondriacism are produced in this way; every symptom is attributed to the worm; the patient is in a state of constant feverish anxiety about it; he talks of nothing else, and is constantly taking medicines to expel it, to the great detriment of his general health and with a manifest exacerbation of his symptoms. Medical men should be extremely cautious on this point. The patient is perhaps a female of hypochondriac and nervous habit; she has gnawing sensations about the epigastrium, which she supposes to depend upon the presence of a worm, and an injudicious practitioner favours the notion. He gives her various medicines to expel the worm; no worm is passed; she becomes more anxious, takes more medicine, and gets weak and emaciated. She then begins to think that all the nutritious matter in her body is going to support the worm, falls into a desponding state, and continues for the rest of her life an incurable hypochondriac.

We come now to consider the exciting causes of worms. On this subject I believe our knowledge is very scanty and inaccurate. The following, however, are generally looked upon as remote causes:—foul air, residence in damp and unhealthy situations, sedentary habits and want of wholesome exercise, over-feeding, the constant use of certain articles of diet—as farinaceous substances, milk, cheese, sugar, &c. An eminent authority (Bremser) asserts, as I have already stated, that unabsorbed chyle in the digestive tube constitutes the most fertile source of worms. It is a common idea, that poor diet has a strong tendency to give rise to the formation of these animals, but it has been frequently observed that worms are met with in persons who are by no means in want of nourishment; and it is said, that, in cases where nutrition has been diminished in man and other animals, the worms die. If this be the case, it would appear that, so far from being the exciting causes of worms, poor diet rather tends to favour their removal. Uncooked vegetables and fruits are also reckoned among the causes of worms, but I believe this arises from the mistaken notion that the ova of intestinal worms occur in vegetables, and, being taken with them into the stomach, are there developed, or even changed in their organisation—a position which we have already proved to have no foundation in truth. Persons who live principally on vegetable food have not been observed to labour under worms in a comparatively greater degree than those who use an animal diet. It is said that the Swiss, who consume a great deal of vegetables, are very subject to worms; but other nations, who live in a similar way, have not been remarkable for the same liability.

Worms have been stated to be occasionally epidemic. It is not very easy to determine this point, but it has been remarked that, at particular periods, these animals have been more than usually frequent and numerous. Many authors have described an epidemic

of what has been called *verminous fever*; that is to say, fever of a gastric or bilious character accompanied by worms in quantity. It is hard to say what the nature of this fever really was, and whether it might or might not be fever with irritation of the digestive apparatus, one of the consequences of which was a discharge of worms already existing. That worms are endemic, is a proposition very easily conceived; for we see it illustrated by the extraordinary prevalence of these animals in sheep which are kept in low, damp pastures. In such situations worms are met with in great abundance in the liver and other parts of these animals.

It would appear from the following remarkable case, detailed by Bremser, that the use of milk and farinaceous food predisposes to the formation of intestinal worms. This gentleman, who was physician to a monastery, and had ample opportunity of studying the habits of its inmates, was called to visit one of the oldest of the monks, who was said to be labouring under great derangement of the digestive system. On enquiry, he found that the patient had lived for sixty years in excellent health, using animal food, which, however, he had been latterly induced to change for farinaceous diet and milk. For a few days this agreed tolerably well with him, and then he began to be tormented with colicky pains, flatulence, sour eructations, and other distressing symptoms. His physician gave him some purgative medicine, and he passed a large quantity of tape-worm with relief: the treatment was persevered in, his former mode of living resumed, and he recovered quickly. This case bears strongly against the fanciful hypothesis that the ova of worms are transmitted in the act of generation; for how could it be possible that the ovum of this tape-worm, transmitted in this manner, could remain undeveloped in the system for the space of sixty years? This case derives additional interest from the fact of a change to a farinaceous diet being apparently connected with the formation of worms.

Another remarkable case is given by the same author. The patient was a married female who had twelve children—six boys and six girls. This woman observed, that whenever she was pregnant of a girl she had a great longing for milk and farinaceous food, and lived on these articles of diet almost exclusively. After living in this way for some time, she uniformly got an attack of worms; and this, as well as the longing for vegetables, coincided with the birth of a female child so invariably, that she was able to tell with certainty whether the child she carried was a male or a female. This is a singular and well authenticated fact.

We come now to the treatment of worms. Generally speaking, this is extremely simple—the principles of treatment in the various kinds of intestinal worms being nearly the same. Simple as they are, however, some persons entertain false notions respecting them. They appear to think that all they have to do is to evacuate the worms; and, having accomplished this, they rest satisfied, and take no steps to prevent their recurrence. But the mere evacuation of worms is no proof of a cure; to effect this you must prevent their

return. From what you have learned with respect to their exciting causes, you will be able to give such directions as to the patient's mode of living as will obviate their recurrence; and, with regard to the means to be adopted for removing them, we may divide them into the following:—We have, in the first place, what is called the mechanical treatment; next, the specific; and, lastly, the purgative treatment. The first and last are nearly connected. For instance, purgatives appear to act in the same way as mechanical anthelmintics, by irritating the mucous surface of the intestine and the worm, and thus causing its dislodgment and expulsion.

Among the principal mechanical anthelmintics are filings of tin, couhage, powdered charcoal, and crude mercury; among the specific are a variety of substances, most of which have a strong and peculiar smell. This is a very curious fact. Valerian, asafoetida, camphor, ether, and other odorous substances, have been found to be anthelmintic; and the *Geoffræa inermis*, which has been employed for this purpose, is remarkable for its strong, unpleasant odour. The same thing may be said of tobacco, the oil of chenopodium or wormseed, garlic, *artemisia absinthium*, and many others. With respect to purgatives, there is not one in the whole list, particularly those of the drastic kind, which may not be looked upon as an anthelmintic.

It is the opinion of the most eminent men, that the thread-worm is the most difficult to expel, because they are generated with an extraordinary rapidity, and accumulate in a very short space of time. You are satisfied of their existence, have seen them in the alvine discharges, and the patient has all the ordinary symptoms. Well, what is the best way of getting rid of them? You shall commence by the exhibition of a mercurial. It is difficult to explain why it is that mercury has such an effect in removing these worms, but the experience of the best practitioners can be adduced in proof of its efficacy. The statements of Dr. Latham, of London, and of many practitioners in this country and on the continent, go to prove this. In whatever way it acts, mercury appears to be a powerful anthelmintic; and it is a fact, that these worms have been expelled where it was given in very small doses, and not sufficient to operate as a purgative. The best plan is, first, to give a mercurial purgative, and then to have recourse to the mechanical treatment—giving, with this view, the syrup of couhage, one of the most efficacious of this class. It is a remedy which is easily managed, and will do no harm; for, though it produces violent itching when applied to the cutaneous surface, it produces very little sensible effect on the intestinal mucous membrane. The form which I employ is the following:—Take of the hairs of the *dolichos pruriens* one scruple, syrup of orange-peel an ounce; of this an electuary or syrup is to be made, of which you may give a child a tea-spoonful three times a day. This is the remedy on which the West Indian practitioners, who have frequently to treat this affection in the negroes, place the greatest reliance; and you will find that, if you employ it, a vast number of worms will be often passed. It

should be continued for two or three days, and then a purgative must be given, after the operation of which it may be again resumed if necessary. An excellent adjuvant to this is the use of aloetic injections, composed of two parts of milk and one of the decoction of aloes. In this way you will be able to remove a vast quantity of these little animals from the rectum. It has also been observed, that injections of cold fresh or salt water have a great power in promoting their expulsion. Bremser mentions, that, in cases where these worms pass from the rectum into the vagina in females, and excite irritation, there is nothing so effectual in destroying them as injections of cold water and vinegar. This you should bear in mind. You should also remember, in the case of administration of syrup of couhage, to give strict orders not to let any of it drop on the child's skin, as it would excite a great deal of irritation. You should forewarn the attendants of its effects on the skin; and if any of it should be spilled on the hands, neck, or face, the best thing is to wipe and wash the part well, and then rub it with a little almond oil.

For the expulsion of lumbrici there is nothing so successful as the ordinary purgative treatment. A bolus, composed of calomel, rhubarb, and jalap, will answer this purpose extremely well; you may also use the syrup of couhage with much advantage. Bremser gives a formula for an electuary, which I have not tried, but have no doubt of its value, for it appears to combine all the qualities of a good vermifuge electuary. It is made as follows:—Take of the seeds of santonicum, and of the flowers and leaves of tansy, reduced to powder, each half an ounce. Here you have two anthelmintics of the specific kind. Add to these two drams of powdered valerian: here is another. You then combine with these two drams of sulphate of potass and a dram and a half of jalap: these are purgatives. You then make them up into an electuary with syrup of squill, which is also an anthelmintic of the specific kind. Of this electuary two or three tea-spoonsful are to be taken during the course of a day. Bremser states that this combination is of great value, particularly against lumbrici and tape-worm.

The treatment of tape-worm is not difficult. All the specific and mechanical anthelmintics are useful in promoting its expulsion, but there is nothing which appears to have such a powerful effect as full doses of turpentine and castor oil. This constitutes the best remedy we possess against the tænia; but, if you wish to get rid of it entirely, you must give the turpentine in full doses. You will frequently be astonished at the vast quantities of this worm which will be passed. When you give turpentine, it is safer to order a full dose of it; for, if it be given in small quantities, it is very apt to irritate the urinary organs. Half an ounce of turpentine, with the same quantity of castor oil, form an efficacious though very disagreeable draught. You may, however, obviate its nauseousness by the addition of a small quantity of camphorated tincture of opium and mucilage of gum arabic. The celebrated empyreumatic oil of Chabert is, in my mind, nothing more than a modification of

the turpentine. This is the remedy which Bremser looks upon as most efficacious against the tape-worm. You have all, I presume, heard of the animal oil of Dippel—the oil which is produced by the distillation of bones or hart's-horn shavings. To one part of this are added three parts of turpentine; these are left to combine for four days and then distilled; the first three parts of oil which come over are called the empyreumatic oil of Chabert. It is an exceedingly nauseous remedy, has a most disgusting smell, and is seldom used in this country. Bremser recommends it to be taken in doses of a tea-spoonful three times a day. Some persons who have tried it have assured me that it is extremely difficult to be taken, and that it excites a train of most disagreeable abdominal sensations. Bremser, however, thinks highly of it; he is in the habit of directing his patients to take it for three or four successive days, then to omit for a day or two, and then to return to it again; and he says that it not only succeeds in evacuating the worm, but also in preventing its return. In addition to this, he recommends the use of a fortifying tincture, which I think very useful in worm cases. It is a combination of one of the salts of iron with a preparation of aloes. If you take equal parts of the muriated tincture of iron and tincture of aloes, you will have a remedy somewhat similar to the strengthening tincture of Bremser. Twenty drops of this mixture, taken three or four times a day, will prevent the recurrence of worms.

In our next lecture, I shall take up the subject of painters' colic, and some other affections connected with the viscera of the abdomen, and then pass on to the consideration of thoracic diseases.

LECTURE XXI.

Painters' colic—Effect of metallic poisons on the nervous system—Symptoms of painters' colic—Pathology of neuroses—Action of lead on the system—Abdominal and cerebral symptoms—Species of painters' colic—Dr. Thompson's researches on lead—Effects of in animals—Effects of on the generative system.

A great deal of our time has been already occupied with the diseases of the digestive system—in fact, much more than I originally intended; the only apology I have to make for this, is the deep and paramount importance of the subject. Before I quit this part of the course, there are yet one or two subjects to which I shall briefly allude, namely, peritonitis and painters' colic. With respect to the first of these diseases, I shall say but very little; the ordinary form of peritonitis is a disease so well known, and so fully treated of in books, that it would be only a waste of time for me to go over it; and with respect to peritonitis from perforation, all the original

information I could communicate on this part of the subject, may be seen in one of my published clinical lectures, and in the article on *Peritonitis from Perforation*, in the London Cyclopædia of Practical Medicine. The ordinary form of peritonitis has been described in this work by Dr. M^r Adam, the disease from perforation by myself. I shall therefore pass over this subject, and proceed to the consideration of a very interesting disease—painters' colic.

This disease is called painters' colic, from the circumstance of house-painters being extremely liable to it from coming into frequent contact with the poison of lead. Its synonyms are numerous, dry colic, Saturnine colic, rachialgia metallica, Devonshire colic, &c., &c.

Painters' colic is an example of the effects of a metallic poison on the nervous system. There are certain metals which produce a powerful effect on the system, not by means of their corrosive properties, or by any direct action on the surface to which they are applied, but by a peculiar impression made upon the nervous system. Thus we find that mercury, under certain circumstances, will give rise to a very singular nervous disease; arsenic may be introduced into the system in such a way as to produce symptoms of nervous lesion; copper exercises a similar morbid influence, and the effects of lead are universally known. I do not mean to say that all these metals produce similar effects on the economy, for this is not the case; but there is one point of agreement between them, that all may produce symptoms which are called nervous or neurotic, and the diseases thus produced are classed among the neuroses. What is the meaning of this term neurosis? *A lesion of nervous function, more or less complete, occurring independently of any demonstrable organic change.* A neurosis, then, is an alteration in the functions of the nerves of organic and animal life, the nature of which alteration we cannot understand, neither can it be demonstrated by the knife, nor by any examination of the state of the nervous tissue. In other words, a person will die with the symptoms of a neurosis; and when you come to examine the body, you will be unable to detect, in the minute ramifications of the nerves, the trunks, or the nervous centres, any appreciable lesion.

Diseases of this description have been divided into two classes—active and passive neuroses. Active neuroses signify an increase or exaltation in the nervous function; passive neuroses are those in which there is a diminution of nervous energy; in both, there is an absence of perceptible organic change. Take, for instance, an example from the nerves of animal life: a case of convulsions, independent of organic disease, is an example of the active neurosis; a case of paralysis, under similar circumstances, is an example of the passive. In the former, there is an exaltation of the nervous function, which is reflected upon the muscular system; in the latter, there is a diminution, producing a partial or total loss of the power of motion. It has been asserted, by eminent physiologists, that passive neurosis can only exist in the organs of the life of

relation, because the functions of the ganglionic system, which presides over organic life, cease only at the death of the individual. But there may be such a thing as semi-paralysis of the organs to which the ganglionic nerves are distributed; and hence we may have passive neuroses of the system of organic as well as of animal life. We get a good idea of these neurotic affections, by taking some of the most remarkable instances of this kind. Hydrophobia is a remarkable instance of excessive lesion of the nervous function, without any known organic change; so is tetanus, and so are some forms of apoplexy, convulsions, and mania. Here we have violent irritations of the nervous system, in which there is no perceptible organic change; and where the only information we derive from pathological anatomy is of a negative character, telling us what these diseases are not, and leaving us, as to their actual nature, as much in the dark as ever. We find by dissection that hydrophobia, and tetanus, and hysteria, and convulsions, and apoplexy, are not caused by inflammation of the brain or spinal marrow, and that is all. Hydrophobia, tetanus, convulsions, and hysteria, are instances of active neurosis; paralysis and apoplexy, without any known cerebral disease, are looked upon as examples of the passive kind, because they present either a diminution or abolition of the nervous function.

In the present state of medical science, we must admit this division of the affections of the nervous system into diseases with and without perceptible organic lesion. I grant that it is very difficult, when we come to consider alterations in the functions of parts, to conceive how such changes could be effected without molecular alteration, or that the brain could be deranged in its functions, without some change of this kind. We are, however, compelled to consider such functional alterations of the nerves as changes with which we are unable to connect any process of hardening, or softening, or anomia, or congestion, or, in fact, any *known* pathological condition. Rostan is of opinion that all diseases are organic; that is to say, that they are produced by some molecular change, and this, he says, should be the basis of medicine. Unfortunately for medicine, it has been given so many bases, that it sometimes knows not what leg to stand on.

But to return to our subject. Painters' colic is an example of a neurosis, that is to say, it is a lesion of the nervous function, unconnected with any known pathological alteration. It presents, commonly, two periods—the first exhibiting the phenomena of active, the second of passive, neurosis; or, in other words, the signs of exaltation of the nervous function precede those of depression. In the majority of cases, we find the first stage of this affection characterised by violent spasm, pain and convulsions, symptoms indicative of active nervous lesion; whereas in the second stage we have paralysis, the diagnostic mark of the passive kind. This is the order in which the phenomena of painters' colic are generally met with, but in some cases the first stage is either very imperfectly shadowed out, or even entirely wanting; the paralysis comes on in

an insidious manner, and without being ushered in by any symptoms of exaltation of the nervous function.

In this country, the most common victims to this disease are painters, who are much in the habit of working in white lead, and when you are connected with the management of any public medical institution (as I hope you will all be), you will often have to treat cases of this description. In Dublin, and all large cities, it is an exceedingly common affection, and the patients are for the most part house-painters. Next to these, the persons who are most subject to it are plumbers, and those who are employed in the melting of lead.

When the poisonous particles of lead enter the system in a highly volatilised state, its morbid effects are more certain and extensive. Every house-painter will tell you that the kind of work which is most likely to produce a deleterious effect, is painting "the *dead white*," or, as it has been termed, *statuary white*. In doing this, they use white lead combined with a large proportion of the oil of turpentine; and, in order to produce the intended effect, they are in the habit of excluding the air as much as possible. By means of the turpentine and the warm temperature of a close room, the lead is volatilised, and, in this state, appears to have an extraordinary power of impregnating the system. Some of the very worst cases of painters' colic are produced in this way. Painting in the open air, even where the same preparation is employed, is comparatively harmless. A poor fellow, who was for a considerable time under my care, assured me that he had escaped for twenty years, and was convinced that he would have enjoyed a much longer immunity, had he not been put to work at the *statuary white* in a close room.

With respect to plumbers, it is now ascertained that this disease is of comparatively rare occurrence among them; and the reason of this is, that they generally work in the open air, or in well ventilated apartments, and have now but little to do with the actual manufacture of lead. The kind of lead which they generally use, sheet and pipe lead, is furnished from the manufactories, and their occupation principally consists in the moulding and soldering of it. We very seldom now see a plumber labouring under colic.

Painters' colic may be observed under a great variety of forms; but, for the convenience of studying the disease, we may divide these varieties into four classes. In the first, we have the phenomena of simple colic, without any obvious or marked symptoms of bilious, gastric, or cerebral derangement. In the second variety, the disease assumes a more decided character; the colic is complicated with symptoms of fever of a gastric character, the pain in the belly is more acute, the constipation more obstinate; there is pain and difficulty in going to stool, nausea and vomiting, with occasional headache, dyspnoea, and sense of constriction about the præcordia; the belly is hard and retracted, and there is often pain in passing urine. In the third variety we have a more formidable array of symptoms. The functions of the brain and spinal marrow are deranged; there are wandering pains in the extremities; and

the patient has frequent attacks of violent convulsions, resembling those of epilepsy. He also labours under the abdominal symptoms, but in this stage they are not so well marked, or so distinct, as in the former; the lesions of the functions of the cerebro-spinal system begin now to exhibit a greater degree of preponderance, and claim the principal share of the attention of a symptomatologist. In the fourth variety there is paralysis, without being preceded by the ordinary symptoms of abdominal or cerebral derangement. A medical friend of mine met with a case of this kind not long since. He was called to visit a child who had lost the use of his limbs. He went, and found the child lying in bed perfectly quiet and easy, his intellect sound, and his spirits good, but labouring under complete paralysis of all his limbs. He enquired minutely into the history of the case, and made a most scrupulous examination, but, from all he could see or learn, there was not the slightest ground to suspect disease of the brain or spinal cord. There had never been any symptoms of colic. He was puzzled with the case, and tried one thing after another without benefit. At length he found out that the child's father was a painter by trade, and this led him to suspect that the symptoms might have some connection with the poison of lead. He enquired; and was told by the mother, that a quantity of white paint had latterly been kept in the room, and that it was impossible to keep the child from it. He instantly had the paint removed, a free current of air admitted into the room, and by the use of purgatives, assisted by stimulating frictions, the child recovered.

The following is the order of symptoms generally observed in this disease. First, we have the precursory, denoted by pain and sensation of weight about the epigastrium; a weak, small pulse; general languor and weakness of the muscular system; want of appetite; cold, clammy skin; a tremulous and coated tongue. At this period there is sometimes diarrhœa. Then comes some exciting cause, exposure to cold or wet, excess in eating or drinking, and the disease sets in with more or less intensity. The patient is attacked with dreadful pain in the belly, which differs from the pain of inflammation in this, that, so far from being increased by pressure, it is in most cases relieved. In fact, so decided is the relief produced in this way, that there is a case on record in which the patient used to get the greatest ease by making one of his fellow-workmen stand upon his belly. This relief from pressure is very generally observed in colicky affections. Indeed, so general is it, that you will hear it frequently stated, that all cases of colic are relieved by pressure. This, however, is not invariably true; for I have seen cases where the patients could not bear pressure, and where it required a careful examination to distinguish the symptoms from those of inflammation. The pain is of a twisting kind, and felt about the umbilicus; and, in connection with this, there is scanty urine, with more or less pain in passing it, obstinate constipation, and a tense, hard, retracted state of the belly, from the violent contraction of its muscles. The upper portion of the belly

is sometimes more retracted than the lower, and the pulsations of the abdominal aorta are unusually distinct. The pain remits, and then becomes exacerbated, and the patient's countenance is expressive of acute suffering. In that form of the disease where there is a complication of gastric or bilious symptoms, the patient has a semi-jaundiced look, a hot, moist skin, quick pulse, foul tongue, vomiting, hiccup, thirst, and epigastric tenderness.

In the third form, the chief force of the poison seems to be directed against the brain and spinal cord. There is vertigo, headache, stupor, and sometimes delirium; the patient has fits resembling those of epilepsy, but of longer duration, and violent convulsions, which sometimes continue with unabated intensity for twelve, or even twenty-four hours. You will see those unfortunate creatures rolling and twisting in every form, sometimes doubled forwards, sometimes in a state of perfect opisthotonos, sometimes moving their limbs with the convulsive action of an epileptic, and foaming at the mouth. In addition to this, it is stated, in the descriptions of this disease, that the patient loses his sight, and becomes amaurotic; this I can confirm, for I have seen it more than once. It is a curious fact, too, that this blindness may come on before the other cerebral symptoms are developed. I recollect a case in which one of the first symptoms was blindness. The patient happened one evening to be indulging himself in whiskey punch, and was in a fair way of getting comfortably drunk, when, unfortunately, he found that all of a sudden he could neither see single nor double. He groped about in a very disconsolate state for his glass, but not finding it, and finding, at the same time, that he had lost his sight, he came to the hospital next morning, and shortly after his admission, had a violent attack of convulsion. In cases of this kind, I have generally found the pupils contracted. The patients toss about in bed, and are frequently found lying with their heads turned towards the foot of the bed. In some cases, the breathing has been stertorous for a length of time, and the head fixed, but the fingers and hands were flexible. I have seen cases in which the coma disappeared, and was followed by perfect blindness, lasting for two or three days, and then yielding to treatment.

These symptoms, striking and extraordinary as they are, do not seem to depend on the same state of the brain as cases of other diseases which are accompanied by sanguineous determination to that organ. The reason I make this assertion is, that many of the most violent nervous symptoms, including profound coma, subside under the use of a stimulant treatment. I think we may look upon these symptoms as similar to what are termed the symptoms of the *nervous apoplexy* of the ancients. A case of this kind, which occurred in the Meath Hospital, is deserving of notice, from the singular effect produced by treatment. The patient was in a state of profound coma, but the head was cool, and the arteries had no inordinate pulsation. If this was a case which presented the other symptoms of apoplexy, I would have prescribed bleeding, leeches, and cold applications. But I reasoned thus—Here is a case in

which there is no evidence of the existence of inflammatory action. Opium has been found to relieve the abdominal symptoms of the disease—may it not also relieve the cerebral? I ordered the patient to have a free dose of laudanum in camphor mixture. In a few hours he awoke, sat up in his bed, and next morning we found the symptoms of coma had completely disappeared. In two other cases of a similar kind, I have given opium and carbonate of ammonia with the most favourable result.

Dr. Clutterbuck mentions a peculiar symptom of this disease—a kind of gouty inflammation attacking the great toe, and followed by relief. I have not seen this. He states that the first joint of the great toe becomes red, hot, painful, and swollen, and that this remits by day and returns again at night. I have never seen this, nor have I ever seen those hard tubercles on the tendons in various parts of the body, which some authors have described.

After these symptoms, we come to a new class, namely, the passive, characterised by paralysis of the muscles of animal life. It is remarkable that this paralysis seems to be principally a paralysis of motion, and that the power of sensation is seldom or never impaired. Generally speaking, the upper are more subject to paralysis than the lower extremities, and the right than the left arm. The latter circumstance is explained by assuming that the direct influence of the poison is more applied to the right arm. The paralysis of the arm is also frequently partial; the extensors lose their power, but the flexors do not in so great a degree. You will see a patient with his arm hanging by his side as if it were dead, but if you give him any thing to hold he can grasp it firmly. I have known painters continuing to work with a semi-paralysed arm. There is also an atrophied condition of the affected part; and this sometimes comes on with such rapidity, that, in the space of a week or ten days, the affected limb will be scarcely half as bulky as the corresponding one. We cannot account for this remarkable emaciation on the principle of loss of motion alone, for the short space of time in which it occurs, in many instances, is opposed to our entertaining such an opinion, and we must look for some other explanation. On this point, science affords us no satisfactory information.

This disease, notwithstanding all its terrible array of symptoms, is very seldom fatal. Hence the uncertainty which long prevailed as to its pathological nature. In the great majority of cases, where a dissection was made, the patients died of some other disease, which either occurred during its course, or had preceded it. All that appears to be established at present is, that there is no known organic change of the nervous system connected with this disease; that it occurs in all its forms without the co-existence of organic lesion, and that its exciting cause is the poison of lead.

It was formerly supposed that all the preparations of lead, whether applied externally, or used internally, were capable of producing colic, but this doctrine is at present considered very questionable. It was thought that metallic lead, and all its salts, were capable of causing the disease; but the morbid influence of this metal is now

restricted by the best chemists and pathologists chiefly to its carbonate. This opinion, I believe, was first put forward by Dr. A. T. Thomson, the author of the London Dispensatory, in an interesting paper published by him in the tenth volume of the *Medico-Chirurgical Transactions*. The object of this paper is to prove that, of all the preparations of lead employed in pharmaceutical and other purposes, the carbonate is that which is chiefly poisonous, and that the acetate and sub-acetate are comparatively harmless.

You have all, I am convinced, heard of cases of colic produced by the external use of the acetate of lead, and you will see some cases in proof of this opinion in Darwin's *Zoonomia*, and other writings. There is a case on record of a woman, who having poulticed her ankle with this preparation, for the cure of a sprain, got colic and fell into a state of marasmus. I knew of a deplorable case of burn affecting the abdominal integuments, which was treated with a solution of the acetate of lead. After using it for a fortnight or more, symptoms of colic came on, which not being recognised, the lead wash was continued, and the woman died in great agony. Dr. Thomson explains all this in a very satisfactory way. He shows that the solution of acetate of lead, when exposed to the air, attracts a quantity of carbonic acid, and is thus converted into a carbonate; of this I have very little doubt, for you will find that, by exposing a solution of the acetate of lead to the full influence of the air, the carbonate will gradually be deposited in the shape of a white powder. In the same way we can understand why it is that a solution of the acetate of lead, added to fermenting poultices, may be converted into a carbonate by the carbonic acid which is evolved. It is also a fact, that the acetate can be used internally for a long time without producing any thing like deleterious effects. I have given it for weeks together in full doses, without its having been ever followed by colic, or any symptoms characteristic of the absorption of a poisonous matter. There are cases on record where as much as six drams of this salt have been taken internally without producing any sensible morbid effect. As far as my experience goes, all those cases, in which the medical use of the acetate of lead has been attended with disagreeable symptoms, were cases in which it had been used as an external application. There were two cases in the Meath Hospital in which this medicine was used externally, in which colic, and other indications of poisonous absorption, took place, but not a single one in which its internal employment had been injurious. An excellent practical rule is laid down by Dr. Thomson, that, where you wish to employ the acetate of lead internally, you should take care to combine it with diluted acetic acid. Of the two combinations of lead with acetic acid, the sub-acetate is most liable to be decomposed and converted into a carbonate, so that, if you prevent this by mixing with the sub-acetate, or acetate, a certain quantity of distilled vinegar, there will be little or no chance of unpleasant symptoms being produced, even where the medicine is given in very considerable doses. We are, therefore,

I think, justified in concluding that it is the carbonate of lead which is productive of poisonous effects; and that where bad symptoms have resulted from the use of the acetate, it was in consequence of its being converted into a carbonate. I must, however, remark, that it has not been sufficiently proved, as yet, that the use of the acetate is *perfectly safe*.

It is an interesting fact, that many of the lower classes of animals are subject to this disease. Burserius was one of the first authors who directed the attention of medical men to this singular occurrence. I have got from my father an abstract of some observations made by him on this subject, during a visit to the lead hills in Scotland. He found that, in the pastures among these hills, and in their immediate vicinity, cows, horses, sheep, dogs, and even poultry, were subject to colic from lead. The symptoms, also, in these animals were observed by him to bear a very close analogy to those of the human subject. Thus, for instance, in cows there was obstinate constipation with suppression of urine; the poor animals seemed to suffer from violent twisting pain in the belly, and sometimes were thrown into a state of furious excitement, running wildly across the country. He learned, also, that during that period it was calculated that at least one tenth of the cows in this situation had died of the effects of the poisonous absorption of lead. One of the most ordinary precursory symptoms, was the animal becoming what is called hide-bound; this was followed by obstinate costiveness, and there was much apparent suffering, with panting, starting, and slavering from the mouth. Where the cerebral symptoms were most prominent, the signs of abdominal irritation were by no means distinct; and this, as I have remarked, is the case in the human subject. In some, who had the head affected, and ran wildly through the country, the secretion of milk was stopped; and this accords, too, with the effect of lead on the human female. Another remarkable circumstance is, that animals living in the vicinity of these lead hills have exceedingly difficult labours. Sheep are subject to epileptic convulsions and paralysis; dogs have the head principally affected, they run across the country slavering at the mouth, as if in a state of hydrophobia, but they do not bite, and are in all respects perfectly harmless. In barn-door fowl, the generative function was injured, and the hens reared or brought there ceased to lay eggs.

There is one fact, mentioned in these observations, which tends to confirm the opinion of Dr. A. T. Thomson, that the poisonous effects of lead are produced chiefly by the carbonate. A distance of very few miles from the valley, renders animals quite free from any liability to the disease; but if they should happen to stray into the immediate neighbourhood, and particularly into a portion of low ground, flooded during the winter months by a river which runs along the valley from the mines, and which, in all probability, leaves behind an efflorescence of the carbonate of lead, they are very liable to be affected with colic. It is said, also, that the poison is produced by the volatilisation of lead in the smelting houses, the vapours of which are carried down the valley and through the

neighbouring parts. Be this as it may, the Gaelic name of the valley signifies, the *poisonous vale*; and, as it is very probable that this name had been given in consequence of the deleterious qualities of the place long before the establishment of lead works, it tends strongly to favour the opinion that it is the water which contains the poison.

The mode of cure employed by the shepherds in this place, is to give strong purgative injections, and remove the cattle from the influence of the poison, by sending them to new and healthy pastures. In this way they frequently recover; and if we look to the cause of the disease, its symptoms, or mode of cure, we shall observe a striking analogy between it and the colic from lead in the human subject. I shall conclude this subject at my next lecture, and then go on to diseases of the chest.

LECTURE XXII.

Pathology of painters' colic—Researches on the state of the nervous and digestive systems—Treatment—Use of narcotics, purgatives, tobacco, &c. &c.—Treatment of paralysis from lead—Efficacy of strychnine and brucine—Colic from copper—Poisonous effects of mercury—Remarkable case—Affection of the respiratory muscles.

We were occupied at our last lecture in considering the symptoms of painters' colic. I mentioned that it occurs under a variety of forms; that the symptoms are to be attributed to a lesion of nervous function independent of any known organic change; and that the same disease may be seen in animals which have been exposed to the poison of lead. There are some other facts connected with this disease, which should not be passed over, and which I am anxious to lay before you previously to entering upon the treatment.

You will recollect that I introduced the subject by stating that painters' colic belonged to the class *neuroses*, and that I endeavoured to show that this implied a lesion of function of any part or viscus of the body, frequently characterised by the most decided departure from the natural condition, and yet unaccompanied by perceptible organic change. I said, also, that it was hard to suppose the existence of great functional alteration, *without any molecular change*; but that, in the present state of science, we are compelled, for the want of a better term, to call these affections *neuroses*, in contradistinction to diseases in which there is organic lesion visible. To illustrate this point, take an example from two different cases. In one case of what is called *dyspepsia*, we have inflammatory, or, at least, sub-inflammatory derangement of the stomach: here the disease is traceable to organic change; in another we have symptoms of nearly the same character, and yet there is no organic lesion. Painters' colic comes under the latter head;

we observe symptoms of excessive functional lesion, but dissection does not exhibit any organic change. Pathological anatomy tells us what it is not, and we arrive merely at a negative knowledge of its nature. We have decided proofs of extraordinary lesions of the nervous system, and yet, when we come to the post mortem examination, we cannot find any visible change to account for these striking phenomena.

The old pathologists maintained that spasm of the intestines was the principal cause of the disease, and attributed the symptoms to their contraction. This opinion appears to have some foundation, when we consider the violent symptoms of colic which accompany this affection. Dubois de Rochfort has mentioned, that in such cases he has found intussusception of the intestines. De Haen says that contractions of the colon are very common; and several authors make the same assertion. The results of more modern observation, however, are against these opinions. I have told you already, that in consequence of this disease seldom or never proving fatal, there is a degree of doubt attached to its pathology; but it is an interesting fact, that where death from other causes has occurred during the existence of painters' colic, the digestive tube has been found either in its healthy state, or with a few detached spots of vascularity, without any decided inflammatory character, and totally insufficient to account for the symptoms. This, which is all that pathological anatomy reveals, may be considered as purely accidental, and only of occasional occurrence, so that we are compelled to look upon the disease as one in which there is great lesion of function without any organic alteration.

In the hospital of La Charité, at Paris, a vast number of cases of painters' colic have been treated. In the space of eight years five hundred cases of this description have been admitted; out of these, five died while labouring under the disease; and the following is an abstract of the appearances observed on dissection. In the first case, there was rupture of an aneurism of the abdominal aorta, and the patient sank from loss of blood. On examination, the digestive tube was found in the natural and healthy condition—there was neither *vascularity nor contraction*. The subject of the second case died of apoplexy. The whole intestinal canal was found healthy, and, contrary to the doctrines of the school of Broussais, there was neither congestion nor vascularity. In the third case, the patient had fits of an epileptic character, in one of which he expired. The colon exhibited a slight degree of redness, but quite insufficient to explain the symptoms during life. In the fourth, the cause of death was the same, and, on dissection, the tube was found healthy. Another patient, after recovering from the symptoms of painters' colic, got a sudden attack of asphyxia and died. His body was examined, but there was no trace of disease in the colon or any other part of the intestinal canal. Here we have five cases in which there was either no disease at all in the digestive tube, or, if there was any, the amount was quite insufficient to account for the symptoms. Louis, in a memoir which he has published, on

sudden and unexpected deaths, gives a case of this disease where death occurred suddenly on the eighth day. The intestines were found to be in a healthy condition. Martinet gives two cases of persons who died of the cerebral symptoms while labouring under this disease: here, also, the tube was in the normal state. Thus we have eight cases with dissections detailed by various authors, all men of high professional celebrity, having no theory to support, and all agreeing in the statement that there is little or no appreciable lesion of the digestive tube; that in the majority of cases it is in a state of health; that no contraction exists; and that such morbid appearances as have been found must be looked on as accidental.

There is one interesting circumstance in these cases which deserves to be noticed. With the exception of the first and fifth cases, all the patients presented that form of the disease in which the functions of the brain are decidedly injured. Here it seems probable that the cause of death was excessive irritation of the nervous system. Now, in the observations I made on the cases which were treated at the Meath Hospital, you will recollect I stated that where the cerebral symptoms were predominant the abdominal were more or less indistinct and latent, and that the cause of indistinctness, or even total absence, of these might be owing to the force of the disease being thrown upon the brain and spinal cord. Such was the case in the instances above recited, and such we have also seen to be the result in the case of those animals of an inferior order that have been exposed to the poison of lead. How far the predominance of cerebral excitement may explain the want of appearances of disease in the digestive tube may be a subject of consideration.

What is the state of science with respect to the brain and spinal marrow? Allow me here to call to your recollection the symptoms of functional derangement of the nervous centres, the coma, the violent convulsions, the amaurosis, the deafness, the delirium, the paralysis. All these are violent symptoms, and you would naturally expect to find them connected with some sensible alteration, some congestion, or inflammation, or ramollissement. But nothing of this kind can be discovered. In all the cases where death occurred under such circumstances, at La Charité, with the exception of some slight appearances of cerebral lesion in the second, there was no perceptible disease in the brain or spinal cord. The membranes and substance of the brain presented their normal condition; there was little or no fluid in the ventricles; the spinal cord was healthy and natural in consistence and colour, and there was no effusion into its sheath. All these circumstances led to the conclusion that painters' colic is essentially a neurosis. Observe, too, how interesting it is to connect the circumstance of the absence of organic change with the singular fact which I mentioned in my last lecture, that the comatose symptoms of this affection may be treated with stimulants and opiates. Where we have coma with congestion of the brain, opium has the effect of increasing the

symptoms; here it was found to have a contrary effect. So that our experience and the results of pathological anatomy, as far as they go, appear to square exactly. We see, then, that painters' colic is not inflammation of the intestines, or of the brain, or of the spinal cord, and this information, though of a negative character, possesses considerable value in a practical point of view. I do not know any cases of what have been termed neuroses, in which the bearings of pathological research on practice are so extensive and so satisfactory.

It is a fortunate circumstance that this disease is seldom fatal, and it is some consolation to think that, although the patient's sufferings are dreadful and often protracted, there is little danger of life, and that the complaint is almost always amenable to judicious treatment. I have been for some years in the habit of treating it in a routine way, and can speak from experience of its success—of course this treatment is to be modified by circumstances. Suppose a patient applied to you with violent pain about the navel, a hard and retracted state of the abdomen, obstinate costiveness, and the other symptoms which characterise an attack of painters' colic; the first thing I would advise you to do is to prescribe a full opiate. Many persons would object to this, and say that there is constipation enough already, and that opening the bowels would be much more likely to give relief. But opium does not here add to the constipation; indeed, so far from doing this, it sometimes acts as a laxative. At all events, it is a remedy which is perfectly unobjectionable. Give, then, in the first place, a full opiate, it will have the effect of relieving the patient's sufferings, and will enable you to gain time for the employment of other means. The next thing is to place the patient in a hip bath, and keep him in it as long as possible. Do not neglect this, for I know of nothing that gives more decided relief. I have often seen cases where the patient was quite easy while he remained in the bath, but experienced a return of the pain as soon as he left it. If you have no means of procuring a bath in this way; the next best thing is to have recourse to emollient stupes containing some narcotic, after the manner first introduced by my colleague, Dr. Graves. One of the best of this kind is the tobacco stupe; if you cannot get this you may employ poppy-heads for the same purpose. The tobacco stupe is much better than the tobacco injection, because its effect can be more easily regulated, but in violent cases I am in the habit of combining both, employing the stupe during the paroxysms of pain, and throwing up a tobacco enema every four or six hours, until a decided impression has been made on the symptoms. In the success which has attended my distinguished friend Dr. O'Beirne's treatment of tetanus by the use of tobacco we see an analogous effect. In this way you will succeed in giving relief; you should also prescribe a brisk cathartic, and this you may do without any fear of injuring the patient, or exciting intestinal inflammation. The insensibility of the intestines to the stimulus of even powerful purgatives is a curious feature in this disease, and bears strongly against the idea of

its being connected with any inflammatory condition of the tube. In the Hospital La Charité the treatment is routine; it consists of an emeto-purgative plan, which is continued day after day until the symptoms yield. The purgatives we employ in the Meath Hospital is croton oil, combined with castor oil and mucilage, or given in the form of pill. When the bowels have been freely acted on, the case generally goes on well. After the bowels have been opened, we continue the employment of the hip bath, the narcotic stupes, and anodyne injections, taking care at the same time to persevere in the use of purgatives.

Andral makes a good remark on this point:—"Here (says he) are cases in which, from some peculiar alteration in the state of innervation, the mucous surface of the bowels is rendered less sensible than in its ordinary condition, and can bear freely the stimulus of powerful purgatives. May not this condition also occur in other states of the economy? We are, therefore, led to conclude that purgatives are not, in all cases, direct stimulants."

Painters' colic has been treated in Paris by bleeding and leeching, but this has not been found so successful as the ordinary purgative plan. I have never seen a case in which general bleeding seemed to be called for except one, and this was a most violent case which had resisted the ordinary means of treatment for forty-eight hours. I recommended bleeding from its well known anti-spasmodic power; a quantity of blood was taken, and soon after the purgatives began to act, and the patient got relief. With respect to leeches, I have employed them only in those cases which are accompanied with symptoms of fever and gastric irritation; where there is quick pulse, hot skin, foul tongue, thirst, vomiting, and epigastric tenderness. In such cases I have applied leeches, but my experience of them is, that the relief afforded is by no means so great, or so decided, as in cases of intestinal inflammation, and it is a mode of treatment which I do not by any means rely upon for removing the disease.

After the violent symptoms have been subdued, the next thing you have to consider is, whether there is any paralytic affection, and how this is to be treated. If the disease be severe or of considerable duration, you may look for paralysis of one or both of the upper extremities with a good deal of certainty. This part of the subject, I believe, more properly belongs to the consideration of nervous affections, but, as I have gone so far into the treatment of painters' colic, I may as well give the whole together. The paralysis which follows this disease is different from that which is the result of apoplexy; it is a neurosis of the passive kind, and to be treated as such. The patient, some time after the occurrence of the usual symptoms of colic from lead, begins to complain of weakness in his arm, he feels some difficulty in extending his fingers or raising his hand to his head, and then the symptoms become more marked. The arm and fore-arm become rapidly atrophied, the paralysis principally affects the extensors, while the flexors retain a considerable share of power, the fingers are bent, and the arm hangs

by the side. Here the first thing you should do is to adopt the treatment recommended by Dr. Pemberton in his work on Abdominal Diseases, namely, to apply a splint to the inside of the fore-arm and hand, so as to counteract the preponderating influence of the flexors. Apply a splint to the fore-arm, wrap it up in flannel, and make the patient keep it supported by a sling. In this way you establish a kind of balance between the antagonist muscles, and place the extensors under favourable circumstances for bringing about a cure. If the patient has both arms affected, which is sometimes the case, change the splint from one arm to the other every second day, and continue this alteration until the cure is completed.

You will next have recourse to the use of strychnine, one of the best remedies we possess in cases where the paralysis does not depend upon organic disease of the brain. This is a remedy which is given with good effects even in cases of paralysis from apoplexy, *where there is reason to suppose that absorption of the clot has taken place*. In a case of apoplexy, it can be employed only after some time and where depletive measures have been sedulously put in force, but in a paralysis of this description you may begin with it at once. Commence with the exhibition of one twelfth of a grain of strychnine two or three times a day, and go on increasing the dose gradually, until a grain, or even a grain and a half, is taken in the twenty-four hours. To ensure the exact division of this powerful drug, you should direct a grain of it to be dissolved in a few drops of alcohol, and then made into pills of an equal size with crumb of bread or conserve of roses. In this way you will succeed in bringing back the lost power of the muscles of the fore-arm and restoring its nutritive functions. I may mention here, that the atrophy of the paralysed limb, which occurs in this disease, cannot be accounted for by supposing that it is produced by want of exercise; the emaciation is so rapid (sometimes taking place in ten days or a fortnight) that we can only attribute it to some unknown lesion of innervation.

If the use of strychnine be followed by severe muscular twitches, pain in the head, or convulsions, you must omit it for some time, and then, when these effects have completely subsided, it may be resumed if necessary. You should also bear in mind that this remedy is one of those medicines which have been termed accumulative, that is to say, a patient may be taking it for a considerable time without any perceptible symptom, and then its effects explode suddenly, the quantity which has been accumulating in the system manifesting itself at once by symptoms of great intensity. Here you omit it immediately, and with a view of relieving the existing symptoms, prescribe a draught, composed of camphor mixture, ammonia, and opium. This has generally the effect of calming the nervous excitement, and you will seldom have any more trouble on this account. *En passant*, I would advise you, whenever you employ strychnine in private practice, to inform your patient of the occurrence of such symptoms, and tell him that there is no cause

for alarm. Instead of strychnine, some of the continental practitioners are in the habit of prescribing brucine, and it is stated with considerable advantage. I have tried it in two or three cases without much apparent benefit, and I am inclined to think that it is decidedly inferior to strychnine. In France, however, it has been very largely employed, and has the reputation of being a remedy of considerable value in the treatment of paralysis. It has one advantage at least over strychnine, it can be much more easily divided and regulated, so far as respects the quantity given, as it is a much weaker preparation than strychnine, one grain of which is equivalent to six grains of brucine.

In addition to these measures, I have seen much benefit result from the application of blisters and frictions, with stimulating liniments to the spine. It is also of importance to remove the clothes in which the patients have worked; they are frequently charged, saturated with lead, and have a considerable tendency to keep up the disease. I have often seen an attack of painter's colic reappear so shortly after leaving hospital, and without any evident exposure that I could only attribute it to the circumstance of their garments being saturated with the lead.

In the foregoing plan of treatment there is nothing new; it is, in fact, a routine practice, but it is one which is borne out by the results of pathology, and which, from long experience, I can strongly recommend. I may also remind you that the plan of treatment followed in the hospital of La Charité, which has more cases of this disease than any similar institution in Paris, is completely routine.

Other metals besides lead, as, for instance, copper, produce effects somewhat analogous. Copper is said to produce salivation, colic, and vomiting. Brass-founders are liable to these symptoms, as also other persons employed in the manufacture of copper. I have not seen the disease, but it is said to be analogous to lead-poisoning, so far as colic is concerned; in other respects the symptoms differ. The convulsions are not so violent, nor is the paralysis or coma so frequent; there is often considerable fever, thirst, difficulty of respiration, præcordial anxiety, diarrhœa, and prostration of strength, so that it comes much nearer to ordinary intestinal inflammation with fever, than painters' colic. Yet it is a curious fact, that, notwithstanding all this array of symptoms so closely bordering on inflammation, it has been found in Paris, where several cases of this disease have been seen, that it is amenable to the same treatment as painters' colic, and that, under the use of purgatives, the fever, thirst, diarrhœa, and tenesmus subside.

Mercury, under certain circumstances, will produce a most extraordinary affection, on which I shall here make a few observations. The disease is not of very frequent occurrence, but it is of importance in practice to be able to recognise and treat it properly. It is a proposition well known to almost every one, that many bad effects have resulted from the abuse of mercury; and I need not tell you how many persons are injured by the empirical

employment of this potent drug on all occasions and in all constitutions. It is a common opinion that mercury acts principally on the capillary and absorbent systems, but there can be no doubt that it also acts upon the nerves, and that in a very remarkable manner. I have seen cases where the constant use of calomel has produced a marked derangement of the nervous system, manifested by great irritability, tremors, hysterical excitement, and hypochondriasis. You will see in the various works on Toxicology an account of the effects produced by mercury on persons employed in quicksilver mines, and on tradesmen, such as looking-glass manufacturers and others, who come in contact with mercury. I shall read for you the notes of a remarkable case of this kind, which was some time back under treatment in the Meath Hospital. It may be called a form of the paralysis agitans from the effects of mercury. Similar cases have been described.

A man, aged forty-six, was admitted into one of our medical wards in October, 1833. He stated, that from the time he was eight years of age he had been employed in a looking-glass manufactory, and that his occupation principally consisted in what is technically termed the silvering of mirrors. In this process the operator's right hand is repeatedly immersed in a vessel filled with mercury, while the left fixes a sheet of tin-foil, on which the metal is rubbed. Artisans while thus engaged are in the habit of using a muffle, which covers the mouth and nostrils. This the patient said he had never used, because he found that those who were in the habit of wearing it did not enjoy better health. For thirty years he continued to enjoy tolerable health, with the exception of some bleeding from the gums, with shooting pains and a sense of formication in various parts of the body, accompanied by a slight loss of power in the hands, which came on at various times, and was generally relieved by the use of ardent spirits. He had been frequently salivated, and when admitted had lost nearly all his teeth. The mode in which he lost them was this, gum-boils formed close to the roots of the teeth, which soon after dropped out, and in this way the local inflammation subsided. About three years ago, he had an attack similar to that for which he had been admitted; he went into the hospital and was put under an active antiphlogistic treatment with relief. From that time up to the period of his admission, he had enjoyed tolerable health, except that the sight of the right eye was considerably impaired, and that his memory was slightly affected. He forgot the names of persons and places, and was frequently at a loss in endeavouring to recollect the persons to whom he had lent his tools. On being brought into the hospital he presented an extraordinary specimen of human suffering, and I was at first unable to give his complaint a name, the case being the first of the kind I had seen. It exhibited the phenomena of a violent spasmodic affection; it was different from tetanus, or hydrophobia, or hysteria, but it bore some faint analogy to chorea. The head, arms, and fingers, particularly on the left side, presented a succession of quick, convulsive, jerking motions.

The angles of the mouth were retracted, the eyebrows twitching, the head constantly thrown back, but the agitation scarcely raised the arms. The nostrils were spasmodically dilated. The sternomastoid, trapezius, scaleni, diaphragm, and the abdominal muscles were similarly affected. Their contractions were short, rapid, and painful. From the constant hiccup with which the spasms of the diaphragm were attended, and the jerking motions of the tongue, his speech was interrupted and indistinct. He was occasionally free from spasms altogether, but whenever he transmitted volition to any part of the muscular system, it became instantly affected. When he endeavoured to raise his foot from the ground, it quivered and fell quite powerless and useless. Whenever he attempted to carry a vessel to his lips he generally overshot the mark, carrying the vessel towards his ear, nose, or forehead, and spilling its contents over his face or neck, so that it was a common saying among the patients in the wards, that he did not know the way to his mouth. But if the vessel was applied to his lips by another person, he could swallow easily. A sudden blast of cold air, the application of a cold hand to the skin, or the abrupt entrance of any person into the wards brought on an attack of spasms. The muscles of the left hand and of the left side were affected much more than those of the right. The mental powers were not impaired, the patient was intelligent, and seemed anxious to communicate the particulars of his case. During the whole course of the disease he retained a full power over the urinary discharge and defecation. There was some slight tenderness on pressure over the fourth and fifth dorsal vertebræ, but the rest of the spine exhibited no increase of sensibility. His skin was cool and dry, his pulse quick, weak, and small, his bowels inclined to be costive, but easily moved by laxatives. Here we see a marked difference between this affection and painters' colic.

The treatment adopted in this case was very simple. Leeches were applied to the tender part of the spine, the patient was placed in a warm bath, and got some laxative medicine, followed by an opiate. He was also ordered to have a large flannel shirt, and to be placed in a warm comfortable bed. He passed the night tolerably well, and next day appeared to be much improved. I shall not continue the daily reports of this case, but shall merely mention, that after a few days a great improvement took place. The spasms of the left side continued, though much less severe. Those of the purely voluntary muscles on the right ceased, while the spasms continued in the respiratory muscles on this side. We found that all the muscles of the face which have been called respiratory by Sir C. Bell, the platysma, scaleni, pectoral, and intercostal muscles, and the diaphragm, were thrown into violent spasms, while the purely voluntary muscles remained in a state of perfect quiescence. I am not aware that this circumstance has been observed in any other case. As far as it goes, it tends to corroborate the views of Sir C. Bell. In the treatment of this case we employed narcotic frictions, particularly those composed of

the extract of belladonna, to the spine with considerable benefit. The patient was cured by very simple means, and at little expense to his constitution.

LECTURE XXIII.

Diseases of the nervous system—Pathology of, unknown—Molecular change in the nervous centres—Difficulties of distinguishing arachnitis from encephalitis—General and partial cerebritis—Symptomatology of—Diagnosis of—Preservation of intellect in—Production of general symptoms by local lesion.

To-day we commence the consideration of the diseases of the nervous system, and here let me remark, that, even on the very threshold, we have to encounter several difficulties; some depending upon the great obscurity of the symptoms—some upon the want of correspondence between the symptoms and known organic changes, and some upon the necessarily imperfect nature of our classification of nervous affections. Many persons are in the habit of taking a limited view of the nervous system. They suppose that, when we speak of its diseases, we merely allude to affections of the brain and spinal cord; but the truth is that the nervous system, so far as regards organisation, is universal; and there is evidence to show that, even in parts and tissues which present no appearance of nerves or nervous communication, there resides a nervous power, either inherent in their organisation or derived from external sources, and by the latter mode, of *nervous irradiation from surrounding tissues*, has the sensibility of serous membranes been supposed capable of explanation. But there can be little doubt that even these tissues present nervous expansions, though of an infinite delicacy. They are, we know, supplied with white vessels, and doubtless have nerves corresponding to their vessels in size and function—nerves, insensible to us in health, but, when inflammation elevates the organ in the scale, capable of transmitting the most exquisite pain to the centre of perception. It seems, also, to be highly probable that nervous disease may commence not only in an affection of the brain or spinal marrow, but also in a similar condition of any part of the system. Again, if we admit the nervous system to be the governing and directing portion of the whole body, it is likely that some modification of that government *precedes* the alterations which take place in the circulatory and nutritive functions of other parts. Thus, in all diseases it may be laid down as a general rule, that there is an affection of the nervous system, either local or general; or, in other words, that there is no disease which we could name, which does not present signs of an affection of the nervous system, either *quoad* the suffering organ itself, or of an affection more general and diffused. If we take, for instance, a case of gastritis or hepatitis, we

find a lesion of function in the nerves of the respective organs, which, in certain cases, seems local; but, if the inflammation be intense and the fever high, we have superadded to this a sympathetic affection of the brain or spinal cord. The same thing applies to all forms of local disease; for in all there is an affection of the nerves, either confined to the suffering organ, or extending to the whole system.

In reviewing the phenomena of nervous diseases, we find them presenting several varieties depending upon certain circumstances. In the first place, they vary according to the seat of the disease. We find that the signs and symptoms of affections of the cerebro-spinal system differ very considerably from those which characterise diseases of the sympathetic nerves. Again, if we take any part of the nervous system, and examine its diseases, we find that here also there is a source of variation connected with the peculiar part affected. Thus, if we take the cerebro-spinal system, we find that disease of one part of it differs most essentially in symptoms from disease of another: we may have enormous and fatal disease of the spine without the slightest injury of the intellectual powers, but we seldom have disease of the brain, particularly of the surface, without a more or less appreciable lesion of the phenomena of the mind. To follow up this point, suppose we take the diseases of the brain itself, as compared with each other; we find that their symptoms vary according to the locality, so that, whether we look to physiology or pathology, we must consider the brain as consisting of several distinct parts, and not as an inseparable whole. It is admitted, by many writers of high authority, that there is a difference between the symptoms of disease affecting the periphery, and disease affecting the central parts of the brain; and there is reason to believe that we may be able, in many cases, to diagnosticate affections not only of the centre and periphery of the cerebrum, but even of other parts of the organ.

The same variety occurs with respect to the effects of diseases of the nervous centres. In some instances we have, as the result of disease of the brain, a loss of muscular power, or of sensation, in different parts of the body—sometimes affecting the face, sometimes one side, or even both; and these paralyses may be single or variously combined. It appears, then, that the component parts of the nervous system, by being to a certain extent separate and distinct, furnish a very extensive source of variety in the phenomena of nervous affections.

Lastly, we have the varieties which depend upon the nature of the lesion. We generally observe an obvious difference between cases of nervous disease, accompanied by some *known change* in the injured part, and cases in which no such change can be demonstrated. Thus, for instance, we know the symptoms of apoplexy, and that, in the majority of cases, it is a disease connected with some perceptible change in the circulation of the brain—as excessive distension of its vessels, or an effusion of blood on its surface or into its substance. We also have some idea of the nature of

inflammation of the brain; we know that its substance becomes at first red, then begins to soften, and finally is converted into a pulpy mass. Now, there are a number of symptoms which are so often and so constantly connected with peculiar organic changes, that, the symptoms being known, we can make a tolerably correct guess at the nature of the alteration, or vice versa.

On the other hand, however, we have a large and important catalogue of nervous affections, in which the symptoms give but very unsatisfactory information as to the real nature of the disease, and to the elucidation of which the painful and long-continued investigations of the pathological anatomist have hitherto been directed in vain. Of the actual nature of a numerous, complex, and interesting class of diseases—the *neuroses*—we know nothing. All we can say of them is, that they are examples of lesions of function in various parts of the nervous system, presenting no trace of structural alteration *appreciable by our senses*. It is a startling fact, and one which must be a source of gloomy reflection to the pathologist, that many of the diseases of the nervous system, which present the most violent symptoms, are those in which there is the least perceptible organic alteration. Every man who has seen a case of hydrophobia, or tetanus, or mania, or epilepsy, has witnessed a train of extraordinary and horrible symptoms, infinitely worse than those which are seen to accompany even great organic alterations of the brain.

Here, then, is a singular fact: that there is a part of the system presenting a series of diseases under this extraordinary law, that the most violent and frequently fatal symptoms are accompanied by the least perceptible organic alteration. Now, what is the nature of these neuroses? To give you a familiar illustration, let us take a case of tetanus or hydrophobia as an example. Here we have a train of symptoms exhibiting the most frightful irritation of the nervous system; and yet, when we come after death to examine, with eager curiosity, the cause of all these appalling phenomena, what do we find?—nothing. There is no unequivocal, no constant, no prominent alteration of any part of the nervous system, to throw light upon the obscurity of our opinions, and enable us to fix the nature or locality of the disease. We lay aside the knife in despair, and bitter indeed is the consciousness of our ignorance.

Two opinions have been entertained by pathologists with respect to those singular affections: one, that they are examples of some peculiar modification of the nervous influence, *independent of any organic change*. In other words, the pathologists who entertain this opinion hold, that the principle of life may be altered in its phenomena, and admit of modifications, independent of any molecular change. The supporters of this doctrine reason thus:—In the phenomena of neuroses we have a train of extraordinary and violent symptoms unconnected with organic change. Now, it is quite unphilosophical to say that there is organic change when we cannot see or demonstrate it; and, on the other hand, it is not absurd to suppose that we may have lesions or peculiar modifica-

tions of the nervous principle, without any organic alteration. The other opinion is, that in the neuroses there is some organic change, the nature of which cannot be ascertained, in consequence of our limited powers of detecting elementary changes. In whatever light we view this question, it appears to be surrounded with difficulties. No one can deny that neuroses are very different from organic diseases of parts. If we compare them with that class which is most familiar to us—the inflammatory affections—we find a remarkable difference. In the first place, the neuroses may be brought on by causes not reckoned among those commonly capable of exciting inflammation. In the next place, their invasion is sudden, and their progress rapid; they arrive at their acme in a very short period of time, and subside rapidly. These are characters which do not belong to the ordinary forms of organic disease. Again, we often observe the utmost intensity of nervous pain without the co-existence of swelling, redness, or heat of the part affected. We find, too, that they are not to be subdued by the antiphlogistic plan; on the contrary, several of them are either relieved or cured by an exactly opposite line of practice; and many cases, which would appear to demand the lancet, are known by long experience to be most benefited by stimulants. Lastly, the most accurate and well conducted investigations of pathological anatomy have failed in demonstrating the slightest organic change in these cases—at least, where changes are found, these are *neither constant, competent, nor commensurate with symptoms*; so that, whether we compare the information we derive from symptoms, or the result of pathological anatomy, we find a great difference between neuroses and organic diseases. It may be said, that, though they are not inflammatory affections, they have some resemblance to them. This, however, is only a gratuitous supposition; for, even in the very worst cases, they present nothing analogous to the results of inflammation, and the brain and spinal cord are as free from perceptible organic change, in the majority of cases of fatal tetanus and hydrophobia, as they would be in nervous affections of a slight and transient character.

You must have been already convinced that it is difficult to form any clear or definite notion of the nature of neuroses; indeed, the only thing we can say of them is, what they are not. When we reflect on nervous phenomena, and consider how occult, how mysterious, the properties of those organs which give rise to them are, we are struck with astonishment at the discrepancy between cause and effect. No medical man has ever witnessed a case of confirmed tetanus or hydrophobia, without being oppressed with a conviction of the imperfect and limited state of our knowledge of nervous disease.

It may be very possible, that in these neuroses the change, though so slight as to escape our means of detection, does absolutely occur; and yet such is the nature of nervous phenomena, that we must admit that great and extraordinary effects are produced by very slight causes. Do we see any thing like this in nature?—any

remarkable alterations in properties depending upon apparently slight causes? We do—we see extraordinary changes taking place in the characters of various inorganic substances, (to which I need not particularly allude,) and there is no reason why the same thing should not occur in organic structures. On considering the doctrine of Isomerism, I should be inclined to think that it throws some light on this obscure subject. In chemistry, it is a well-known though singular law, that the properties of two bodies may be essentially different at the same time that their respective component elements are, as far as our knowledge goes, identically the same; and the change, whatever it may be, appears to result, not from the abstraction or removal of any of the component atoms, but from their peculiar juxta-position. Now, it being admitted in chemistry that many bodies having the same constitution possess totally different properties, and this difference being explained by the different position of their elements, it does not seem strange if the same thing should take place in the phenomena of organised beings; and, if this be the case, we have a key towards elucidating the nature of these neuroses, and can conceive how an analogous change—a difference in the arrangement of the molecules of the component parts of the nerves, or their centres—may produce new modifications of their properties, without making any distinct change in their nature, or adding or abstracting a single organic molecule. I am much inclined to adopt the opinion of those who think that, in the neuroses, a peculiar organic change actually takes place, though we cannot demonstrate its existence; because, to reason on the phenomena of animal life, independently of organisation, is to plunge blindly into hypothesis, and retrace the errors of an antiquated and exploded school.

In treating of the diseases of the nervous system, I regret that time will not permit me to enter into the subject as fully as I could wish; all that I hope to be able to accomplish is, to give a sketch of some of the more prominent affections. The arrangement I purpose to adopt is the following:—1st, I shall treat of local inflammations of the brain; 2d, of general inflammations of that organ; 3d, of mere sanguineous congestion or hyperæmia of the brain; 4th, of apoplexy; and 5th, of the various forms of paralysis.

In taking up the subject of cerebral inflammation, I beg leave to observe, *in limine*, that the brain may be attacked by general or local inflammation; and further, that it may, as stated in books, be inflamed in its membranes or in its substance, or in both together. A great deal has been written to show that we can distinguish, during life, between inflammation of the substance and of the membranes of the brain. On this point, I believe, we may come to this conclusion—that inflammation of the membranes of the brain, or arachnitis, may be distinguished from some cases of local inflammation of the cerebral substance, but that it cannot, in the present state of our knowledge, be distinguished from *general* inflammation of the brain. We can, in most instances, make a distinction between local disease of the brain and arachnitis; but, when the whole

substance of that organ is affected, our means of diagnosis fail. This, however, is not so much to be regretted, as the distinction is of very little consequence, so far as treatment is concerned. Here we arrive at the knowledge of a principle highly consolatory in the practice of medicine; namely, that in many acute cases where the diagnosis between two diseases of neighbouring parts is difficult or impossible, it is also, so far as regards immediate treatment, unnecessary.

If we enquire what are the symptoms of membranous inflammation of the brain, as laid down in books, we shall find them to be the following: pain, delirium, convulsions, alteration of sensibility, and coma. These are the symptoms which are generally given as characteristic of arachnitis; and it is quite true that they are observed in many cases of the kind. But the person must be dull indeed who thinks that such symptoms imply nothing more than an inflammatory affection of the membranes of the brain. Take, for instance, one of the most prominent symptoms—delirium; what does this imply?—that the portion of the brain which discharges the functions of intelligence or mind has been injured, and is rendered incapable of performing its office. No one will venture to assert that the membranes of the brain are the organs of thought, and that the delirium proceeds from *their* morbid condition: such a notion as this could not be entertained for a moment. What then are we to suppose? One of these two things—either that there must be inflammation of the substance as well as of the membranes, or that the substance of the brain must be affected in a neurotic manner without any actual inflammation. As far as delirium is concerned, it appears to me to be quite impossible to distinguish between inflammation of the brain generally, and of its membranes. The same rule applies to the other symptoms—convulsions, alteration of sensibility, and coma. I repeat, that all we can say on this subject is, that, in such cases, there is either inflammation of the substance as well as the membranes of the brain, or that, with the membranous inflammation, there is a neurotic condition of the substance of the brain. Yet who, in such cases, can affirm with certainty that the symptoms of derangement of the substance of the brain are merely neurotic, when inflammation is admitted to exist within the cranium, and when we know that the two inflammations commonly co-exist?

The fact of delirium occurring so frequently in inflammation of the membranes of the brain, is of considerable importance, as showing, not that membranes of the brain have any thing to do with intelligence, but as supporting the opinions of those who believe the periphery of the brain to be the seat of the intellectual faculties; and here is a fact which, as far as it goes, is in favour of the doctrines of phrenology. If we compare those cases of cerebral disease, in which there is delirium, with those in which it does not occur, we shall find that it is most common in cases where disease attacks the periphery of the brain, as in arachnitis. The cases in which we observe great lesions of the brain without delirium are

generally cases of deep-seated inflammation of a local nature, or inflammation of those portions of the brain which the phrenologists consider not to be subservient to the production of mental phenomena. This fact, also, would seem to confirm the truth of the opinion of the difference in function between the medullary and cortical parts of the brain. It is supposed that the cortical part of the brain is the organ of intelligence, while the medullary portion performs a different function. It is, however, a curious fact, that in delirium the inflammation is generally confined to the surface of the brain, and that, in cases of deep-seated inflammation, the most important symptoms are those which are derived from the sympathetic affections of the muscular system.

Partial encephalitis may be either primary or secondary. An example of the latter is that inflammation of the substance of the brain which supervenes on apoplectic effusion, tumours, or cancer. What we generally observe, in a case of this kind, is more an alteration in the functions of the muscular system, and less of the intellect. This alteration consists at first in an apparent increase of innervation in certain muscles of the body, and we generally find that one of the earliest symptoms of local encephalitis is the occurrence of pain in some of the muscles of the extremities. This is a curious fact, but one which is well established. In partial encephalitis there is often but little, or even no pain in the head; and the only warning we have of the approach of cerebral disease is the occurrence of pain in the extremities, followed by rigidity. Here are the two most prominent symptoms of the disease—pain in the muscles of the extremities, and then rigidity. Further, we have alternate spasms and relaxations of the muscles, in which, however, the power of the flexor muscles ultimately prevails; so that, if the disease be in the fore-arm, it may become permanently flexed on the arm, and the contraction of the fingers is sometimes so great as to drive the nails into the flesh. If it affects the leg, the heel may be pressed against the buttock sometimes so forcibly as to form a sore. As the case proceeds, the limb becomes more fixed in its new position, and every attempt to extend it causes pain. During the prevalence of these symptoms, it frequently happens that the patient does not feel pain in the head, or any diminution of intellectual power. The absence of pain in the part affected may be accounted for by recollecting that it is a general law, that all inflammatory affections of deep-seated parts are, to a certain extent, of a comparatively painless character; and we may account for the non-existence of any lesion of the mind, by remembering that the disease is partial, and confined to a portion of the brain which appears to have little or no connection with the intellectual functions. In cases of this kind, when the muscles of the face are affected, the phenomena are interesting, from their being (*in the first stage*) the reverse of those of apoplexy. The face is drawn *from* the affected side, and the tongue pushed, by the opposite half of the genio-hyo-glossus muscle, *to* the affected side. This is the spastic stage, when complete disorganisation has not yet occurred. But when this

happens, then the phenomena of the face are like those of apoplexy, because the opposite muscles, which were in a spasmodic, are now in a paralysed state; so that the face is drawn *to* the affected side, and the tongue pushed *from* it, by the healthy action of muscles which are deprived of their antagonists.

I mentioned before that delirium may not occur during the course of a partial encephalitis; and I gave, as a reason for this, the circumstance of the disease being of small extent, and confined to parts of the brain which do not discharge any of the functions of mind. Another explanation has been given, drawn from the consideration of the double nature of the brain. It is thought that, where disease exists in one part of the brain, sanity may be still preserved in consequence of the healthy condition of the corresponding part; but where disease attacks both hemispheres together, as in a case of arachnitis, then there is a distinct lesion of the mental faculties.

The next stage of partial encephalitis is that in which the diseased portion of the brain breaks down, softens, and is converted into purulent matter. This stage is marked by a new train of symptoms. The first stage is characterised by pain occurring in the muscles of the face, or of the extremities of either side, and followed by great rigidity. The second stage is of a different character; the rigidity and spasm of the muscles diminish, and are succeeded by a paralytic and flaccid state of these organs. Voluntary motion on the affected side now becomes impossible, the organ on which it depends being destroyed. Now let us, for sake of arrangement, call the first, or spastic condition, the convulsive paralysis, and the second, the paralysis with resolution. In the first, or convulsive stage, the brain is affected in the first degree; it is labouring under irritation or actual inflammation, and the disease still holds out a tolerably fair prospect of relief or cure. But in the second stage a cure is impossible, and hence it is a matter of the greatest importance to commence our operations at an early period; and, by having recourse to prompt and active treatment, give the patient every chance for a cure.

In the partial inflammation of the substance of the brain, sensation is variously altered. In some cases motion is lost, while sensation remains intact; in others, sensation is partially or wholly abolished. In many instances the intellectual powers remain in all their integrity, or but little impaired, even after the occurrence of symptoms which mark the softening down of the substance of the brain, and its conversion into purulent matter. In a few there is, during the first stage of the disease, a slight alteration in the state of the intellect, marked by a certain degree of excitement or exaltation of the mental faculties, and this, on the supervention of the second stage, is exchanged for a state of depression. In fact, the morbid phenomena of the mind and of the muscular system, where they co-exist, appear to be regulated by the same laws. Where the disease is extensive, you can easily observe the injury of the mental faculties which accompanies the second stage; the patient answers

slowly when questioned; his memory is weak, and his countenance has a stupid expression. But cases, even of extensive local suppuration, have been described by various authors, in which there was no lesion of the intellectual functions observed. These, however, generally admit of an explanation. Thus, in the cases recorded by Lallemand, the abscesses were situated in the cerebellum, pons Varolii, and other parts which are not supposed to have any connection with the phenomena of mind. There are several well-authenticated cases of extensive disease, not only of these parts, but even of the substance of the hemispheres, occurring without any appreciable lesion of the intellect. Thus, Mr. O'Halloran gives the case of a man, who, after an injury which destroyed a large portion of the frontal bone, had extensive suppuration of the brain, and lost an enormous quantity of the substance of one of the hemispheres, and yet preserved his intellect entire up to the moment of his dissolution. There is some difficulty in explaining this. It is an opinion entertained by some physiologists, that when one hemisphere is diseased its functions are discharged by the other; and that, the brain being a double organ, disease of one side does not impair the functions of the other. But, in answer to this, it may be urged that there are many cases on record in which disease of a *single hemisphere* has produced great alterations of intellect. The supporters of the former opinion attempt to explain such cases in this way. They state, that in the majority of such cases there was, besides the local encephalitis, inflammation of the arachnoid membrane, and that the lesion of intellect was not so much the effect of local disease of the brain as the result of its complication with an arachnitis engaging the whole periphery of the organ. In the next place, they explain the fact of a *general* affection of the brain arising from *local* disease, as depending in most cases on the pressure which the tumefied state of the diseased portion necessarily makes on the sound hemisphere; and they state that this pressure must be very considerable, as the brain, being confined within a bony cavity, has no power of expanding itself. Now, it is a most interesting fact, in support of this view, that, in a great number of the cases of loss of brain with preservation of intellect *all through the case*, an extensive opening existed in the bones of the skull, so as to permit of expansion in the diseased hemisphere, and prevent the pressure being exercised on the opposite one. This point appears to be borne out by the result of Mr. O'Halloran's cases, and by many other examples. Lastly, in every acute case of local inflammation of the brain, two causes having a tendency to produce symptoms exist. One of these is the local disease which gives rise to those phenomena of motion and sensation which we observe on the opposite side of the body; the other is the determination of blood to *the whole brain*, the result of the irritation of that disease.—“*Ubi stimulus ibi humorum affluxus.*”

LECTURE XXIV.

Encephalitis, diagnosis of—Preservation of function with organic disease—Vicarious action of parts—Importance of pathology to pre-nology—Use of pathology to phrenologists—Arachnitis at the base of the brain—Symptoms of—Influence of age over the intellectual faculties—Opinions of Bouillaud, Serres, and Foville—Influence of the optic thalami and corpus striatum on the motions of the extremities—Diagnosis of disease of the cerebellum—Connection with the generative system—Remarkable cases of.

We were occupied at our last lecture in considering some of the phenomena of partial encephalitis, by which is generally meant, a localised inflammation of the deep-seated parts of the brain; because superficial inflammation of the cerebral substance is very rarely partial. I endeavoured to show that the diagnosis of this local encephalitis was to be drawn, in a great measure, from the occurrence of pain and muscular affections of *one side of the body*; in other words, that the phenomena of this disease were partial, so as to give us at once a distinction between general and partial inflammation of the brain. In cases of general inflammation, we have convulsions of both sides—delirium and coma; in the partial form these symptoms are absent until complication takes place. Thus the supervention of delirium, or of convulsions on both sides, in a case where previously the signs of only *partial* encephalitis existed, would point out, in all probability, an extension of disease to the opposite hemisphere. I also endeavoured to point out the different modes in which partial encephalitis might be accompanied with symptoms of a general character, or affecting both sides; that there might be a co-existing inflammation of the membranes; or that the pressure of the diseased on the healthy hemisphere of the brain might be the cause of the complication. I stated, that some of the most remarkable cases of extensive destruction of the brain, without perceptible injury of the mental powers, were those in which a traumatic opening in the skull gave full scope to the swollen parts, and obviated the effects of pressure on the sound hemisphere. I also observed that, in cases of local affections of the head, there are two causes which have a tendency to produce general symptoms. One of these is the cause which determines the pain and muscular affection of the opposite side; the other is the general determination of blood to the head; so that we may have cases in which the *actual inflammation* is limited to a part of one hemisphere, and yet, from the general determination of blood to the head, we may have coma and general symptoms.

To return again to the interesting consideration of great loss of cerebral substance with preservation of intellect, I have to remark, that this circumstance is one which some persons might quote against the opinion that the brain was the organ of intelligence; and I believe this fact has been laid hold of by the opponents of phrenology, and put forward as a powerful argument against the truth of its doctrines. Thus, for instance, in the case of Mr. O'Halloran's patient, who lost a large portion of one hemisphere,

and yet, with all this mischief, the powers of the intellect remained unimpaired; it would not seem strange if a person should say, here is vast destruction of substance without any lesion of intelligence; how then can the brain be considered as the organ of thought? But let us look at this matter in its true point of view. In the first place, it is to be remembered that cases like this are rare—that they are to be considered as the exception and not as the rule. I have already shown you, that it is a law in pathology that lesion of structure and lesion of function are not always commensurate. This law applies to the brain as well as to all the other organs. To say that the brain was not the organ of intelligence, because in cases of extensive cerebral disease that intelligence was preserved, is false reasoning. A man will digest with a cancerous stomach; is it to be argued from this that the stomach is not the organ of digestion? I have seen the liver completely burrowed by abscesses, yet the gall-bladder was full of healthy bile. I have seen one lung completely obliterated, and yet the respirations only sixteen in the minute, and the face without lividity. What do these facts prove? Not that the health of organs is of no consequence, but that with great disease there may be little injury of function.

By reference to the original laws of organisation, we may (in some cases at least) arrive at an explanation of this fact. You know that organs are primitively double; and we find, that though the fusion at the median line is produced by development, yet that the symmetrical halves still, to a certain degree, preserve their individuality. Thus we see how the laws of organisation affect the phenomena of disease, and recognise a provision, acting from the first moment of existence, against the accidents of far distant disease.

Now, admitting that the brain is the organ of thought, we may suppose that, as in case of partial obstruction of the lung from inflammation, the remainder of the organ takes on an increased action, so as to supply the place of that which has been injured or destroyed. We know that if one lung be hepatised, the other takes on its functions, and carries on the process of respiration for a time. That this is the case, is shown, first, by life being continued, and, secondly, by the stethoscope, which informs us that the respiration of the lung, which has a double duty thrown upon it, is remarkably intense, proving the force of its action; and it has been further established, that the lung which thus takes on a supplemental action may become enlarged and hypertrophied. May not this also occur in the brain? There is no reason why such a pathological phenomenon, occurring in one viscus, may not also take place in another. But the opponents of phrenology say, supposing the organ of causation to be destroyed, how can the person continue to reason? It strikes me that the only way in which we can account for this is, by supposing that other parts of the brain take on the functions of those which have been injured or destroyed. Nor is there any thing extraordinary or anomalous in such a supposition. We see, almost every day, examples of this kind. We see that in certain

diseased states of the liver, accompanied by suppression of its secretion, its functions are assumed by other parts, and bile continues to be separated from the blood by the kidneys, salivary glands, and by the cutaneous exhalants. Here is a remarkable case, in which the glands and other parts take on the performance of a function totally different from that in which they are ordinarily employed. We find, also, that when the urinary organs are obstructed, urine, or its principles, are discovered in parts of the system where we should not at all expect them. Thus we have a very remarkable case detailed in the American Journal of the Medical Sciences, in which we find that a young female, who laboured under paralysis of the urinary organs, discharged urea from almost every part of the body, even from the ears. Neither is there any thing very extraordinary in this. In several instances of suppression of the menstrual discharge, do we not see a vicarious secretion taking place from the surfaces of parts the most distant, and unconnected with the uterine system? It is a well-established law, that when the functions of organs are suspended or destroyed, other parts will often take on the action of the injured viscus. Now, supposing that a portion of the brain is to be looked upon as the organ of causation, and such portion is injured or destroyed, there is no reason why the remaining sound portion of brain should not take on, at least to a certain extent, in addition to its own, the functions of that part which has been injured. If, independently of any phrenological views, we admit the brain to be the organ of thought, there is no reason why we should not admit that the loss of intellectual power, produced by lesion of one part, may not be supplied by an increase of activity in the remaining portions. It is only by a supposition of this kind that we can account for the preservation of the integrity of mind in many cases of disease of the brain. If we admit the phrenological doctrines, we can suppose that when one organ is injured, another may take on an additional function, and in this way preserve the integrity of the intellect; so that, whether we reason from phrenology or not, the continuance of soundness of mind, in cases of injury of the brain, can be understood when you come to contrast it with other analogous pathological facts. I again repeat, that it is not more extraordinary that, in case of local injury of the brain, the sound parts should take on a supplemental action, than that bile should be eliminated by the salivary glands, skin, and kidneys, or that the principles of urine should be discharged from almost every part of the system, or that a vicarious discharge from the roots of the hair should supply the place of the uterine secretion.

On this subject, one point should be always borne in mind, viz., that we may be wrong in saying that a patient is *quite sane*, while he is still an invalid and in bed. Unless we can show that after his recovery, and in his various intercourse with the world, he preserves his original intelligence, it would be wrong to assert that there has been absolutely no lesion of intellect consequent on the affection of the brain. While lying at ease in bed, and unaffected by any moral stimuli, he may seem to possess a sound condition of

mind; he may put out his tongue, or stretch forth his hand, when requested; he may give an accurate account of his symptoms, and answer all the ordinary medical interrogatories with precision. But you are not, from this, to conclude that he is perfectly sane. Many persons, under these circumstances, have died in bed, and appeared to preserve their intellect to the last; but in such cases, the test of sanity, *intercourse with the world*, could not be fairly applied, and hence I think that there are not sufficient grounds to pronounce a decided opinion as to the real condition of the intellect in such cases.

Before I quit this part of the subject, I wish to make a few remarks on the doctrines of phrenology. There can be no doubt that the principles of phrenology are founded on truth, and, of course, highly deserving of your attention, as likely, at some future period, when properly cultivated, to exercise a great influence over medical practice. The great error of the phrenologists of the present day, consists in throwing overboard the results of pathological anatomy. If a pathological fact is brought forward, as appearing to bear against the validity of their opinions, they immediately exclaim, "we dont recognise any fact or principle drawn from disease; our science has to do with the healthy, and not the morbid, condition of the brain." Now, this is altogether absurd. Phrenology, if true, is nothing but the physiology of the brain, and pathology is nothing but the physiology of disease. Phrenology must be tested by disease as well as by health, and if it does not stand the test of pathology, it is wrong. If phrenology be a science founded on truth, if it is a true physiology of the brain, or of that portion of it connected with mental phenomena, one of two results should obtain—either that it should be confirmed by pathology, or that the difficulties, which pathology presents, should be explicable in a manner consistent with the science. The phrenologists, in my mind, are doing a direct injury to the cause of their science, by their unnecessary and ill-timed hostility to pathology. It is idle to say, as they do, that theirs is the science of health, and that it is unfair to apply to it the test of disease. From pathology is drawn a host of facts, from which the doctrines they profess derive their principal support. The mere phrenologist, who understands not and despises pathology, is nothing better than a charlatan, and professes a science which he does not comprehend. If he would recollect that the brain in a state of health is most, and in a state of disease least, adapted to the purposes of thought, he would see that this is one of the strongest arguments in favour of his doctrine, that the brain is the organ of mind. The more healthy it is, the fitter it is to discharge the functions of intellect, and vice versa; yet phrenologists are so absurd as to think that pathology has nothing to do with their science.

But besides confirming the doctrine that the brain is the organ of thought, there are innumerable facts drawn from pathology, which have a tendency to prove that particular parts of the brain are the organs of peculiar phenomena. We see an injury of one part of

the brain, accompanied by a train of symptoms indicating some peculiar lesion of mind; we see an affection of another part, attended by a different class of phenomena. Here pathology, the science which phrenologists reject and despise, goes to establish the ground-work of their doctrines, that the brain consists of a congeries of parts, having each a separate and distinct function. We find, for instance, that disease of one portion of the brain affects the intellect; of another, the generative organs; of a third, the muscular system. What does this prove but that the brain is not a simple organ, but composed of a congeries of parts, each of which governs a different part of the system, or ministers to a peculiar purpose? Now, what is this but what the phrenologists themselves wish to prove?

Further, the professors of phrenology have placed all their organs on the surface of the brain, and for this they have been loudly censured. Phrenology, it is urged, knows, or professes to know, nothing about the central parts of the brain, which must be equally important with the superficial, and have confined their investigations to the surface alone. Now it is a curious fact, that the pathology which they deny, in this instance, furnishes the best reply to this objection. I mentioned at my last lecture, that if we examine the symptom of delirium, we find that it characterises the inflammation of the periphery, and is commonly wanting in that of the deep-seated portions. In other words, mental alienation is the characteristic of the disease of that portion of the brain where the phrenologists have placed the intellectual organs. Here is a strong fact in favour of the doctrines of phrenology, derived from that science which the mere phrenologist throws overboard and despises. Again, according to the researches of some celebrated French pathologists, there are a number of facts to show that there is a remarkable difference between the symptoms of arachnitis of the convexity and of the base of the brain. This conclusion, which, after a most careful series of investigations, was adopted by them, is borne out by the results of my experience, and appears to me to be established on the basis of truth. They have discovered that arachnitis of the convexity of the brain is a disease characterised by prominent and violent symptoms, early and marked delirium, intense pain, watchfulness, and irritability. We have first delirium, pain, and sleeplessness, and then coma. But in arachnitis of the base of the brain, the symptoms are of a more latent and insidious character; there is some pain, and the coma is profound, but there is often no delirium. What an important fact for the supporters of phrenology is this, and how strikingly does it prove their absurdity in rejecting the lights derived from pathology! Here we find the remarkable fact, that inflammation of the arachnoid, investing the base of the brain to which the phrenologists attach, comparatively, no importance, is commonly unattended with any lesion of the intellectual powers, while the same inflammation on the convexity is almost constantly accompanied by symptoms of distinct mental alienation.

It is objected to the phrenologists that they know little or nothing

of the central parts of the brain; that though these parts may be fairly considered to be of as much importance as any others, still they do not admit them to be organs of intellect. Now, what does pathology teach on this subject? It shows that we may have most extensive local disease of the central parts of the brain—that we may have inflammation, suppuration, abscess, and apoplexy, without the slightest trace of delirium. Indeed, there can be no doubt that the central portions of the brain have functions very different from those on the surface. They appear more connected with another function of animal life, muscular motion and sensation. Then let us examine the phenomena of old age. Every one is familiar with the fact, that when a man arrives at an extreme age, he generally experiences a marked decay of intellectual power, and falls into a state of second childhood. Does pathology throw any light upon this circumstance? It does. From a series of ingenious and accurate investigations, conducted by two continental pathologists, Cauzevielh and Desmoulins, it has been found that a kind of atrophy of the brain takes place in very old persons. According to the researches of Desmoulins, it appears that, in persons who have passed the age of seventy, the specific gravity of the brain becomes from a twentieth to a fifteenth less than that of the adult. It has also been proved that this atrophy of the brain is connected with old age, and not, as it might be thought, with general emaciation of the body; for in cases of chronic emaciation from disease in adults, the brain is the last part which is found to atrophy; and it has been suggested that this may explain the continuance of mental powers, during the ravages of chronic disease; and also the nervous irritability of patients after acute diseases, in which emaciation has taken place.

I might bring forward many other facts to show that phrenology is indebted to pathology for some of the strongest arguments in its favour; and I think that those phrenologists who neglect its study, or deny its applicability, are doing a serious injury to the doctrines they seek to establish. The misfortune is, that very few medical men have turned their attention to the subject; and that, with few exceptions, its supporters and teachers have been persons possessing scarcely any physiological, and no pathological, knowledge. Phrenology will never be established as a science until it gets into hands of scientific medical men, who, to a profound knowledge of physiology, have added all the light derived from pathological research. To give you an instance of the mode of reasoning of the non-medical phrenologists. In their drawing-room exhibitions, they appeal with triumph to the different forms of the skull in the carnivorous and graminivorous animals, with respect to the development of destructiveness; and all are horrified at the bump on the tiger's skull. But, as Sir H. Davy well observes, this very protuberance is a part of the general apparatus of the jaw, which requires a more powerful insertion for its muscles in all beasts of prey. Phrenology, as generally taught, may answer well for the class of dilettantis and blue-stockings, or for the purposes of humbug and

flattery; but its parent was anatomy, its nurse physiology, and its perfection must be sought for in medicine. The mass of inconsequential reasoning, of special pleading, and of "*false facts*," with which its professors have encumbered it, must be swept away, and we shall then, I have no doubt, recognise it as the greatest discovery in the science of the moral and physical nature of man that has ever been made. I feel happy, however, in thinking that, of late, the science has been taken up on its true grounds, in Paris, London, and Dublin. Vimont's splendid work on Comparative Phrenology will form an era in the science. In London, Dr. Elliotson has directed the energies of his powerful mind to the subject; and in Dublin we have a Phrenological Society, of which Dr. Marsh is the president, and my colleague, Dr. Evanson, the secretary; and, under such auspices, much is to be expected.

Having drawn your attention to the ordinary symptoms of local encephalitis, our next enquiry is, how far we can diagnosticate the actual seat of disease from phenomena observed during the life of the patient. Do not suppose, for a moment, that this part of the subject is undeserving of your attention, in the strongest sense of the word. Recollect that the more accurate and extensive is diagnosis, the more certain and available is the practice of medicine. On this subject, matters are not altered to the same extent as in the cases of chest or abdominal diseases. In our knowledge of the two latter, we have made vast strides within the last few years; but in cerebral affections, though much has been effected, much still remains to be done; and it is not improbable that some of the opinions on this subject, still promulgated in schools, require correction. If we examine the various cases of cerebral disease on record, we find that in some the paralysis was complete, and that sensation and muscular motion became, as it were, annihilated. In other cases, the muscular system alone appeared to suffer; while in a third class we find that sensibility is destroyed, while the power of motion remains intact. Again, in some we have complete hemiplegia, in others the paralysis is but partial; in some the affection is slight and transient, in others it is incurable and permanent. The result of all this would appear to imply that there are different states and seats of cerebral disease, producing different modifications of nervous phenomena. It has been taught, that a paralysis of the organs of speech points out a lesion of the anterior lobes of the brain, and there are many cases on record in support of this opinion. Here is a pathological statement strongly in favour of the doctrines of phrenology. But, on the other hand, it must be confessed that there are numerous cases on record of lesion of the powers of speech, independent of any affection of the anterior lobe; and hence, as far as the diagnosis of lesion of the anterior lobe, derived from loss of speech, is concerned, we cannot make up our minds. You are aware that the phrenologists place the organ of language in the anterior inferior part of the brain. Now, when an affection of this portion of the brain is found to coincide with the loss of speech, it is all very well; but the difficulty is to account

for those cases of loss of speech in which there is no appreciable lesion of the substance of the anterior lobe. In investigation on this point, however, you must bear the following distinction carefully in mind. The organ of language of the phrenologists is not properly the organ of the *power of speech*, but that by which, as it were, thought is converted into language. A man, from paralysis of his tongue, might be incapable of speaking; and such a case, existing without lesion of the anterior lobes, might be most unfairly quoted against the phrenologists. Again, paralysis of the upper extremities has been connected with disease of the optic thalami, and posterior lobes of the brain. It is the opinion of Bouillaud, Serres, and others, that the optic thalami regulate the motions of the upper extremities; and it is a fact, that in many instances of paralysis of the upper extremities, disease has been found in these parts. We might term the following a synthetic case, illustrative of the doctrine:—"A soldier was wounded in the right shoulder with a lance, in consequence of which, he got an aneurism of the axillary artery, for which an operation was performed. At the moment the ligature was tightened, he experienced exquisite pain in the situation of the ligature, which extended to the brachial plexus; this continued until the next day, and then ceased. On the fourth or fifth day the pain returned with increased violence, and continued until the seventh day, when it became intolerable. He was bled, but without any good effect, he then became comatose; his head was drawn backwards; he had alternations of stupor and excitement, and soon after expired. On dissection, the ligature was found to embrace some of the principal branches of the brachial plexus, and there was an abscess in the posterior lobe of the brain, extending to the optic thalamus." Here we have a case of injury of the upper extremity, and that portion of the brain which is supposed to govern it was found in a state of manifest disease. Serres gives, also, the details of some experiments in support of this opinion. On removing the posterior part of the right hemisphere of the brain in a dog, he found that the left anterior extremity became paralytic; he prolonged his incisions into the corresponding portion of the opposite hemisphere, and found that the right extremity became paralysed. In another dog he plunged a bistoury into the posterior part of the right lobe, and found that the left anterior extremity became affected with convulsive motions. He then introduced into the wound a few drops of nitric acid, so as to produce inflammation of that portion of the brain, and observed that the convulsions of the left fore-foot became more violent; in fact, that the animal had all the symptoms of a local inflammation of the brain, namely, convulsions, rigidity, and then paralysis. Rolando has performed a series of experiments with the same view, and his conclusions are exactly those of Serres. So that if we connect the results of these experiments with some facts drawn from pathology, we might conclude that the optic thalami, and posterior lobes of the brain, have a very important share in regulating the muscular motions of the upper extremity. I may here state, that,

in this city, a case of a female occurred, who got an attack of severe pain in the left hand and fingers, which became afterwards contracted; and she had, in addition to this, alternate flexions and extensions of the fore-arm, *followed by resolution and paralysis*. On dissection, there was an abscess found in the right optic thalamus; the rest of the brain was healthy.

With respect to those cases in which there is paralysis of one of the lower extremities, it has been taught that it arises from disease of the corpus striatum. On the anterior lobe the following case is given by Serres. "A woman, forty years of age, had an attack of apoplexy, from which she recovered with the left leg in a state of complete paralysis, and the left arm admitting of a slight degree of motion." Here was a case of lesion of both the upper and lower extremity of the same side, but in the former the paralysis was partial, in the latter complete. On dissection, it was found that two circumscribed abscesses existed in the substance of the right hemisphere, the larger situated in the corpus striatum, the smaller in the optic thalamus. Another case is given of a patient who got paralysis of the side; the muscular power of the arm being completely destroyed, while the leg retained a considerable degree of motion. In this case the corpus striatum was but slightly affected, while nearly the whole substance of the optic thalamus was destroyed. I have also to remark, that Serres performed similar experiments on the corpus striatum in dogs, and came to the conclusion, that it governs the motions of the lower extremities. The structure, extent, and special action of the corpus striatum and optic thalamus, are said to afford some explanation why, in ordinary cases of paralysis, the arm is more often affected than the leg, and does not recover so soon. The fact of the prolongations of the optic thalami being much more complicated and extensive than those of the corpora striata, is thought to explain their greater liability to disease.

There are, however, not unfrequent exceptions to this law; and it is not uncommon to meet with cases which militate against the doctrines laid down by Serres, and other pathologists, particularly so far as regards the connection between the corpora striata and the government of the lower extremities, so that I would have you look upon it as a point *by no means* fully established. The latest observations on this subject are by Andral, who brings forward many facts opposed to the opinions of Serres, Foville, &c. &c. Out of seventy-five cases of accurately circumscribed disease of the brain, the disease being hemorrhagic, or otherwise, he found that, in forty, where the paralysis existed in both extremities of one side, there were twenty in which nothing was injured but the anterior lobe, or the corpus striatum; while in nineteen the lesion existed in the posterior lobe, or the optic thalamus. In these seventy-five cases, also, were twenty-three in which one arm was paralysed. In these, eleven presented the disease in the anterior lobe, or in the corpus striatum; ten in the optic thalamus, or posterior lobe; and two in the middle lobe. Finally, out of these cases were twelve of paralysis of one arm; ten of these presented disease in the corpus

striatum, or anterior lobe; and two only with disease in the optic thalamus, or in the posterior lobe.

These facts prove how uncertain the matter is yet. It would appear that when a simultaneous and equal injury of both corpora striata and optic thalami exists, it would be natural to expect complete paralysis of one side, and I believe there are some cases on record in support of this opinion. But when you have paralysis affecting both sides of the body, you are not to suppose that there is necessarily an affection of the corpora striata and optic thalami, for such symptoms, in the majority of cases, are found to depend upon either an intense congestion of the brain, or a large serous, or sanguineous effusion. The same phenomena are produced by the pressure exercised by the diseased on the sound hemisphere, in a case of local encephalitis, or by disease affecting the upper part of the spinal cord.

With respect to disease of the cerebellum, the only means of determining its affections consists in first considering the seat of the pain, if any, and, in the next place, the effect on the genital system. There are a great number of cases detailed in various treatises in proof of the close connection between the cerebellum and the genital function. I shall relate a few of these. A man, aged thirty-two, got an attack of apoplexy, followed by violent erection of the penis, which continued until death; here we have a case of apoplexy accompanied by priapism. On dissection, the whole of the cerebrum was found healthy; but there was an apoplectic effusion in the middle lobe of the cerebellum.

Another case is given of a man, aged fifty-five, who died of apoplexy in a brothel, and who, after the attack, had violent priapism. On dissection, the substance of the cerebellum was found to be extensively destroyed, and there was an apoplectic effusion in the fourth ventricle. There is a remarkable case on record of a prostitute, in whom the clitoris was extirpated, as it was considered that it was the irritation of that organ which brought on a pernicious habit, by which her health was greatly impaired; and it was conceived that, as soon as the supposed source of excitability was got rid of, she would give up her vicious propensity, and be restored to health. But in this instance it is probable that the effect was taken for the cause; for on her death, which took place some time after, the cerebellum was found to contain a number of chronic abscesses. Serres gives the case of a woman, who died of an apoplectic effusion into the cerebellum. During the fit, she had hemorrhage from the uterus; and, on examining that organ after death, a large clot of blood was found within its cavity, and the broad ligaments, ovaries, and, in fact, every part of the generative apparatus, were in a state of high vascularity. Yet this female was seventy years of age, and her menses had ceased at the usual period. There is a most important case bearing on this point on record. A gentleman, who was subject to constant and distressing nocturnal emissions, consulted his physicians, who, considering them to be the result of debility, prescribed various tonic and

stimulant remedies. He used various preparations of iron, bark, camphor, opium, hyosciamus, nitric acid, and many other things of a similar kind, but without advantage. From the fact of the failure of all these remedies, and the circumstance of his having complained of an occasional sense of uneasiness in the back of the head, his physician was led to think that his symptoms might have some connection with an excited condition of the cerebellum; and, under this impression, had the back of the head shaved, leeches, and covered with a quantity of pounded ice. *From this time his symptoms began to decline rapidly, and in a fortnight he was quite free from complaint.* Now, this case, taken singly, would prove very little; but when we view it in connection with the number of cases in which disease of the cerebellum has been known to be followed by excitement of the genital organs, it becomes of considerable importance. I have now seen two cases in which this connection was observed. In the case of a young man, who was brought into the Meath Hospital some time ago with paraplegia, it was observed that the penis was in a state of constant erection, and there were continual seminal emissions. On dissection, an effusion of blood was found in the cerebellum, and another in the hemisphere opposite the paralysed side. There was another case of a patient who was attacked with apoplexy and paralysis of one side, but with the unparalysed hand he continued to attempt the act of masturbation, so that it was necessary to tie down his hand. On dissection, there were several effusions in the substance of the cerebellum. All these facts strongly go to prove the connection which subsists between the cerebellum and the generative function; and I think it would not be unsafe to make the diagnosis of disease of that organ in cases of cerebral disease, where the genital system was much excited.¹

¹ A case of this kind was published by the editor some years ago, which has been cited as a case of meningitis of the cerebellum, by Dr. Abercrombie. A boy, aged five, pale and delicate, after having been slightly indisposed for four or five days, was seized, on the 9th of August, with violent convulsions. On the 10th, there was fever with delirium; a vacant look of the eye, and an evident imperfection of vision, which appeared by his attempting to lay hold of objects that were presented to him, and missing them; the pupil was dilated, and there was slight strabismus. On the 11th, 12th, 13th, and 14th, the symptoms gradually increased. On the 15th, coma; constant motion of the right arm and leg; the left appearing to be paralysed. In the night, he was seized with violent convulsions, which continued till his death, which took place on the morning of the 16th. On dissection, the brain was found healthy. There was remarkable vascularity on the tuber annulare, forming a thick web of vessels. This was connected with the arachnoid coat of the right side of the cerebellum, which was thickened, with some deposition of coagulable lymph. About four ounces of fluid was found at the base of the skull, but not above a tea-spoonful in the ventricles.

An important point in this case, which Dr. Abercrombie appears to have overlooked, was the connection between the state of the cerebellum and the genital functions; the latter being much excited, and the penis in an almost constant state of erection. See "Case of Arachnitis Cerebelli, by Robley Dunglison, &c. &c.," in the "London Medical Repository," for October, 1822; and Abercrombie "On Diseases of the Brain," 3d edit., Lond., 1836. p. 60.—*R. D.*

LECTURE XXV.

Symptoms of encephalitis—Conclusions as to contraction and paralysis—Remarkable cases of encephalitis—Abscesses in the brain—Sympathetic affections—Enteritis simulating cerebritis—Prognosis in cerebritis—Remote neuralgia a symptom.

To-day we again take up the subject of encephalitis; and allow me here to observe on the extraordinary variety and complication of the symptoms of this disease. Unless you study with extreme care a great number of separate cases of cerebral disease, you will never be able to get clear ideas on the nature of this affection, so peculiarly interesting to the pathologist, and the practical physician. More circumstances seem to combine in creating a variety in the symptoms of cerebral affections than in those of any other viscus of the body. We have in the case of cerebral disease all the variety of symptoms depending on the peculiarity of the part engaged, on the complication of local encephalitis with arachnitis, on the results of pressure, the nature and extent of effusions, the difficulty created by the phenomena of neurosis, and many other circumstances.

At my two last lectures I drew your attention to some cases of local encephalitis, in which the disease was pointed out by certain affections of the muscular and generative systems. There are several other circumstances connected with this part of the subject, which are also deserving of attention, and it is necessary that you should be aware that there are other sources of diagnosis in cases of local encephalitis besides those already mentioned. There is no doubt, that though in many cases the occurrence of contraction, spasms, and pain in the extremities, precedes that of paralysis, yet we may have paralysis from local cerebritis coming on *without these precursory signs*, and as suddenly as in cases of apoplectic effusion. This important fact you must never lose sight of.

Of this I have now seen several instances. I recollect a remarkable case of a man who had been bled in the cold stage of an ague, with the effect of stopping the intermittent. In a few days symptoms of pneumonia set in with great prostration of strength. These were followed by signs of disease of the brain, which were that the patient became suddenly nearly insensible, and on that day was observed to have his hand constantly placed on the right side of the head. Next day, without any preceding spasms or contractions being observed, he was found paralytic in the left upper and lower extremities, with paralysis of the left sterno-mastoid, and loss of sight in the left eye. On dissection we found softening of the two anterior thirds of the right hemisphere, which were of the consistence of thick cream. The disease engaged the corpus striatum, but the optic thalamus was healthy.

Another remarkable instance occurred lately in a person labouring under aneurism of the innominata and hemiplegia. Here the paralysis came on suddenly, and its cause was found to be an

abscess of the brain. I must observe, however, that there were some precursory signs in this case, though contraction and spasms were not observed. The patient had violent headache, and was subject for some time to occasional numbness and pain in the affected arm.

I repeat it, you may have the greatest variety in the succession and combinations of the symptoms of this disease, and this observation applies to the lesion of muscular motion, sensation; the state of the intelligence, and the organic functions. You must study numerous cases to get an accurate idea of this disease. I would advise you to examine the writings of Lallemand, Bouillaud, Abercrombie, and Serres, on this subject, and then consult the last edition of Andral's *Clinique Médicale*, where you will find the value of the symptoms discussed in a most impartial and philosophical manner. In this splendid work you will find many cases of cerebritis, in which the symptom of spasm and alternate flexions and extensions was wanting. Indeed he looks upon it as a symptom which cannot yet be called pathognomonic.

We may, I think, come to the following conclusions on this subject:—

1st. That local encephalitis is often accompanied by various forms of muscular contraction in the parts afterwards to be paralysed.

2d. That in some cases the paralysis is not preceded by muscular contraction, though various lesions of sensibility may occur.

3d. That the paralysis may be gradual, (which is the most common case,) or sudden.

4th. That the contraction may be intermittent, periods more or less elapsing when the symptom is absent.

5th. That in general the contractions occur in the first, the paralysis in the second stage.

6th. That in a few cases the reverse occurs.

7th. That in some cases, general or partial convulsions, and in others, tetanic symptoms, precede the paralysis.

You will see in the *Gazette Médicale*, for October, 1833, the particulars of a most interesting case, recorded by Bérard, jun., of fungous tumours of the dura mater, which was not accompanied by any alteration of muscular motion. This was removed, with the adhering portion of the dura mater, when the patient was attacked, for the first time, with loss of consciousness and convulsions of the trunk and extremities. The operator, justly concluding that the sudden removal of the partial resistance of the brain was the cause of the symptoms, applied a piece of agaric to the denuded surface, and made gentle pressure upon it, when he found that immediately the convulsions ceased, and the intelligence was restored. Thus, gentlemen, does disease often become a second nature, and its want is the cause of symptoms.

As far as we see of the brain, this pathological fact appears certain, that injuries of the upper part of that organ are accompanied by more marked and distressing symptoms than similar lesions of

the lower part. There seems, indeed, to be a decided difference between the sensibility of the superior and inferior parts of the brain. The great proportion of those cases in which there was extensive latent disease of the brain, have been cases in which disease predominated in or towards the inferior surface of that organ. In this situation it has been proved by numerous examples that you may have extensive disease without those symptoms of muscular or mental derangement, which ordinarily characterise inflammatory affections of the brain. I recollect the case of a patient who was brought into our wards complaining of feverish symptoms, with pain of the left temple, extending to the eye of the same side. With the exception of this pain, he had no cerebral symptoms of any kind; his intellect was sound, and he was quite free from muscular pain, rigidity, spasms, or paralysis. He was ordered to take some opening medicine, and to have leeches applied over the seat of the pain, but derived no benefit whatever from their application. This led me to suspect that something unusual was going on, and more particularly when I observed that the leeches were repeated without any decided benefit. One morning on going into the ward I looked about for him for some time to no purpose; in fact, his countenance was so altered that I could no longer recognise him. During the night, the globe of the eye was almost suddenly thrust forward by an enormous œdema of the soft parts of the orbit, and the pain became excruciating. It was then conceived that the pain complained of on admission was the result of disease of the bones of the orbit, and that abscess had formed behind the eyeball. Under this impression, and in accordance with the earnest request of the poor sufferer, it was determined to make an incision to give exit to the confined pus. A curved bistoury was cautiously though deeply introduced over the eyeball, but on withdrawing it, only a small quantity of serum escaped. The swelling went on increasing, and the eyeball was pushed forward so as to be raised above the level of the nose. A curved bistoury was then carried extensively round the orbit, but without giving exit to any matter. Under these circumstances, I came to the conclusion that it was an example of deep-seated abscess of the brain, with symptomatic œdema of the orbit. This œdema of superficial parts, in cases of deep-seated disease, is, you know, a thing of common occurrence, and may be observed in many instances of hepatic abscess, acute pleuritis, and other inflammations. In fact, there is such a remarkable sympathy between deep-seated parts and the integuments over them, that you may have this œdema in deep-seated inflammations of the organ. The patient now became gradually worse, his agony was intolerable, and the protrusion continued undiminished, but he had not either delirium or convulsions. He sank into a state of profound coma, in which he remained for about twenty-four hours, when death put a period to his sufferings. On dissection, there was no pus found in the orbit, and its bones were healthy, but in the inferior part of the

anterior lobe of the brain there was an abscess about the size of a large walnut, resting on the cerebral surface of the orbit. I have since learned from several of my friends that they have witnessed cases of the same description. It is an interesting disease, and one which you should be acquainted with. I think the existence of the following symptoms should lead you to suspect it. First, pain in the head, preceding the appearance of tumour of the orbit, and this pain not affecting the orbit itself; for observe, in this case the pain was referred to the temple and not to the orbit. The next thing is the pain resisting ordinary treatment, and being followed by a sudden œdema of the parts within the orbit, and protrusions of the eyeball. These two circumstances, when occurring in conjunction, should, I think, lead you to suspect acute internal disease. Again, in those cases where abscess supervenes on caries of the internal table of the bones of the cranium, the affection is much more chronic than in this or similar instances of deep-seated abscess of the brain. With respect to this remarkable symptom of local inflammation of the brain, this external œdema, I shall relate the history of another case, as I am anxious to throw as much light as possible on this obscure subject. It may appear strange, that when a dense bony plate and an extremely strong membrane (besides other parts) intervene between the integuments and the seat of disease, that local œdema of external parts should take place as a consequence of internal inflammation. Strange however as it appears, it is true, and the intervention of the skull does not prevent it, as will be seen by the following case.

A boy was admitted into the Meath Hospital, complaining of severe pain in the situation of the mastoid process. He was of a scrofulous habit, and had for a length of time a discharge of matter from both ears, with slight loss of hearing. Some time before his admission the discharge had been very copious, but on being exposed to cold it was diminished in quantity, and he immediately was attacked with severe pain behind one of his ears. When he came into the hospital he was screaming with agony, but had no delirium, and the muscular system was unaffected. But what was chiefly remarkable in this case was, that, on the second day after admission, a distinct tumour formed in the upper portion of the neck, about an inch and a half behind the mastoid process. So distinct indeed was it, that it was generally believed that the disease was periostitis of the base of the skull, which had run on to suppuration. An incision was made over the tumour, and the knife was carried down to the bone, but no matter could be discovered. The patient then became gradually worse, the pain was dreadful, but there were no convulsions. Shortly before death he had a few slight muscular twitches, with delirium, and died in great agony. During the whole course of the disease, the discharge from the ear had continued and was remarkable for its fetor. On examining the brain, we found neither abscess nor arachnitis. On slitting up the longitudinal sinus, a remarkable fetid odour was perceived,

which increased as the incision was prolonged in the direction of the left lateral sinus. Here there was a quantity of extremely fetid matter, of an almost cheesy consistence, and mixed with blood; and a communication was discovered between it and the internal ear, the bones of which were carious, and its cavity filled with the same kind of pus. Here we have a curious example of œdema of the external parts depending on deep-seated disease.

I shall now relate the particulars of a case in which, although the symptoms of an affection of the brain were better marked than in the foregoing, still they were by no means so decided as one would have expected from the appearances revealed by dissection. A patient was brought into the Meath Hospital, with symptoms which were thought to be those which mark the ordinary form of delirium tremens. The man had been a great drunkard, but for some time back had given up the use of ardent spirits. He complained of severe and constant pain of the ear, which he stated to be of twelve weeks' standing, and that it was this which first induced him to give up drinking, as he found that it was always aggravated by the use of spirits. On admission, he appeared to labour under a highly excited state of the nervous system; he had general tremours, and was incapable of keeping up a connected conversation, though he could answer a few questions accurately. Here we observe a remarkable difference between this and the last case detailed, in which there was not the slightest evidence of any lesion of the intellectual powers. In the present case, the symptoms were pain, tremours, and incapability of supporting a rational conversation, but no decided constitutional symptoms. The pain, which had never abated since its commencement, became now violently exacerbated, he moaned frequently, and kept his hand constantly applied to the affected side of the head. To this last symptom I beg leave to direct your attention, as it is an exceedingly common one in cases of local inflammation of the brain. After a few days the mouth was drawn slightly towards the affected side, and it was found that the tongue was protruded in the opposite direction. Symptoms of fatuity now became more distinct, followed by coma, and the patient sank. During the whole course of the disease he had no spasms or paralysis of any of the limbs. On dissection, there was a circumscribed abscess found in the substance of the middle lobe of the brain. The abscess itself was encysted, but the substance of the brain round it was soft, particularly at its inferior part, where it was found to be connected with a carious state of the squamous portion of the temporal bone. There was a considerable degree of softening in that part of the brain which lay between the abscess and the corpus striatum. Here we have a case in which pain of the ear is chiefly complained of; but, in addition to this, it was observed that the patient could not sustain a connected conversation, that there was some fatuity, that the mouth was drawn to one side, and that coma came on before death. Under such circumstances there could be less hesitation in pronouncing the disease to be an affection of the brain; and accordingly we find, on

dissection, unequivocal marks of disease of the middle lobe, in addition to the caries of the temporal bone.

I might detail many cases of a similar kind, without being under any apprehension that I should be occupying your time to no purpose, for the recital of such cases is better calculated to convey information on this obscure subject than any lecture. I shall, however, content myself with one or two more. A man, addicted to the use of ardent spirits, was brought into the surgical wards of the Meath Hospital in a state bordering on coma. It was thought at first that he was labouring under typhus fever, and, under this impression, no particular attention was paid to the cerebral symptoms for the first day or two. At the end of this period, it was learned that he had fallen in going up stairs, while in a state of intoxication. His head was shaved, but no signs of wound or contusion discovered, though his friends persisted still in their statement that he had fallen while intoxicated and hurt his head. When admitted into my wards he appeared moribund; his pulse was imperceptible at the wrist, he had extreme coldness of the limbs, and a disposition to the formation of gangrenous spots about the ankles. He was in a state of stupor; but when roused answered questions tolerably well, and said that he had no pain in his head. The remarkable feature however in this case, was a great degree of muscular rigidity, affecting all the extremities. The fore-arm was flexed, and he had not the power of extending it. The penis was in a state of permanent semi-erection, but there were no seminal emissions. Here was a case in which, taking all circumstances into consideration, the cause of the disease seemed to be in the brain. He had been drunk, and was supposed to have got a fall while in that state; he was comatose, from which, however, he could be roused; and he had rigidity of the limbs, with erection of the penis. With this view I came to the determination of treating it as a case of general inflammation of the substance of the brain. I concluded that there was no arachnitis, from the fact of his answering correctly when roused, while I felt convinced that if there was not actual inflammation of the substance of the brain, there was at least very intense and general irritation. The treatment in this case was successful. After warming the extremities by wrapping them in flannel, and the use of artificial heat, the head was shaved, a large number of leeches applied, and an ice cap ordered to be worn constantly. The leeching was repeated, and he used the ice cap for four days. On the second day after this plan of treatment had been entered upon, there was some improvement, but on the following day the accuracy of our diagnosis of inflammation of the brain appeared, for the patient had violent spasms of the right arm and leg. These however subsided, the coma, rigidity, and other symptoms also disappeared, and the patient slowly but perfectly recovered. In addition to the means of treatment already detailed, the patient's system was placed under the influence of mercury.

A question might arise as to the exact nature of this case. Was

it a case of actual inflammation of the substance of the brain, or was it mere sympathetic irritation produced by some other disease? It may be said that it was a case of gastro-enteritis, with a sympathetic affection of the head. It certainly might be so, but the great probability is, that it was not; because such symptoms as were exhibited in this instance are very rarely the result of gastro-enteritis; and if it was a gastro-enteritis, it is not likely that such complete success should have followed treatment directed to the head. These circumstances make it likely that it was general irritation or inflammation of the substance of the brain itself; and, if so, the case strongly illustrates the utility of mercury, leeching, and cold applications in the reduction of encephalitis. The man was brought into the hospital in a dying state, and recovered under the influence of physiological treatment.

While I am on this part of the subject, namely, the possibility of the head being sympathetically engaged in some instances to a very remarkable degree, I may say that the following conclusions on this point seem to be fairly drawn. That when an affection of this kind depends upon a gastro-enteritis, the signs of cerebral irritation are *general* rather than *local*. In children who are labouring under apparent symptoms of cerebral affection, it has been long known that the irritation of the brain may depend on a variety of causes. In adults, too, the symptoms of cerebral irritation may be the result of various affections, of gastro-enteritis, worms in the intestinal canal, hysteria, hypochondriasis, and many other diseases. In most of these cases, however, particularly with respect to children, the symptoms are general, being pain, delirium, coma, and convulsions on both sides. But we very seldom witness the occurrence of symptoms of local irritation of the brain as produced by sympathy with some other disease, though it is a fact that they may occur occasionally, and without our being able, after death, to discover any existing local encephalitis. A young female was admitted into one of the surgical wards of the Meath Hospital for some injury of a trivial nature. While in hospital she got feverish symptoms, which were treated with purgatives, consisting of calomel, jalap, and the *black bottle*, a remedy which deserves the name of the *coffin bottle*, perhaps better than the pectoral mixture so liberally dealt out in our dispensaries as a cure for all cases of pulmonary disease. She was violently purged, the symptoms of fever subsided, and she was discharged. A few days afterwards her mother applied to have her readmitted, and she was brought in again and placed in one of the medical wards. Her state on admission was as follows:—she had fever, pain in the head, violent contraction of the fingers, and alternate contractions and flexions of the wrist and fore-arm. These muscular spasms were so great that the strongest man could scarcely control the motions of the left fore-arm. In addition to these symptoms, she had slight thirst, some diarrhœa, *but no abdominal tenderness*. On this occasion a double plan of treatment was pursued; the therapeutic means being directed to the head, in consequence of the marked symptoms of

local disease of the brain, and to the belly, from the circumstances of abdominal derangement observed in this and in her former illness. She died shortly after with violent spasms of the hand and fore-arm; and as she had presented all the ordinary symptoms of a local inflammation of the opposite side of the brain, we naturally looked there first for the seat of the disease. After a careful examination, however, no perceptible trace of disease could be found in the substance of the brain, which appeared all throughout remarkably healthy. She had all the symptoms which, according to Serres and Foville, would indicate disease of the optic thalamus, or the posterior lobe of the opposite side, yet we could not find any lesion whatever of its substance after the most careful examination. But on opening the abdomen we found evident marks of disease; *the lower third of the ileum, for the length of six or eight inches, was one unbroken sheet of recent ulcerations.* This case I look upon as a very singular one, showing that we may have well-marked symptoms of a *local* irritation of the brain depending on a sympathetic cause. It is fortunate, however, for the study of medicine, that such cases form the exception and not the rule. I may remark here on the latency of the enteritis as to the pain. There was no abdominal tenderness, a fact illustrative of the great law which so particularly applies to gastro-enteric disease, *that when the sympathetic affections are prominent, the usual or local symptoms are proportionally latent.*

With respect to the prognosis in cases of local encephalitis, the following conclusions seem to be well grounded. As a general rule, the prognosis is to be unfavourable, from the nature of the organ, its importance to life, and the frequent complicated and obscure nature of cerebral affections. In local encephalitis you have always two things to apprehend—the acuteness of the disease, and its subsequent effects. The patient may die of acute inflammation, or, if you control this, of the chronic disorganisation which frequently supervenes, terminating in apoplexy, paralysis, and other consequences. On the other hand, it is consolatory to reflect that experience has proved the possibility of curing both general and local inflammation of the brain. There are numerous cases on record in proof of the success of well-directed treatment. The annals of surgical science are filled with cases of extensive injury of the brain successfully treated; and it is equally true, that medicine can exhibit many instances of well-marked idiopathic inflammation of the brain brought to a favourable termination. In making our prognosis on a case of local encephalitis, much will depend upon the extent to which the muscular system is affected. Spasm of one extremity is more favourable than spasm of both; and an affection of the muscles of the face is not so unfavourable as of those of the extremities. The next thing to be considered is the age of the patient. In the very young, and in persons advanced in life, our prognosis is not to be so good as in the case of one removed from these extremes, as neither of the former admit of such active treatment; but of the two, it is better to have to manage the disease

in a child. It is also singular how well children will often bear active treatment.

There is another point which should not be omitted. There are, in some cases of local inflammation of the brain, muscular contractions and extensions, alternating with a state of rigidity, while in other cases the rigidity is permanent. It is not easy to say which of these cases is the worst, but I believe that the most unfavourable are those in which we have chiefly violent contractions and extensions. Again; with respect to the cessation of the spasms, it may be considered either as a favourable or a most unfavourable symptom. The circumstance of the cessation of the spasms must have been produced by some modification in the state of the cerebral affection. If it be accompanied by a return of the power of transmitting proper motion to the affected limb, it is then a sign of great value, as showing that the cerebral irritation is nearly gone. But if the spasms subside, in consequence of the supervention of *resolution and paralysis*, then the cessation is a symptom of a most unfavourable kind, as showing that actual disorganisation has taken place, which seems to be incurable.

It may be necessary to remind you that if the patient has, combined with these spasms, alternations of delirium and coma, it affords grounds for making a bad prognosis, as such symptoms indicate that the inflammation has extended to the periphery of the brain, and the arachnoid membrane. The state of the intellect is also a matter of importance; the more intact and undisturbed it is, the greater is the chance that the affection of the brain is confined within a small compass. Here, however, I am anxious to impress this upon your minds, that the absence of delirium should not mislead you, or induce you to form any favourable conclusions on that account alone, in cases of encephalitis, for it is a fact that we may have extensive and fatal disease of the substance of the brain without delirium. I need not tell you that convulsions, or paralysis of one side, do not indicate so unfavourable a prognosis as where both sides are engaged. Lastly, you should bear in mind that cases of inflammation of the substance of the brain are very subject to relapse. All these circumstances should be taken into account, and a favourable prognosis should be always formed with a great deal of caution.

I alluded in a late lecture to the occurrence of pain in some particular part of the extremities, as a premonitory sign of this disease. A remarkable case, bearing on this point, has come to my knowledge, and I think I cannot better employ the remaining part of our time than in giving a brief abstract of it. A lady got a pain in the lower part of the tendo Achillis, which was considered to be rheumatic, and very little notice taken of it. There was no swelling, heat, or tenderness on pressure, in the painful part, and the nature of the disease was so imperfectly understood that all the efforts of her medical attendants were directed to the heel, but without any benefit whatever. Matters remained in this state for some time, when she was suddenly attacked with convulsions and coma, and died. On

opening the head some hours after her demise, a large abscess, together with an apoplectic effusion, was found to exist in the opposite hemisphere of the brain. There are various other examples of a similar kind. I have no doubt that many of those anomalous pains are frequently connected with incipient disease of the brain. I know the case of a gentleman, labouring under a painful affection of the face, which had got the name of tic douloureux, and had been subjected to all the variety of treatment which persons labouring under that affection so commonly undergo. But it has since been proved that his complaint is by no means analogous to what has been termed tic douloureux, for it has been most successfully treated by shaving the head, and applying leeches and an iced cap over the seat of the suspected irritation. At present, whenever an attack comes on, he immediately gets a bladder, containing a quantity of pounded ice, applies it to his head, and in this way obtains relief. This shows that the severe pain in his case, which many would confound with a local affection of the nerves of the face, is decidedly the result of a morbid sensibility of the cerebro-spinal centre.

LECTURE XXVI.

Encephalitis—Treatment of in the adult—Importance of energetic means—Dangerous effects of opening the temporal artery or jugular vein—Copious blood-letting from the arm—Difficulty of producing syncope—Employment of cold—Good effects from purgatives—Encephalitis caused by piles—Treatment—Beneficial effects of blisters—Mercury—Dangerous effects of emetics—Dessault's treatment—Use of opium—Violent counter-irritation of coma—Application of boiling water—Treatment of partial encephalitis.

We have now to enter upon the treatment of inflammation of the brain; and you will find that a knowledge of the general principles of the treatment of cerebral inflammation will be quite sufficient to guide you, even in the management of cases which present apparent exceptions to the ordinary symptoms. The truth is, that the principles which should regulate the treatment of inflammation of the brain are nearly the same in all cases.

I shall commence with the treatment of the acute form in the adult. Acute phrenitis in the adult is an exceedingly severe disease, characterised in its first period by an high exaltation of the functions of the brain, and in its second by a corresponding depression. In this form of disease we have generally high fever, a strong bounding pulse, throbbing of the carotids, intense pain of the head, great brilliancy of the eye, with intolerance of light, vivid redness of the face, a ferocious countenance, and furious delirium. Under such circumstances there is no time to be lost; the brain is a delicate organ, and cannot bear much disease, and its powers of

recovering from idiopathic disorganisation seem much less than those of the lungs or abdominal viscera. Indeed, we must believe that, notwithstanding the assertions of Lallemand, it remains to be proved that recovery can take place after the stage of softening has set in, in idiopathic encephalitis. The brain differs from the lungs or digestive organs in having no excretory duct for the products of inflammation, and hence one cause of the greater danger of its idiopathic inflammations than its traumatic, where an opening is formed in the skull. In such a case you have to apprehend two pathological lesions, the inflammatory softening of the substance of the brain, and the inflammation of its serous membranes, with effusion into their cavities. The patient, too, may die from congestion, or even an apoplectic effusion may occur, illustrative of the proposition of Broussais, that all encephalic irritations may produce an apoplexy. I have seen this termination, even in the infant under a year old; in such a case I once saw an apoplectic effusion which had supervened in the course of an arachno-cerebritis, and which amounted to several ounces of blood. Every moment is precious, and no consideration should induce you to put off, even for an hour, the adoption of the most rigorous measures. In the first place, you must bleed; and here let me remark that blood-letting should be performed so as to make a decided impression on the symptoms. It will often happen, that, from the state of uncontrollable fury which the patient is in, it is dangerous and almost impossible to bleed him. Here you must endeavour to moderate the delirium, and there is no way by which you can accomplish your purpose so fully as by cold dashing. Where there is high delirium, I believe you will always find it the best plan to precede venesection by throwing a few basins of cold water over your patient's head. This will procure an interval of comparative tranquillity, during which you can open either a vein or an artery with convenience and safety. Of course, if any thing like collapse ensues (which is possible) you will not bleed immediately. The object of the cold pouring, under these circumstances, is to obtain such a diminution of the fury as will allow of your bleeding the patient with safety, as to the operation. If you cannot reduce the cerebral excitement by this means, it will then be necessary to put on the strait waistcoat, *pro tempore*. There is a difference of opinion among medical men with respect to the mode of abstracting blood; some prefer taking it from the arm, some from the jugular vein, and some from the temporal artery. Now, I am inclined to think that it is better to open a vein in the arm, and that venesection performed in this way will be found to answer every purpose. It is said that if you take blood from the temporal artery or jugular vein, you deplete the brain more directly than you would by opening one of the brachial veins. This may be true, though I think it still remains to be proved that the drawing of a smaller quantity of blood from these vessels will have a more powerful effect on the system than from the arm. If you open the temporal artery, there are two disagreeable circumstances which you should be prepared to meet.

In the first place, the patient is in a state of furious delirium, you don't know how long this may last, and it may happen that in one of his paroxysms he will tear off the bandage, and, if not watched, bleed to death. A case of this kind occurred not long since in the person of a gentleman of this city, who had the temporal artery opened. He tore off the bandage, and a terrible hemorrhage ensued; assistance was procured, and the bandage re-adjusted; he tore it off a second time, and died shortly after, his death being evidently accelerated if not actually caused by the quantity of blood lost. Again, it is possible that an aneurism may be formed as a consequence of the operation, which may excite a determination to the head, and tend to keep the patient in a state of excitement. Thirdly, you must employ a bandage to secure the artery, and to this there is a strong objection, in consequence of the pressure which it makes on the external vessels of the head. I am therefore strongly opposed to opening the temporal artery in cases of acute inflammation of the brain, accompanied by high mental or muscular excitement. Now, with respect to the jugular vein, you are aware that to command this vessel pressure is also required. How this pressure can be made without interfering with respiration and compressing the veins of the neck, so as to add to the existing congestion of the head, I am at a loss to know. I would advise you, therefore, when you bleed in phrenitis, to prefer opening a vein in the arm; by making a free incision you can draw blood in such a way as to make an impression on the system, fully equal to that produced by either of the foregoing modes; and without subjecting your patient to the same degree of inconvenience or risk. The quantity of blood to be taken away must be regulated by the age, strength, and constitution of the patient, as also by the intensity of the disease. Where you have to deal with a young man of robust constitution, your first bleeding may amount to thirty ounces. You will often find it difficult to produce fainting in this disease, for the excited condition of the brain keeps up a constant determination to that organ, and prevents syncope. The same difficulty is met with in cases of hypertrophy of the left ventricle, which causes a great determination to the head.

Your next step is to have the head shaved. Never omit this. The very circumstance of freeing the head from the covering of hair, and permitting the free contact of air with the scalp is of advantage; and if you wish to employ cold applications, you cannot do so properly without premising this operation. After you have done this, you should apply a large number of leeches to the scalp, or if you cannot readily procure leeches, employ instead of them light scarifications to the temples and nape of the neck, and keep on the cupping-glasses until you have obtained a sufficient quantity of blood. By acting in this way with promptness and decision, you arrest the violent symptoms and gain time.

In treating a case of this kind it is a very common practice to use cold applications. They are for the most part applied in shape of a cold lotion to the head, but I need not tell you that this is a

very imperfect mode of using them, and indeed I have seen but very few persons who were acquainted with the proper mode. Persons are in the habit of supposing that the mixture of a certain quantity of saline ingredients with water should produce a very cold lotion, and so it does indeed while the salts are dissolving; but as soon as this is accomplished, the mixture rapidly acquires the temperature of the surrounding air. The solution is generally prepared by the apothecary, (and sent in a bottle, as if they could cork up the cold,) but the cold is quickly lost, and, in a few moments after the lotion has been applied, you will find it tepid, and passing into a state of vapour. Now if you wish to derive any benefit from the use of cold applications, you must stand by yourself, and see the thing properly done. The object is to have the scalp kept constantly cold, and this can be done only by the repeated application of cold lotions. If you prefer saline lotions, you should have them made by the bedside, and applied *while in the act of solution*, or you should put a quantity of ice into your lotion, for while a single piece of the ice remains undissolved, the temperature of the lotion will be very little above the freezing point. A very good way is to have a jar of cold water with a quantity of ice in it, and to apply cloths dipped in it every minute, taking care not to immerse the hot cloth into the iced water until it has been wrung out in another vessel of water. You may also use the ice cap, though this is a painful remedy. But the mode of using ice to the head, which I prefer in all cases, and particularly in that of the child, is to take a piece of smooth ice, about the size of a dollar, and half an inch thick; this is to be placed in the hollow of a fine cup sponge, and steadily moved over the whole shaved scalp. By this mode you prevent the pain which the iced cap produces, and the sponge absorbs the water produced by melting, and the application may be continued for an indefinite length of time. But one of the best modes of applying cold to the head is that recommended by Dr. Abercrombie, and, as far as my experience goes, I can safely affirm that there is scarcely any remedy of such unequivocal value in acute inflammation of the brain or its membranes. Dr. Abercrombie's mode is this—the scalp being first shaved, you direct the patient's head to be held over a basin, and then taking a jug of cold water, pour its contents over the head from some height in a small continuous stream. This measure, simple as it may appear, is one of extraordinary efficacy. In fact, so great and instantaneous is the depression of the vital power produced by this mode, that it must be used with caution. There are numerous cases of persons in the highest state of maniacal excitement, reduced in a few moments to a low and weak state by this powerful remedy. There are also instances of its rapidly depressing effect in the early stages of acute hydrocephalus. I have used it more in the phrenitis of adults than in the hydrocephalus of children; but in the latter disease I know many instances of its value, and believe it to be only secondary to the application of leeches. In acute inflammation this form of cold effusion should be employed every hour or half hour,

according to circumstances, and if you wish to increase its efficacy you can do it by placing the patient's feet in warm water at the time of its application. Here, then, gentlemen, is the first set of remedies you should employ in a case of acute phrenitis; a full bleeding from the arm, premising it, if there be great maniacal excitement, by dashing a basin of water over the patient's head; shaving the head, and applying a large number of leeches, or if these are not within reach, the use of cupping; and, lastly, the constant application of cold lotions, or the use of the cold affusion after the manner employed by Dr. Abercrombie. These are the great measures which should be boldly and promptly put in practice, in order to counteract the first violence of a case of acute inflammation of the brain.

You will next act upon the bowels by purgatives. This is a matter of the deepest importance, for there is hardly a disease in which the judicious administration of purgatives has been followed by more decidedly beneficial effects, than in inflammation of the brain, where the digestive tube has been in a healthy condition. Purgatives are also found to be of great benefit in the simple hydrocephalus of children, and in several cases it has been observed that the disease did not yield even after active bleeding, until purgation had been employed. Dr. Abercrombie speaks in the highest terms of the value of purgatives, even after coma has set in. The purgatives which are generally used are those of the drastic kind, and they may be given by the mouth or in the form of enemata.

Such are the rules for the treatment of the ordinary form of acute encephalitis. I shall now make a few observations with respect to the local applications. It may not be necessary to repeat the venesection, particularly if the means which I have recommended be put in practice in a regular and proper manner, but it will in most cases be requisite to repeat the leeching. *Even in the advanced stage of the disease, and after coma has made its appearance, Dr. Abercrombie lays great stress on the benefits derived from the application of leeches;* and I think I have myself saved some lives by the employment of leeches, even after the supervention of coma. In all violent cases I would recommend strongly to you the using relays of leeches from the first, to keep up a continual detraction of blood. In addition to this, the patient must be kept perfectly quiet, all loud sounds, and the stimulus of light avoided; the room should be kept cool and well-aired, the bed-covering light, the attendants few, and the nurse should be a person of cool temper and steady disposition.

These are the principal measures to be employed in the treatment of acute inflammation of the brain in the adult; there are certain cases, however, in which you may add to these measures others of a different kind, particularly in cases where the disease has occurred as a consequence of the metastasis of inflammation from other parts. Suppose you have a case of rheumatism, or of some suppressed evacuation in which there is a metastasis to the

brain. Under such circumstances, while you employ the means I have mentioned for the purpose of subduing cerebral inflammation, you will also put in practice the best measures for restoring the original disease. Here, however, you should bear in mind, *that your attempts to bring back the original disease are always to be looked upon as secondary to those for the direct removal of the existing irritation of the brain.* Some practitioners, in such cases, content themselves with endeavouring to restore the original affection, but this is playing a dangerous game. An organ of vast importance to life is affected, and you cannot calculate how far the inflammation may proceed. You should never neglect taking proper steps at first to reduce inflammation, while at the same time you need not neglect the means calculated to bring back the former disease. If the encephalitis be caused by the suppression of bleeding piles, or a sudden checking of the menstrual flux, leeches to the anus or vulva are found useful along with the direct treatment. If the disease be produced by the repression of an exanthematous eruption, the same principles apply. You should never omit employing the means for bringing back the original affection, but you should always recollect that they are to be secondary to the measures adopted to directly relieve the cerebral excitement.

With respect to the use of blisters, the same rules apply here as in other cases of disease treated of during the course. They are never to be used in the early stage of the disease, and while active inflammation is present; and, as a general rule, I believe it is better to apply them to the nape of the neck, or the inside of the legs, than directly to the head. There is only one case in which you can apply them with advantage to the head itself, and this is where there is coma with a cool skin. Here the stimulus of a blister is frequently found to be highly useful.

As to the use of mercury in cases of acute cerebral inflammation, I think we have not as yet a sufficient number of facts on which to form any decided opinion. If we look to hydrocephalus, we shall find that there are many cases in which the symptoms did not yield to the ordinary measures until mercury was employed; this, however, we do not find to be so much the case in the acute inflammation of the brain in the adult.—I shall return to this subject on a future occasion.

I have little doubt that emetics are very dangerous in this disease, from the determination to the head which they produce.

Any of you, gentlemen, who has vomited, cannot forget the violent sense of tension about the head with which the act is accompanied; and, if the brain be in a state of acute inflammation, you can readily conceive how injurious such an effect must be. The use of emetics in this disease has been adopted in consequence of a misconception of the opinions of Dessault. He attributed extraordinary efficacy to the use of tartar emetic, in cases of injuries of the head. But you must be aware that Dessault did not give tartar emetic so much with the view of exciting emesis, *as of producing a degree of nausea calculated to keep down inflamma-*

tory action. Moral, who was a pupil of his for five years, makes a statement to this effect, and says that so far from proving beneficial when it vomited, the tartar emetic was always attended with unfavourable results. When it acted on the skin, or by stool, he says the effects were favourable; but when it vomited, the symptoms of cerebral excitement were always increased. Under these circumstances, I think you should be cautious in having recourse to the use even of tartar emetic, after the manner of Dessault; for even in this way you run the risk of vomiting. On this point we have eight very instructive cases given by Lallemand. In the first two cases, where emetics were used, the head had been merely threatened. The emetics were followed by profuse vomiting, and this by symptoms of *violent cerebral excitement and rapid death.* The third case was that of a patient who had apoplexy: the emetic was followed by symptoms of inflammation of the brain and death. On dissection, there were marks of inflammation discovered round the clot. Now it has been observed, in several instances, that where the substance of the brain round an apoplectic clot became inflamed, that, in addition to the phenomena of apoplexy, symptoms of a spasmodic affection of the muscular system supervened. Here we see, that after the use of an emetic these symptoms appeared, and their nature was verified by dissection. In the remaining five cases, where emetics were employed, the cerebral affection was rather increased than diminished; and, in some of them, disease of the digestive tube was superadded. Weighing these circumstances calmly, I think the use of emetics in acute inflammation of the brain may be considered dangerous.

With respect to opium I must say, that I am strongly opposed to its employment, at least in the early stage of encephalitis. I have seen many cases of hydrocephalus in children, in which opium seemed to be decidedly injurious; and I believe that in all cases where there is congestion of the brain, its employment will be attended by bad effects. But when all the symptoms of active inflammation have passed away, and when there remains a peculiar nervous condition of the brain, characterised by symptoms of mental excitement and persistent watchfulness, somewhat resembling delirium tremens, here, I believe, that you may have recourse to opium with much benefit. In many cases where the antiphlogistic treatment had been properly employed at the commencement, there frequently remains a neurotic condition of the brain, accompanied by great irritation and absence of sleep; and in such cases I have seen much good resulting from the use of opiates. When I speak of fever I shall return to this subject.

In the treatment of this disease, I am anxious that you should always bear this principle in mind—that you cannot be too cautious in adopting means of coercion. Coercion has always a bad effect: it should never be resorted to, except in cases of extreme necessity; and you should never suffer the patient's attendants to employ it without your express permission. It is a common practice in hospitals, where the attendants always wish to save trouble, to put on

the strait waistcoat as soon as the patient exhibits symptoms of delirium. What is generally the result of this treatment? The poor sufferer becomes irritated by confinement, and uses the most violent efforts to liberate himself; his struggles increase the excitement of the brain, and prevent the measures you employ from taking effect. I have known many melancholy cases, illustrative of the abuse of the strait waistcoat. I shall give you one:—A female, of delicate habit, was attacked with fever and some delirium. She was supposed to labour under disease of the brain. They put a strait waistcoat on her, and tied her down to the bed, where she remained for several days in a most deplorable state. A medical man, who was called in to see her at this time, found her in the situation described, with her head shaved and blistered, and her strength sinking. It struck him that there was something peculiar in the case, and he asked her several questions with the view of testing her sanity; and, finding that she answered rationally, he immediately directed that the strait waistcoat should be taken off. She then told him that, during the whole course of her illness, she had laboured under pain of the right side. He examined her side, and found a large tumour in the situation of the liver. There was also an eschar on the back. She died shortly afterwards; and, on dissection, the liver was found to be in a state of extensive suppurative disease; the brain perfectly healthy. It is unnecessary for me to make any comment on this case.

While, however, I deprecate coercion as a common mode of proceeding, I fully admit that cases will occur that demand it for the safety of the patient. The dreadful tendency to suicide is one of the characters of this disease, and must never be forgotten in any case. All that I wish to impress upon you is, that coercion must be used with great caution, and only so long as it is absolutely necessary. When we come to treat of the nervous systems in fever, I shall recur to this subject.

In all cases of cerebral disease you should never omit enquiring into the state of the bladder, for there is often retention of urine. This is to be obviated by drawing off the urine with a catheter, two or three times a day.

You will meet with cases of cerebral inflammation in the last stage, with profound coma, general paralysis, an imperceptible pulse, and tracheal rattle. It is a melancholy thing to be called to a case of this description, where the ordinary means furnished by medicine are so inadequate to the removal, or even the alleviation of symptoms; and yet it is a fact that, even under these circumstances, cases have been cured by the adoption of an extraordinary measure. This consists in the employment of enormous and sudden counter-irritation, by pouring boiling water over the lower extremities, while, at the same time, ice is applied to the head. This is certainly an extraordinary and barbarous method; but it has succeeded in rescuing the patient, as it were, from the jaws of death. One of the most singular cases of this kind is recorded by Lallemand—that of a man upwards of sixty, who, in consequence of a fall on

the head, was attackd with encephalitis, which was mistaken for an essential fever until the tenth day. At this time he was first seen by Lallemand, who found him labouring under severe and long-continued syncope; the right extremities flexed; the hand firmly closed; the surface on this side insensible; the eyelids closed; the eyes turned up, squinting, and insensible to light; complete loss of hearing and intelligence. The body was covered with a cold viscid sweat; the respiration frequent and stertorous, and the pulse absent. Lallemand proposed pouring boiling water on the ankles, and, at the same time, applying ice to the head, an advice which was consented to with great reluctance by the other medical attendants. At the moment the boiling water was applied, there was a sudden motion of the whole body: the left arm was agitated, the eyes opened, and the pulse could be felt at the wrist. In half an hour the boiling water was applied to the thighs with still greater effect; colour returned to the face, and the pulse became fuller. From this time improvement went on. Deep suppurating wounds were produced by the boiling water which took more than six weeks to cicatrise. The patient's recovery was perfect.

In Dr. Mackintosh's work you will find this practice recommended. It is indeed an extreme remedy, and one which, for many reasons, practitioners would have repugnance to use; but it is well to be acquainted with such a powerful remedy, and to know that it has succeeded under the most desperate circumstances.

With respect to partial encephalitis, the principles of treatment are the same. In this form of disease you will often have to contend with the prejudices of the patient, and sometimes of practitioners who do not recognise its existence. Its symptoms, you will remember, may at first appear slight or insidious, and to the superficial observer less referable to the head than elsewhere; yet the disease is full of danger, slight though it appear. The recent researches on this subject have shown, too, that it is commonly a comparatively acute disease. Andral gives a table, showing the periods in one hundred and five cases: in eighty-nine of them death occurred within a month. The liability, too, of secondary complication, with general congestion, arachnitis, or apoplexy, must be always borne in mind.

When the symptoms of a local encephalitis are decided, I think you should always commence by bleeding from the arm, and then apply relays of leeches and cold lotions to the opposite side of the head. You will also find the application of tartar emetic ointment, so as to bring out an eruption as soon as possible, of great value in cases of this kind. Above all things, take care to relieve the symptoms by prompt and decided measures before the stage of paralysis comes on, for when this arrives, I believe you can do very little in the way of cure. I have seen three cases in which, after the depletions, the symptoms were relieved by bringing the patients rapidly under the use of mercury; and I think local inflammation of the

brain may be treated by mercury as well as localised inflammation of other parts. My late lamented friend, Dr. Leahy, communicated to me the particulars of two cases in which pain, spasms, and other symptoms of a local encephalitis were present, and in which complete relief was obtained as soon as mercurial action was brought on. I recollect an old lady who got pain in the right side of the head, with contraction of the finger of the left hand, and alternate flexions and contractions of the fore-arm, accompanied by slight lesion of the intellectual functions. She was leeches three or four times, blistered, and purged, without any decided relief. I then determined to try the effect of calomel, and was gratified to find that, according as her mouth became affected, the pain and contraction of the fingers, as well as the motions of the fore-arm, diminished considerably, and as soon as full ptyalism was established all her symptoms disappeared. This case is particularly interesting, inasmuch as it shows that the ordinary treatment by leeching, counter-irritation, and purging, failed in giving relief, so that we are justified in attributing some value to the use of mercury. In the advanced stages of this disease, it seems right to employ a seton in the back of the neck; and I would advise all who have been attacked to continue the use of this remedy for a great length of time.

The term *ramollissement*, or softening of the brain, is one which is very extensively used, and I fear often without any precise idea of its meaning. In ninety-nine cases out of a hundred this ramollissement will be found to depend upon local inflammation of the brain; of this I do not entertain the slightest doubt. I think we may very safely consider it as analogous to the softening of the lungs, liver, or spleen, or from inflammation of their texture. There is a peculiar softening of the brain in old persons, which we cannot connect with actual inflammation, but in all cases in the child, and in almost every case in the adult, ramollissement of the brain will be found to depend on inflammation. I do not mean to infer from this that it is in our power to cure every case of softening of the brain, for when it once sets in, the great probability is that the texture of the affected part is destroyed; but we can cure many cases by subduing the inflammation from which it derives its origin. Of course we cannot expect to accomplish this in the case of old persons, where the symptoms come on without any inflammatory phenomena, as in that peculiar softening of the brain which forms the subject of Rostan's work, and occurs in persons beyond the age of seventy. This appears to be a species of senile gangrene. That form of ramollissement, which occurs in adults and children, is, however, very different from this, being, in the vast majority of cases, the result of inflammation. You will hardly ever dissect a case of partial encephalitis in the adult, or of hydrocephalus in the child, without finding more or less of this inflammatory softening.

LECTURE XXVII.

Analysis of symptoms of cerebritis—Inconstancy of pain—Arachnitis, pain of—Intermittent pain—Headache—Phenomena of the eye—State of the pupils—Various affections of the functions of vision—Researches of Parent and Martinet—Relief by convulsions—Brain considered as a secreting organ—Dangerous effects of opium; delirium—Phenomena of organic life—Vomiting in hydrocephalus—Sympathies of the digestive and respiratory systems—Treatment of hydrocephalus—Of internal remedies—Cancrum oris, treatment of.

Before we leave the subject of inflammation of the brain, I shall draw your attention to a brief analysis of some of the more prominent symptoms of this disease; and here I am anxious to impress upon you, that the true mode of studying this subject is not by reading the descriptions given by this or that systematic writer, but by the careful perusal of *monographs*, in which the details of a great number of cases, occurring under different circumstances, are accurately reported. You would be mistaken, indeed, if you were to conclude that you had acquired a thorough knowledge of the symptoms of phrenitis or arachnitis by reading the description of Cullen, Thomas, or Mason Good. The only mode of studying the subject properly is, to take accurate notes of every case which you meet with, and to study with care those monographs in which a number of cases, attended by different symptoms, are detailed with impartiality.

I would not occupy your attention further with this subject, but that there is much error prevailing with respect to inflammation of the brain and its membranes. Persons are in the habit of supposing that these symptoms are always constant and well marked, but, the truth is, they are subject to very great varieties. The first symptom, to which I shall call your attention, is *pain*. This, you will recollect, is a prominent symptom of most visceral inflammations, where the disease is situated on, or close to, the surface of the organ; but, when it is deep seated, this symptom becomes more or less obscure. Now, in a case of arachnitis, we have a double source of pain—one depending upon the affections of the serous membrane, the other arising from the circumstance of disease being situated on the surface; and hence it is that, in the great majority of cases of arachnitis, pain is a constant and prominent symptom. Still, if you were to conclude that pain is *always* present in arachnitis, you would be wrong—for there are many cases on record in which it was either partially observed or completely absent. You will be greatly assisted in your pathological studies by attending to the different results of inflammation of analogous structures, for we find that in some of the inflammatory affections of serous membranes there is little or no pain. We may, for instance, have pleuritis, pericarditis, and even peritoneal inflammation latent, so far as pain is concerned; nay, many persons have gone so far as to say, that it is only where the muscular tissues of the belly are engaged that we have pain in peritonitis. I have seen pericarditis run

through all its stages without any pain being complained of by the patient. Now, if this absence of pain be a matter of no unusual occurrence in some inflammatory affections of the pleura, pericardium, and peritoneum, there is no reason why it may not occur in some cases of arachnitis. Still, it must be acknowledged that pain is one of the most remarkable and constant symptoms of arachnitis, and that, of all the serous membranes, the arachnoid seems to be endowed with the greatest sensibility.

We might enquire, here, whether the pain of cerebral inflammation be significant of any particular lesion of the brain. I believe that upon this point the state of our knowledge is very unsatisfactory. Pain, as a symptom of cerebral inflammation, occurs in very different cases. We may have it in connection with disease of the superior, lateral, or inferior parts of the brain; we may have it in cases where the result of the disease is a serous, hemorrhagic, or purulent effusion. The rule, then, to be borne in mind is this: first, that it is present in the great majority of cases of arachnitis; next, that it may accompany many different lesions; thirdly, that it may be absent; and lastly, that, with the same lesions, we may have pain in one case and absence of it in the other.

The next subject for enquiry is, does the seat of pain generally point out the seat of inflammation? Andral distinctly affirms that it does not. In some cases, pain of the frontal region has been found to accompany disease of the ventricles, and pain in *one side* of the head, an affection of the arachnoid covering of both hemispheres. We see the same thing occurring in the case of other serous membranes. Thus, in the pleuritic inflammation of phthisis, pain is very seldom felt in the situation of the disease, but generally lower down; and I have seen some cases in which pain has been complained of only in the sound side. I recollect a case of very extensive pneumonia, in which the patient complained only of some pain in the region of the kidney and small of the back.

The pain which accompanies arachnitis generally sets in at an early period of the disease, and is characterised by great intensity—two circumstances in which it resembles the pain of pleuritis. In most cases, it is found that any thing that impedes or oppresses the circulation of the brain increases this pain; and hence it is that some practitioners are led to think, that, if pain of the head be relieved by pressure, it cannot be inflammatory. Now, I wish to call your attention to this point, because, in some cases where evident marks of arachnitis were found after death, it was observed that during life the pain of the head was relieved by pressure. The patients have been found with a bandage tied firmly round the head, from which they experienced decided relief, and yet a post mortem examination gave unequivocal proof of the existence of arachnitis. So far, then, as these cases go, it appears that the mere fact of pain being relieved by pressure does not prove that it is unconnected with an inflammatory cause. The pain, too, of an arachnitis may be intermittent, and continue to exhibit this character even for a considerable length of time. I have seen many

instances of this in children, where the little patient was seized with acute pain of the head at a particular time of the day, which, after a few hours' duration, subsided, and then returned again the next day at precisely the same hour, and continued in this way for several weeks, until at length his friends were surprised by the unexpected supervention of coma, convulsions, or blindness. I knew two cases of this kind in which the intermittent character of the pain was so prominent as to engross the practitioner's whole attention; so that the real nature of the affection was overlooked, and bark prescribed. I have now witnessed three or four of these regular quotidian attacks of pain in children, which, after continuing for days and even weeks, were suddenly followed by perfect blindness—in some cases with, and in others without coma.

You might here ask, whether pain is to be considered as a diagnostic of arachnitis? I cannot say it is. We constantly meet with severe pain of the head without arachnitis, and every one knows that the headache of fever is by no means an indication of inflammation of the brain. In many cases of hysteria, the headache and determination of blood to the head are violent, and yet unconnected with inflammatory action. I know a young lady who is frequently attacked with most agonising headache, accompanied by violent throbbing of the carotids and great heat of the face and scalp. Yet, in this case it is plain that the pain cannot be inflammatory, for she has been subject to these attacks once or twice a week for the last six years, and yet continues otherwise in a state of good health. If her disease were to be measured by the violence of the pain and determination of blood to the head, it would be natural to expect that death would have long ago put a period to her sufferings. This is another proof of the truth of the opinion, that there is no single pathognomic symptom of disease. Bear this in mind. I might go farther, and say, that, whether we looked to symptoms or to signs, the rule was the same. The man who merely looks to a single sign or symptom will frequently err; it is only from the whole group of signs and symptoms presented by a disease that we can arrive at any accurate diagnosis.

The state of the eye, in cases of arachnitis particularly, has attracted much attention. On this subject much valuable information has been obtained by the laborious investigations of Andral, of which I shall give an abstract. He states that the phenomena of the eye, in cases of cerebral inflammation, may be reduced to three classes; its motions, the various conditions of the pupil, and the state of vision. With respect to the first of these, it may be observed that in some cases we find the eyeball in constant motion; in others, it is quite fixed; while in others the balance of muscular power is lost, and there is a constant tendency to strabismus of one eye or both. Of all these varieties in the state of motion, the last appears to be the most valuable, so far as the diagnosis of arachnitis is concerned. By many persons this strabismus is looked upon as a sign that *effusion* has taken place, and that the disease has reached its incurable stage; a position which I am inclined to doubt, from

having seen cases recover in which this symptom was present. However, Andral looks upon strabismus as a very valuable sign, and thinks that, of all the lesions of motion of the eye, it is the most important with respect to the diagnosis of *arachnitis of the ventricles*. With respect to the condition of the pupil, it is stated in books that in the early stage you have a contracted, and in the advanced a dilated pupil, and that the latter condition signifies that effusion into the brain has taken place. Now, the truth is that this statement must be received with great caution, and as admitting of numerous exceptions; for it has been established that the same lesions of the brain are sometimes accompanied by very different conditions of the pupil, and *vice versa*. Parent and Martinet, who have investigated the subject carefully, are the best authorities on this point, and I shall give a brief abstract of their experience. In cases where *both pupils were dilated*, they observed that in some there was effusion into one of the ventricles, in others, into both. In cases where there was no dilatation, they observed that in some there was serous or purulent effusion under the arachnoid, while in others, in which there was no effusion whatever, the pupil was dilated. Lastly, it was found that in some cases, where only one pupil was dilated, there was effusion *into both sides of the brain*. You might here ask, whether effusion into the substance, or on the surface of one side of the brain, is connected with a dilated condition of pupil? In reply to this, it may be stated that effusion into the substance—not of one, but of both hemispheres—has been known to be accompanied by a contracted state of the pupil to the last. You may also have one pupil contracted and the other dilated; nay, you may have *an alteration of these conditions*—the right being dilated to-day, the left to-morrow. The mere circumstance, then, of dilatation or contraction of the pupil is no sign, when taken by itself, as to the seat or even the existence of effusion; for you may have either condition with or without effusion, and you may have dilatation of the pupil of one eye with an effusion into both sides of the brain. As a general rule, however, it seems to be made out, that, in most cases of cerebral inflammation terminating in effusion, there is often, towards the advanced period of the disease, some dilatation of pupil, and that this condition generally marks the occurrence of effusion.

With respect to the affections of the function of vision, there are great varieties. Some patients have double vision—others see sparks of fire, or *muscæ volitantes*. There are many other phenomena of the kind, causing a great variety in the symptoms; and this variety is found to depend more on the susceptibility of the brain to irritation, rather than on the mere existence of irritation of the serous membrane investing it. The same rule applies to all cases of serous inflammation, the phenomena of inflammation varying according to the susceptibility of the organ which the inflamed membrane covers. Thus, for instance, one patient will have pericarditis with palpitations of the heart, another without them; their occurrence or non-occurrence merely showing that the

heart is more or less susceptible to irritation. So it is with respect to the brain, and the symptoms of deranged vision are connected with the greater or less susceptibility of the organ, which we know varies very considerably in different persons. This remark applies to all the forms, and, I believe, all the phenomena of meningitis.

In acute disease of the brain and its membranes, we often have convulsions and paralysis, and in these symptoms also we find great variations: in some we have convulsions of one side, in some of both, in others we have paralysis, but scarcely any convulsions. The same remark also applies to these symptoms, as to some already mentioned—namely, that we cannot from them alone form an accurate estimate of the situation or amount of disease. You may have convulsions and paralysis of various kinds with the same kind of lesion, and you may have a variety of lesions with the same paralysis and convulsions. The only thing that appears to be pretty well established is this—that, generally speaking, in cases where the right side of the brain is engaged, you have convulsions and paralysis of the left side of the body, and *vice versa*.

Before I proceed to speak of delirium, I think it necessary to say a few words more with respect to convulsions, as I find Andral has not touched on a point to which I beg to call your attention. The occurrence of convulsions in a child, labouring under symptoms of inflammation of the brain, is always looked upon as formidable; and indeed it is natural that convulsions, to persons unacquainted with pathology, should seem to point out a great intensity of disease. I have, however, been long of opinion that convulsions occurring during the existence of hydrocephalus in children, or of meningitis in adults, are not so dangerous as persons generally think. I will even go so far as to say, that the worst cases I have seen, in which a cure was effected, were those in which there were the greatest and most violent convulsions; and that, in most of the cases which appeared to go on without any benefit from medicine, there were scarcely any. I am of opinion that convulsions are often of benefit by giving relief to the brain. This statement must appear somewhat paradoxical, but I trust I shall be able to prove to you that it has some foundation in truth. Broussais has taught that there appear to be two great modes of reaction in the economy, to obviate the effects of abnormal stimulation applied to important viscera—fever and convulsions. The irritations which attack the cerebro-spinal system may be relieved by convulsions; those which attack the viscera may be relieved by fever and secretion. This doctrine, I think, might be expressed otherwise. The irritations of organs are often relieved by an increase, with or without alteration, of their secretions. But, as we have used the term *secretion* to express something material, we apply the proposition merely to the viscera of organic life. Now, it may also be extended to the organs of animal life. A violent expenditure of nervous power may relieve the brain or spinal cord, and delirium and convulsions prevent or modify organic changes, just as secretion from the lung or bowels may prevent ulceration.

I have said that the brain might be relieved by convulsions. Let us, holding this assertion in view, compare the phenomena and results of apoplexy with those of epilepsy. In the first place, it is to be remarked that the earlier phenomena of both are the same—namely, an active congestion of the vessels of the head. Any one who has seen the first stage of both must admit this. But let us follow them up through their remaining stages. In the one, we have the determination to the head, followed by convulsions more or less violent and protracted, which, however, subside after some time, and the patient gets well; in the other, there is either death from the violent determination of blood and probable effusion, or, if the patient recovers, there is very often paralysis, showing that injury has been done to the substance of the brain. Now, here we perceive that the case of determination without convulsions is that in which there is either death or recovery with paralysis; there are no such bad consequences to be dreaded where the determination to the head is followed by convulsive fits. In apoplexy we have congestion followed by death, or recovery with paralysis; in epilepsy we have congestion, convulsions, and relief. It is plain that, if we admit the identity of the phenomena in the early periods of both, we must then also admit that the only cause of relief we can ascertain is convulsions. This idea of the subject will explain how it is that a man may continue for years subject to repeated attacks of cerebral congestion, and yet continue to enjoy tolerable health. It will also explain why it is unnecessary and sometimes even dangerous to bleed in epilepsy. It also shows why it is so often unaccompanied by paralysis, because the brain is relieved by the expenditure of its nervous energy on the muscular system. I think we should generally look upon the occurrence of convulsions, in a case of cerebro-spinal irritation, in the light of an attempt at a crisis made by nature itself. What is a crisis? An organ labouring under irritation is suddenly relieved by a new process taking place, either in itself or in some other part; and when we come to examine what these modes of relief are, we find them to consist in the occurrence of supersecretion, hemorrhage, exanthematous eruptions on the surface, or convulsions. There is no doubt that, when we look to the results of the sudden supervention of a copious secretion in an inflammatory affection of any secreting organ, the source of relief is manifest. If we take two cases of hepatitis or bronchitis—one attended with copious secretion, the other without any secretion at all—it will be easy to conceive how much more dangerous the latter is, and how much more difficult to manage. Now, if we consider the brain in this point of view, we find that it is not a secreting organ, in the ordinary acceptation, and that the only mode in which it can relieve itself is by the expenditure of its excess of nervous energy on the muscular system, or by the same expenditure of mental energy, as in the case of high delirium. I think we might fairly draw an analogy between this mode of relief and that which, in other diseases, is the result of hemorrhage or secretion. One fact, at all events, appears

certain, that in two most remarkable cases of different diseases—each, however, characterised by the same phenomena in the early stage, namely, active determination to the head—we find that the case which turns out favourably is that in which convulsions occur (namely, epilepsy); while in apoplexy, where these symptoms are absent, we have either death or recovery with paralysis.

If this opinion be well grounded, it would militate strongly against the practice of checking the convulsions of meningitis by opiates. I feel convinced that this practice is wrong and dangerous; its effects may be as injurious as the arresting the reactions by astringents in a case of acute inflammation. There are two ways in which we can explain its bad effects. In the first place, opiates prove detrimental by checking the convulsions, which appear to be a mode of relief adopted by nature; and, next, they must do mischief from their well-known tendency to add to the existing cerebral congestion. I have now seen a good many cases of meningeal inflammation in which convulsions took place, and where opiates were employed to remove them, and feel compelled to state that the opium has certainly relieved the convulsions, but the patients have afterwards fallen into a state of profound coma, from which they never recovered. I have witnessed this so often, that I should not discharge my duty properly, did I not warn you against the employment of opium in arachnitis. The same rule most commonly holds good in cases of visceral inflammation, where an organ is in a state of irritation, and has its secretions suppressed. Here also opium, by arresting secretion and increasing congestion, will be productive of bad effects. I allude here particularly to the treatment of pneumonia by opium, as recommended by Dr. Armstrong, who lays great stress upon its use in full doses after having premised a single bleeding. I have had some experience of this mode of treatment, and find that the effect of the opium is not to remove, but to convert a manifest into a latent disease. I have seen the pain, dyspnoea, and cough subside, but the fever continued, and the destructive process of the lung went on as usual. This is the result of my experience.

I shall now make a few observations on the occurrence of delirium in disease of the brain. In one of my former lectures I alluded to the important fact, that, in the majority of cases of meningitis, where delirium was present, there was inflammation of the convexity of the brain. I stated also, that, when inflammation attacked the base of the brain, we might have it going through all its stages without delirium, and pointed out the importance of this in favour of the phrenological doctrines. Andral admits the occurrence of delirium in case of inflammation on the convexity of the brain, but his reasoning upon this subject appears to me to be inconclusive. He divides affections of the convexity of the brain into those which are characterised by delirium through their whole course, and those in which coma is the most remarkable feature; and seems to think that, where coma is the most remarkable symptom, the results of the case are unfavourable to phrenology. But we shall find, on

examining these cases, that, in many of them, where coma was the predominant feature, there had been delirium in the commencement. He gives the details of thirty-nine cases accompanied by delirium all through, in thirty-six of which there was disease of the convexity of the brain, either simple or complicated with arachnitis. As far, then, as his first set of cases go, they are in favour of the opinion that inflammation of the convexity of the brain is most commonly attended by delirium. It appears, also, that in those cases in which coma was the most remarkable symptom there was more or less delirium in the commencement; so that, whether we take the cases in which there was delirium all through, or those in which there was coma, the conclusions appear to be in favour of the doctrines of phrenology.

I shall now proceed to make some remarks on the phenomena of organic life in cases of cerebral inflammation. In the first place, with respect to the tongue, we find that in simple arachnitis it is but slightly affected; there may be some trifling degree of foulness, or it may be quite clean and moist. You will observe the value of this, as connected with the diagnosis of irritation of the brain from disease of the digestive system. There are many cases of irritation of the digestive system putting on the semblance of hydrocephalus to such a degree as even to mislead an experienced practitioner. Now, if it be true that in simple arachnitis the tongue remains clean, it furnishes us with very material information, as, under such circumstances, our attention will be directed to the true seat of disease. Andral says, that in some cases of arachnitis he has found the tongue red, or dry, or foul, but that at the same time there was disease of the digestive system. The majority of his cases, however, were simple, and exhibited no marks of an affection of the tongue or digestive system.

There is one more symptom on which I wish to offer a few observations, and that is the occurrence of vomiting in the hydrocephalus of children. In all cases where there is obstinate vomiting, particularly in children, you should have your suspicions roused, and look carefully to the state of the head. Vomiting is a symptom which occurs in many cases of arachnitis; in some it is slight, in others more constant, while in a third class it is harassing, incessant, and produced by swallowing the most unirritating substances. The nature of the fluid rejected from the stomach is various—being sometimes bilious, sometimes mucous, sometimes only consisting of what has been recently drunk. In some of these cases you will find the symptoms of incessant vomiting, unaccompanied by pain of the stomach, tenderness of the epigastrium, or any other sign of disease of the digestive system. I have even seen it co-existing with a good appetite. Many persons have been lost by such cases having been mistaken for disease of the digestive system, the practitioner being ignorant that vomiting was here only symptomatic of disease of the brain. No matter what the situation of the meningitis may be, it is now established that you may have vomiting as a common symptom. I recollect the case of a delicate

child, about seven years of age, who laboured for some time under catarrhal fever, on the subsidence of which she got an attack of vomiting, which came on at different times in the day, but without headache, delirium, or intolerance of light. This vomiting continued from day to day; and, at the end of a week, the pupils became suddenly dilated, and coma set in, under which she died. There is one very remarkable circumstance connected with this subject, with which I am anxious you should be acquainted. *Where this incessant vomiting is present, you will have the other symptoms of meningitis more or less latent.* This illustrates a law before alluded to, that, where the phenomena which are the result of sympathy with an affected organ are very prominent, those which characterise the disease of the organ itself are more or less latent. If we take the reverse of the former case, and consider a case of gastric disease, we know that the irritation of the stomach will produce violent cerebral symptoms, and that here also the same law is exemplified—for we shall have absence of pain, tenderness, and vomiting. The great value of this rule is, that a knowledge of it will put you on your guard, and that the mere absence of the peculiar symptoms of an affection of an organ possessing extensive sympathies, should not lead you to conclude that there was no disease of that organ. In some remarkable cases of gastritis, the principal symptoms observed were convulsions and delirium; there was no vomiting or thirst, very little pain on pressure, and nothing remarkable in the condition of the tongue. The same latency of inflammatory disease is frequently seen in cases of delirium tremens.

With respect to respiration and the state of the pulse in meningitis, there is very little to be said. You may have meningeal inflammation with every variety of pulse—strong, weak, full, rapid, slow, or intermittent. Generally speaking, the pulse is, towards the close of the disease, feeble and intermitting, but you may have the disease running through all its stages without any peculiarity in the character of the pulse. Respiration seems to be very little affected, and this would appear to favour the opinions of Sir Charles Bell. There is no doubt, at least, that the sympathy of the brain with the respiratory system is much weaker than with the digestive.

TREATMENT OF HYDROCEPHALUS.

I shall occupy your time but very briefly on the treatment of the hydrocephalus of children, as it appears to me to be a disease in which, of all others, the principles of treatment are most simple. The old idea of this affection was, that it was a species of dropsy, depending on the relaxed state of the cerebral vessels, and hence the term hydrocephalus. Modern pathology has shown that the occurrence of serous effusion is a mere accidental circumstance, as it is present in one case of arachnitis and absent in another. When it does occur, however, *it is the result of inflammatory disease*, and it is to the prevention and cure of this that the practitioner must direct his attention. With the symptoms of this disease I shall not take up your time, as you will find them sufficiently

detailed in books; but, with respect to treatment, I shall say that hydrocephalus is a disease *much more* under the influence of treatment than persons generally think. It is said that, when once effusion has taken place, the case is hopeless, and nothing can be done. This remark appears to me to be unnecessary, for there is no symptom from which you can venture to assert that *effusion* has set in. You may, from the inflammatory state of the brain, have delirium, coma, deafness, blindness, and paralysis, without any effusion of serum; and in many cases life has been saved, even after the appearance of all these symptoms.

This term *effusion* is one of the bugbears of medicine. Many patients are lost from the prevalence of false ideas connected with this subject; for, as soon as *effusion* is supposed to have set in, the efforts of the practitioner are given up. Hundreds of patients die of bronchitis and pneumonia, in whom life might be saved if the symptoms of *effusion* had been treated for those of inflammation; and so it is with respect to the brain. This effusion is not the disease—it is not even a constant result of the disease. We have no certain means of ascertaining its existence; and we know, that, by a persistence in antiphlogistic treatment, life may be often saved, even after all the supposed symptoms have occurred.

Take this with you as a rule in medicine: always to keep your eye more upon the causes than the effects of disease.

The treatment of hydrocephalus in the child should always be active, and conducted on the same principles as those of general encephalitis in the adult. Shaving the head, bleeding when practicable, *repeated leeching*, *cold affusion*, *calomel*, and *purgatives*—these are the great measures upon which we are to rely for success. It is satisfactory, too, to reflect, that many cases have been saved by the prompt and steady adoption of this simple mode of treatment.

OF INTERNAL REMEDIES.

The use of mercury seems to be that on which you should most rely. Some of the most singular recoveries have occurred after ptyalism has been produced. Let me remind you, however, that the rules connected with this mode of treatment, which I pointed out in speaking of hepatitis, apply equally in this case. There is a terrible consequence of mercurial action in the lymphatic temperament, with which you should be acquainted; I allude to a violent and destructive inflammation of the soft parts of the mouth and face, which has got the name of the *mercurial cancrum oris*. An œdematous inflammation of the cheeks, lips, and tongue, takes place, and, if not checked, rapidly runs on to extensive ulceration. I have seen one cheek, half of the nose, the lower eyelid, and the opposite angle of the mouth, utterly destroyed, in a case where but five grains of calomel were used. This drawing represents the disease, after a frightful perforation of the cheek. In this case the quantity used was nine grains. I have seen the disease from the use of so small a quantity as a grain and a half of calomel! These facts show that there is a state of the constitution in which a minute

dose of calomel may have terrible effects. The same, too, may arise from the external use of mercury. I recollect the case of a young woman in the Meath Hospital, whose head was rubbed with *one dram* only of mercurial ointment, for the purpose of destroying vermin. She was attacked, and with difficulty saved.

The disease may also come on suddenly in a patient who has been for some time using mercury in considerable doses; but this is the rarest case.

You recognise this disease by the sudden supervention of great swelling of the lips and cheeks, so as to completely alter the expression. The tongue is also swollen. All these parts are hot and tender to pressure. The breath is fetid, and the internal surface of the mouth excoriated, and often covered here and there with patches of lymph. At other times we have a circumscribed œdematous swelling, occupying the centre of the cheek, which runs on to ulceration; but most commonly the ulceration of the external parts begins at the depending angle of the mouth.

In a case of this kind, if you are called before ulceration has taken place, I believe you can often save your patient, and prevent destruction of the face. Treat the disease as a violent inflammation; use repeated leeching, poulticing, and the warm bath.

While you do this, you must keep up your patient's strength by light nourishment and wine. Apply to the internal ulcerations the mel æruginis, the nitrate of silver, or the chloride of soda. I have now saved many cases by bold and repeated leeching. I remember one case of a man in which ninety leeches were used; he recovered perfectly.

In the treatment of this affection, it is of the utmost consequence to attend to the position of the patient. By keeping him as much as possible upright, or by preventing him leaning constantly on one side, we do much to prevent the occurrence of the ulceration of the angle of the mouth.

As far as I can see, hydrocephalus, when taken in time, is a very manageable disease; and there is only one case in which it is difficult to treat, and that is where the cerebral affection is accompanied by symptoms of gastro-enteric disease. In several cases of hydrocephalus, this complication certainly exists; and you have first symptoms of disease of the digestive tube, and then of the head. Such cases as these are involved in great difficulty, and in their treatment you run the hazard of falling into a twofold mistake. The first is your acting on the supposition that the disease of the head is only sympathetic, and that it will subside as soon as the abdominal symptoms are removed; the other is occupying your attention exclusively with the head. Now, there is one rule with respect to this, which I think will serve to guide you through many difficulties, *and this is, never to neglect the head.* Though you have first an affection of the digestive system, and then of the head, it is better (even though the symptoms of the latter still continue) to pay attention to the head. You can do this at the same time that you are attentive to the condition of the digestive organs.

Another rule is, that the cases of disease in which the purgative plan does not answer are generally those in which there is primary inflammation of the digestive tube. Dr. Cheyne, in speaking of the treatment of hydrocephalus, says, that some cases are benefited by purgatives, others not; and that the latter are those in which there is disease of the intestinal canal. In such cases you will not irritate the bowels, or add to the existing inflammation by purgatives. Let the bowels be kept open by enemata, and direct your attention immediately to the head. Children with largely developed heads, and of a strumous diathesis, are very subject to this disease; and I feel convinced that the present rage for the early mental education of children has a strong tendency to produce it in subjects of this description. I believe there are many cases of fatal hydrocephalus from which the poor victims would have escaped, but for the pernicious efforts of the parents to make them literary prodigies. I have observed many cases of this kind among the children of persons who, having been originally situated in an humble sphere, and deprived of the benefits of education, accumulate wealth; and then, feeling in their new condition the want of education, are anxious to communicate it to their offspring; and, with that view, have them educated with too much care, and from too early a period. The child is constantly kept at his books—his little mind is perpetually tasked—a degree of cerebral excitement is kept up—and, while he is delighting his gratified parents with the manifestations of a precocious intellect, his health is neglected, and the seeds of disease are insensibly sown. One of the most ordinary consequences of this early application of the mental powers is hydrocephalus. These little creatures, too, have a congenital disposition to disease of the brain, for they have generally large heads. Such cases are examples of the results of an arrest of development. A relative condition of head exists similar to that which occurs during foetal life, and this is always accompanied by a remarkable susceptibility to inflammation. This peculiar development of head also produces a precocious state of intellect, which is increased by the pernicious habit of obliging children to study at too early an age. Where you meet with children suffering under these circumstances, you will not discharge your duty properly if you do not point out to the parents the mischievous tendency of their conduct. In such cases as these it may be justly said that ignorance is bliss.

LECTURE XXVIII.

Apoplexy—Cerebritis and meningitis—Definition of apoplexy—Simple or nervous apoplexy without disorganisation—Complicated with other diseases—Congestive or serous apoplexy—Dr. Abercrombie's opinions—Apoplexy with extravasation—Sites of extravasation—Absorption of clot—Apoplexy in children.

We were occupied at our last meeting in considering some of the most prominent symptoms of meningeal inflammation; and I beg of you to recollect, that all these symptoms, with the exception of pain, are those which ordinarily characterise inflammation of the substance of the brain itself, and are to be explained by referring them to some lesion in the functions of that organ. It appears, then, that the symptoms of meningitis, with the exception of pain, are symptoms of an affection of the brain itself; and this is a point which you must always bear in mind, when you agitate the question as to the possibility of making a diagnosis between meningitis and encephalitis. We have a set of symptoms characterising meningeal inflammation, the majority of which belong to irritation of the brain itself; and we find that these may exist with or without any *perceptible* alteration in the cerebral substance. Now, in cases where you suppose the existence of meningeal inflammation, and find these symptoms present, it would be venturing too much to assert that there was no complication with organic disease of the brain; and, therefore, we must conclude that, in most cases, it is nearly impossible to distinguish between inflammation of the substance of the brain and of its membranes.

In speaking of the more important symptoms of cerebral inflammation, I alluded particularly to convulsions, and stated, that, as far as my observations went, this symptom, formidable as it may appear, is not in reality so unfavourable as it is generally thought to be. In fact, there are many cases of affections of the brain, accompanied by convulsions, in which the danger is by no means so great as in others of a different description; and many of the worst cases are those in which convulsions are absent, or only trifling. I think we may look upon convulsions as being more or less a source of relief to the brain, when labouring under the excitement of irritation or inflammatory disease. You are all aware, that one of the great functions of the brain is to regulate and control the motions of the muscular system. If a man exercises his limbs violently for some time, he becomes tired and exhausted; he cannot pursue the same exercise any longer, for, in addition to whatever the muscular system may suffer, there has been a great expenditure of nervous energy; and if he should attempt to keep up the same exertions, such a degree of muscular and nervous debility is superinduced that syncope is the consequence. Now, the expenditure of energy produced by the supply of nervous power to the muscles, seems to bear a strong analogy to the secretory discharges from other viscera. In the case of irritation or inflammatory affections of other organs, you are all aware that there is

nothing which gives such speedy and effectual relief as supersecretion, or an increased action of the secreting vessels of the affected organ. Now, if we look upon the expenditure of nervous energy in the same light (and I see no reason why we should not), we can easily conceive why it is that convulsions relieve the irritation of an over-excited brain. I drew your attention strongly, at my last lecture, to the curious and important fact, that if we compare apoplexy and epilepsy, with respect to the danger and the chance of disorganisation attendant on each, we shall find the danger is infinitely greater, and the chances of organic change more numerous, in the former than in the latter. In epilepsy, where the convulsions are violent, we seldom have a fatal termination of the fit, and there is rarely lesion of the substance of the brain, until the disease has lasted for a great length of time. This is not the case in apoplexy. Here, as I have already stated, we have two cases of active determination to the head: in one case there are no convulsions, and we frequently find the result to be death, or extravasation with paralysis and slow convalescence; in the other, we have violent convulsions, followed by rapid recovery and no disorganisation. From this, it would seem reasonable to conclude, that convulsions are a mode of relieving the brain, adopted by nature, and that their occurrence in hydrocephalus should not be looked upon as unfavourable. Now, if this be true, it must strike you, that nothing can be more dangerous and improper than to take any steps to control an attack of convulsion during the prevalence of hydrocephalic symptoms. The true mode of treating them is to adopt measures calculated to relieve irritation of the brain, and not hazard the patient's safety by following the ordinary but mischievous mode of attempting to control the salutary efforts of nature. I allude here particularly to the practice of administering opiates and antispasmodics, a practice which I firmly believe to be fraught with danger.

We have to-day to consider another form of cerebral disease, scarcely less important than those with which we have been hitherto engaged. In all the former instances, we find the determination of blood to the brain followed by that organic change which we term inflammation. But we may have accumulations of blood in the brain, unaccompanied by inflammation, and this brings us to the consideration of apoplectic disease. The term apoplexy, as I suppose you all know, is derived from a Greek word, signifying a stroke or blow. It is a term which, in the present state of medicine, has been very frequently abused, or at least employed in very different senses, and hence the many erroneous opinions respecting it. The true meaning of the term expresses an alteration of the phenomena of the life of relation, that is, of the functions of the cerebro-spinal system. In taking a view of the nature of this alteration, we find that the attack generally comes on in a sudden manner, and that the functions of the brain are partially or completely suspended. You are aware that the manifest phenomena of the life of relation are those which belong to *sensation, muscular motion, and the intellect*; and that the system of

the life of relation is composed of the brain, spinal cord, and nerves. Now suppose, for example, that a man gets an attack of apoplexy, we find him paralytic—here is a lesion of the muscular function. We find him insensible to external stimulants, he feels no pain—here is a lesion of sensation. We may find his sight, hearing, taste, smell, and touch, are injured; he lies in a state of insensibility, and is unconscious of every thing passing around him—here we have an example of interruption in the performance of the intellectual functions. All these phenomena exhibit the various lesions superinduced by an attack of apoplexy, in the functions of those organs which subserve to the life of relation.

I have said that the term apoplexy is frequently abused in modern medicine. From the circumstance of most cases being accompanied by an effusion of blood on the surface, or into the substance of the brain, the term has been also applied to sanguineous effusions into other organs, and we hear every day of pulmonary and hepatic apoplexy; terms implying the extravasation of blood into the substance of the lung or liver. The analogy, however, in such cases, will on examination be found to be coarse, and the application of the term loose and improper. Apoplexy, as a cerebral disease, may occur with or without effusion; in either case, the disease, *quoad* the lesion of function, is the same; but to give the name of apoplexy to hemorrhage into the lungs or liver, is improper. The term apoplexy should be used only with reference to the brain, and applied to a particular train of lesions in the functions of the life of relation, occurring *with or without an effusion of blood, or even congestion*. When we have effusions of blood into other viscera, we may have them unaccompanied by any apparent lesion in the functions of the organ affected (a circumstance rarely met with in the case of the brain); and it would be much better to give some other name to those hemorrhages into the substance of the liver and lungs, than to designate them by one drawn from a loose and imperfect analogy.

The suspension of the phenomena of the life of relation, complete or partial, which constitutes apoplexy, may be connected with any of the following pathological conditions. First, great congestion of the brain, in which the vascular system of that organ is overloaded, but without extravasation of blood or serum; this is termed *the congestive apoplexy*. In the next place, we may have this congested state of the vessels of the brain with an extravasation of blood on its surface. To the latter form, the *meningeal apoplexy* has been applied. Thirdly, with an effusion of blood into the substance of the brain, which is the most common case, and, lastly, we may have complete apoplexy without morbid appearance, or, if there be such, *quite insufficient to account for the phenomena*. A man will fall down suddenly, he will lie in a state of insensibility, with stertorous breathing, coma, and paralysis, he will die with all the symptoms of the worst form of apoplexy, and yet, on dissection, the brain may be found, *to all appearance, healthy*. This is what has been termed, by the older authors, the nervous or convulsive

apoplexy; of the real nature of which we are still as ignorant as we are of the real nature of tetanus, hydrophobia, and other nervous diseases unaccompanied by perceptible organic change.

This is the *simple apoplexy* of Dr. Abercrombie, of which he gives several most important cases, and refers to others related by the older authors. You will at once admit that it is not more extraordinary that apoplexy should exist without perceptible organic change, than mania, tetanus, hydrophobia, and other affections. Of the fact there is no doubt. Such cases, indeed, are rare; which, in one sense, may be looked on as a fortunate circumstance. But in the progress of other diseases, this nervous coma, or apoplexy, is by no means uncommon. Thus there is no symptom more common than coma in typhus; and yet, if you examine the head after death, you generally either find no lesion at all, or such as will not be sufficient to account for the symptoms. The coma, which occurs in cases of painters' colic, too, appears to be closely connected with this nervous apoplexy. You will recollect an interesting clinical experiment I made in the case of a patient with painters' colic, who had profound coma. In this case, I thought it probable that the condition of the brain bore no resemblance to sanguineous apoplexy, because the symptoms of painters' colic are seldom or never accompanied by hyperæmia of the nervous or other systems. Under this impression, I prescribed a full opiate, and this not only did not increase the coma, but, on the contrary, produced the very best effect, for the patient was amazingly improved the next morning. I do not so much mean to say, that opium is useful in nervous coma, as that, in this instance at least, the coma was not of the congestive kind. It is not unlikely, too, that the coma of jaundice is of the same description, and unconnected with any decided hyperæmia of the brain. I am aware that in jaundice the coma is supposed by some to depend upon a bilious condition of the blood circulating in the brain; but there are so many cases of persons who have laboured under jaundice for years without having coma, that we must seek for some other explanation. Now, so far as we know of the encephalon in persons who have died of jaundice, it appears that little or no congestion exists; and hence it seems probable that the coma of jaundice is similar to that of nervous apoplexy.

I shall now proceed to the consideration of those forms of apoplexy which are connected with changes more or less apparent in the circulation of the head, and with which we are, consequently, better acquainted. I have told you that simple congestion of the brain may be accompanied by symptoms of apoplexy, or that we may have the disease presenting, in addition to this, an effusion of blood into the substance, or on the surface of the brain. The simplest idea you can get of the condition of the brain in the congestive form, is to consider what its state is in persons who have been hanged. These persons have the vessels of the brain loaded with blood from the violent interruption of the venous circulation. Now, this increase in the quantity of blood circulating in the brain,

may arise from two causes, one depending on the interruption of the venous circulation, the other produced by an increased action of the arterial system. Hence in certain cases of disease of the heart, where the blood is sent with great force to the head, there is a strong predisposition to apoplectic attacks. The kind of disease of the heart, however, which has been found most liable to produce this, is not, as you would suppose, Corvisart's active aneurism, but simple hypertrophy of the heart, where the cavity of the left ventricle continuing the same, its walls are increased in thickness and strength, so that, on the natural quantity of fluid, an increased impulse is exercised. Such, at least, is the result of Andral's researches, and there is every reason to place confidence in the accuracy of his conclusion.

About this congestive apoplexy there appears to have been a good deal of misapprehension. You have all heard of *the serous apoplexy*. In this form, it has been supposed that the cause of the compression of the brain, and all the other symptoms, is an effusion of serum, just as an effusion of serum into the cavity of the pleura will produce compression of the lung and dyspnoea. The idea which has been generally entertained is, that the effusion of serum is the cause of *all* the symptoms; and, in consequence, the same active treatment has not been adopted as in the other forms of apoplexy. This opinion will be best refuted by the investigations of Dr. Abercrombie, and I cannot do better than read for you the opinions of this eminent writer on the subject, as given in his celebrated and admirable work, which, I have no hesitation in saying, constitutes one of the brightest ornaments of British medicine.

"This distinction, which has been proposed between sanguineous and serous apoplexy, is not supported by observation. The former is said to be distinguished by flushing of the countenance and strong pulse, and by occurring to persons in the vigour of life; the latter by paleness of the countenance and weakness of the pulse, and by affecting the aged and the infirm; and much importance has been attached to this distinction, upon the ground that the practice which is proper and unnecessary in the one case, would be improper or injurious in the other. I submit that this distinction is not founded upon observation, for, in point of fact, it will be found that many of the cases which terminate by serous effusion, exhibit in their early stages all the symptoms which have been assigned to the sanguineous apoplexy; while many of the cases, which are accompanied by paleness of the countenance and feebleness of the pulse, will be found to be purely sanguineous; and one modification of the disease in particular will be described, in which these symptoms are very strikingly exhibited, while the disease is found to be sanguineous apoplexy in its most hopeless form.

"Portal has described a series of cases which afford the same result; of three, which presented all the symptoms of serous apoplexy, one was saved by repeated bleeding, and in the other two, which were fatal, there was found extensive extravasation of blood. Case XCVI., lately described, forms a remarkable addition to these

observations. If any case could be confidently considered as serous apoplexy, this was such. Dropsical effusion had existed in the body for months, and, in defiance of every remedy, it had been progressively gaining ground. There were symptoms indicating its existence, both in the thorax and in the abdomen; the patient then became comatose, with pale countenance, and died; but though dropsy was found in other cavities, none could be detected in the brain.

"In other parts of the body serous effusion is very seldom a primary disease; it arises as a result either of inflammatory action, or of impeded circulation, and takes place slowly, not accumulating at once in such quantity as to induce urgent symptoms. It is, therefore, in the highest degree improbable, that it should occur in the brain as a primary disease, and accumulate with such rapidity as to produce the symptoms of an apoplectic attack.

"The quantity of fluid effused, bears no proportion to the degree of the apoplectic symptoms. We find it in small quantity, though the apoplectic symptoms had been strongly marked and long-continued; we find it in large quantity when the symptoms have been slight; and, finally, we find most extensive effusion in the brain where there have been no apoplectic symptoms at all. The direct inference from these facts is, that, in the cases of apoplexy with effusion, the presence of the fluid cannot be considered as the cause of the apoplectic symptoms."

The same error has been committed with respect to hydrothorax, a disease almost never primary, but the result of either pleuritic inflammation, obstruction of the heart or lungs, or some analogous cause. The cause of the symptoms is not the mere effusion of fluid, but some pre-existing disease which has given rise to a serous effusion. In Dr. Abercrombie's work, you will find the remarkable fact stated, that there may be a copious effusion of serum in the head, without producing apoplectic symptoms. The following case, mentioned by Dr. Abercrombie, furnishes a remarkable illustration:—A patient, who had laboured under hypochondriasis for upwards of thirty years, began to decline rapidly in health. He was extremely feeble, his bowels costive, his sleep disturbed, and his appetite gone. This state continued for some time, and he began to sink, but he never complained of headache, giddiness, convulsions, or paralysis, and his mental powers remained unimpaired until a very short time before his death. Yet, on opening the head, there was an exceedingly copious effusion of serum found under the arachnoid; and in some places this was so great as to give the arachnoid the appearance of small bladders filled with water. The ventricles were distended with fluid. Dr. Abercrombie gives another case, where the quantity amounted to eight ounces, and notices a case, mentioned by Dr. Marshall, of a maniac who died of mortification of the feet; a few hours before death he became perfectly rational, yet effusion was found both on the surface of the brain and in the ventricles, amounting to more than a pound.

All these facts go to prove, that what has been termed serous

apoplexy is only an apoplectic attack depending on congestion of the brain; that in some cases we may have this congestion accompanied by serous effusion, in others not; that the effusion is secondary, and by no means of constant occurrence; and that altering our practice, and pursuing a less active plan of treatment, in such cases, would be improper. The same treatment should be adopted in the serous, as in the congestive form of the disease, for where the nature of the affection is the same, the same curative means should be employed. Why it is that effusion takes place in one case, and not in another, we cannot tell; such changes are connected with laws of organisation, of which we are at present ignorant. We know as little why this should occur as why inflammation of the liver in one case is followed by enlargement, in another by the secretion of pus, in a third by cancer, or in a fourth by hydatids.

We now come to the consideration of apoplexy with extravasation of blood. This is the form of the disease to which the term apoplexy has been restricted by one of the last writers on the subject, M. Rochoux. In this affection, the extravasation of blood, which constitutes the principal pathological feature of the disease, is found to exhibit a remarkable variety as to its seat and extent. In some cases, the blood is effused on the surface of the brain; in others, into its substance; and in a few cases into the ventricles. De Haen gives some cases of apoplexy produced by rupture of the choroid plexus; but, in the great majority of cases, where blood is found in the ventricles, the extravasation has taken place in one hemisphere, and, tearing through the substance of the brain, has made its way into their cavities. Of the three varieties of apoplectic effusions, the ventricular is the rarest; the next to this is the meningeal, or that in which blood is poured out on the surface of the brain, and the most common is where it is effused into the substance. It has been also found that certain parts of the brain are much more liable to sanguineous effusions than others; of the reason of this, as of many other phenomena connected with the circulation of the brain, we are still in ignorance. The following table, which you should bear in mind, exhibits a remarkable preponderance in the liability to sanguineous effusions of certain parts of the brain. It has been taken from the "Précis d'Anatomie Pathologique" of Andral. The following is a summary of the results of 386 cases of apoplexy.

In 202 cases, the effusion took place into the substance of the hemisphere of the brain, in that part which is on a level with the corpora striata and optic thalami. The portion of the brain next most liable to effusions, are the corpora striata; and here we have 61 cases. Next to this are the optic thalami, in which we have 35 cases. In that portion of the hemispheres above the centrum ovale, 27 cases. Lateral lobes of the cerebellum, a proportion of 16 cases. In those portions of the brain anterior to the corpus striatum, 10 cases. In the mesocephalon, 9. Spinal cord, 8. Posterior lobes of the brain, 7. Middle lobe of the cerebellum, 5. Peduncles of the brain, 3. Olivary bodies, peduncles of the cerebellum, and

pituitary gland, 1 in each, making 3—total 386. Out of these, we find 325 cases occurring in the hemispheres of the brain, corpus striatum, and optic thalamus.

In the number and size of these effusions we find the greatest varieties. In some cases, an enormous effusion takes place, and many ounces are extravasated into the substance of the brain; in others, the quantity is trifling, being sometimes as small as a pea, or even less. It has been observed that in cases where numerous extravasations were discovered, they were generally found to be in different states, as if they had occurred at intervals, and not simultaneously. This leads us to the knowledge of one of the most important facts in pathology, that in many cases of apoplexy, after a clot has been formed, nature commences, at an early period, a process of cure. The change, which takes place in cases where a patient recovers, seems to be the following:—It becomes, at first, somewhat gelatinous; it is next observed to be more consistent, and it loses its red colour, and takes on a whitish or yellow appearance. The clot is gradually removed; and along with the absorption of the clot there is a process of isolation going on. A fine membranous cyst, furnished with vessels, is formed round the clot. In some cases, the clot is replaced by a quantity of serous or gelatinous fluid; but in the majority of instances this does not occur, and the cyst has been found empty. This is a fact which has been established by numerous observations.

There is the greatest possible difference as to the period at which the absorption of the clot is completed; but we may safely assert, from the number of cases in which, after paralysis, a recovery takes place, that this process is of very common occurrence. In several cases, where apoplexy, followed by paralysis, has happened several times during the lifetime of the patient, a number of those cysts, corresponding with the number of attacks, and presenting various appearances according to the date of their formation, have been found. It appears, then, that the cure of apoplexy depends solely on the absorption of the clot; and that, as long as this remains unabsorbed, the patient is in danger. In some cases, absorption does not take place at all, the clot becomes organised; and in this way it is supposed that some of the tumours found in the brain are formed. There are several circumstances which favour the absorption of the clot, but nothing so powerfully as a healthy condition of the whole cerebral circulation. This leads us to the consideration of the importance of paying attention to the head, long after an attack of apoplexy. It inculcates the necessity of avoiding every thing calculated to add to the existing congestion; and shows that, in the paralytic or after-stage of an apoplectic attack, we should not neglect to deplete the head from time to time. The great point is to keep the head perfectly free from irritation; for it has been found, that, where a cure appeared to be going on, any new irritation applied to the brain has had the effect of arresting the absorption of the clot, and marring the process of cure.

I regret I cannot dwell longer on this subject, as I wish to con-

clude the pathology of apoplexy to-day. There are, however, two more observations to be made before I close the subject. The source of an apoplectic effusion is very hard to be discovered; it appears generally to come from a number of minute vessels, for we are seldom or never able to trace it to the rupture of a vessel of any size. The age at which persons are most subject to apoplexy, appears to be from fifty to seventy. You should, however, be aware that apoplexy with sanguineous effusion is by no means uncommon, even in persons of a tender age. Billard details an instance of this in a child, soon after birth. There are also several cases mentioned as occurring in children during the first three or four years. Andral gives the case of a boy of nine years of age, who died of apoplexy, with a vast effusion of blood. One of the most remarkable cases of this kind I ever witnessed, occurred in a child who had been just weaned. This child had been labouring for some time under symptoms resembling incipient hydrocephalus, and then suddenly got an attack of convulsions, followed by coma and paralysis of one side. From a careful study of the symptoms, I ventured to make the diagnosis of apoplectic effusion, and on examining the brain, after death, there were nearly three ounces of blood found effused in the base of the brain.

LECTURE XXIX.

Apoplectic effusions—Curative process adopted by nature—Connection of temperaments with disposition to apoplexy—Researches of Rochoux—Periods of life most subject to apoplexy—Principles of diagnosis—Varieties of apoplexy—Connection of symptoms with pathological appearances—Rostan's division of—Different symptoms of—Double effusions—Rupture into ventricles—Hemiplegia—Value of the suddenness of paralysis as a diagnostic examined—Symptoms of apoplectic effusions.

At my last lecture, I spoke of the nature of apoplectic effusions; I stated that they exhibited a considerable variety as to their situation, extent, number, and condition in different cases; that it was frequently a matter of great difficulty to ascertain their source, and that they might occur at any age, but chiefly from that of fifty to seventy. I gave a brief sketch of the process adopted by nature in effecting a cure, and showed that in many cases, where the effused blood is absorbed, there is scarcely any trace of the disease, except a slight cicatrix; but that in some instances, where the sanguineous effusion has been removed, its place becomes occupied by a quantity of serous fluid, and this, with the cyst which contains it, seems to explain what the old anatomists termed *false ventricles*. You will find, by looking over some of the earlier writers on anatomy, that they have described the brain as containing more than the ordinary number of ventricles, and the mistake seems to have arisen from their taking for ventricles those serous cysts or cavities which

remained after the absorption of an apoplectic effusion. Of course other causes, such as congenital formation, may give rise to the appearance. You will see in the museum of the College of Surgeons a fine specimen of abnormal cavities filled with serum in different parts of the brain.

In speaking of the period of life at which apoplectic disease is most frequent, I stated, that though it might occur at any time of life, still there was a particular period at which there is a greater liability than any other. Rochoux has shown that the tendency to apoplexy is greatest towards sixty, and diminishes towards seventy years of age. The number of cases which occur between sixty and seventy are very great, when compared with those between seventy and eighty; and after eighty he considers the liability to be still farther diminished. It seems strange, that persons after seventy should not be so liable to attacks of apoplexy as before that period, but such is the fact. It has been thought that this may be explained by the anæmic state of the brain in old persons; it is said, that at such an advanced age general emaciation takes place, and the quantity of blood is greatly diminished. This explanation, however, is doubtful, because it is at present well ascertained, that persons of ordinary development, who are nether fat nor thin, and also persons of spare and delicate habit, are as much, and even more, liable to apoplexy than the fat and plethoric. It has been ascertained by careful investigations, that a high degree of plethora does not necessarily predispose to the disease, and that it is oftener met with in persons not of a plethoric habit than in those who are. These considerations throw some doubt on the opinion that an exemption from apoplectic attacks is connected with an anæmic condition of the system. It generally happens, however, that at this advanced period of life, from the general debility of the system, and the incapacity for active exertion, a man ceases to employ his thoughts about business, and there is little exercise for the intellectual functions. We now have finished the task; the brain reposes from the turmoil of active and incessant thought; there is a comparative absence of mental exertion, and this may in some degree account for the rarity of apoplexy after the age of seventy.

With respect to the different temperaments as bearing on this point, Rochoux shows that in Paris, at least, there was a nearly equal frequency of the disease in individuals of the sanguine, sanguineo-bilious, and sanguineo-lymphatic constitutions. The bilious temperaments, however, are much less liable. Such is the result of the observations in Paris; but it must be recollected, as Rochoux observes, that in that city the bilious temperament is the rarest. With respect to the sanguine or plethoric, it has been found that this temperament does not predispose to apoplexy so much as has been generally supposed. The disease has been observed to be most common in persons of ordinary development, next to those in persons of thin, spare habit, *and last of all in the plethoric and fat.* Rochoux's researches lead him to conclude that the number of persons of ordinary development, attacked by apoplexy, is three times

that of the plethoric, and that that of the spare habits is little more than twice as great as that of the fat and plethoric. If these researches are correct, they afford great consolation to stout gentlemen.

The conclusion, which has been come to, with respect to temperaments as bearing on the liability to apoplexy, appears to be true, namely, that there is no sign appreciable by the senses which will unequivocally point out a predisposition to apoplexy. This is of great importance in a practical point of view. You may expect the disease in the fair or dark haired, the thin or fat, alike. The frequent occurrence of this disease in persons who were never suspected to have any predisposition to it is another proof in favour of this opinion. With respect to the mere medical diagnosis of apoplectic effusion, it would be well if, in making it, you would always bear in mind the anatomical characters of the disease. Extravasation of blood into the substance of the brain generally takes place by a tearing or separating of the cerebral tissue. A quantity of blood is rapidly effused, the substance of the brain torn, and a cavity formed. There can be no doubt that the tissue of the brain is torn, for we can see the loose shreds hanging on each side of the cavity, and mixed up with the clot. Now, what are the principles which should guide us in making our diagnosis? They are exactly the same as those in other diseases *connected with a sudden solution of continuity in the substance of internal organs*. We have, with or without any preceding symptoms of a different kind, the *sudden* supervention of new and remarkable phenomena. The phenomena which are the result of disease proceeding in its ordinary course are gradual and progressive; but occurrences of this kind are almost always characterised by sudden and well-defined symptoms. Thus, we make the diagnosis of the rupture of an aneurism of the aorta from the sudden vomiting or expectoration of blood, followed by the death of the patient. Here, you perceive, the diagnosis is founded on the *sudden* supervention of new symptoms. In the same way we make the diagnosis of pneumothorax with a fistulous opening communicating with the bronchial tubes, and calculate, from the sudden occurrence of pain in the side and the other signs of pneumothorax, that there has been a solution of continuity in the pleura. Again; if a person labouring under hepatic abscess is seized with a fit of coughing, and suddenly expectorates a quantity of pus, and that this is found to be accompanied by a subsidence of the tumour in the region of the liver, we make the diagnosis of perforation of the diaphragm and pleura, and the escape of the contents of the abscess into the substance of the lung. Or he may, under the same circumstances, be seized with sudden and rapid peritonitis, and here we make the diagnosis of an effusion into the peritoneum. It is on precisely the same principles that Louis has established the diagnosis of perforation of the small intestines in cases of gastro-enteritis. The patient is lying in bed, perhaps apparently improving; he is not exposed to any exciting cause, and every care may have been taken of him. *On a sudden*

he exhibits symptoms of intense peritonitis, and rapidly dies. Any one conversant with such cases can easily make a correct diagnosis. On the same principles we found the diagnosis of apoplectic effusion. Almost all the instances of disease which I have given occur with a *sudden* violent invasion; and the same thing may be said of apoplexy with extravasation. It is true, that there are some cases which do not exhibit this character, but the general rule is suddenness of attack.

We may divide apoplectic attacks accompanied by extravasation into three great classes; and, if you look to the great majority of cases of this disease, you will find that, although they appear to pass by insensible degrees into one another, still, when taken and examined singly, there will be found a difference between them. This classification is that of Rostan, and I have known his principles verified in many instances. In the first class of cases, which are the worst and generally prove fatal, the extravasation is enormous. A person, apparently in perfect health, will fall down in a fit of apoplexy, remain for a short time insensible and paralytic, and then die. In such a case as this, the ordinary pathological character is an enormous effusion of blood, or excessive congestion. In a case of the second class, we have an apoplectic seizure with coma, which disappears after some time, and the patient recovers his intelligence, *but with paralysis of one side*. The pathological character of this form is, that the effusion is more limited and exists only on one side of the brain. Neither is the congestion so severe, and the patient recovers from the coma. In the third form, we have an attack of apoplexy of a milder description; there is scarcely any coma or loss of intelligence, and the paralysis is slight, generally affecting the muscles of one side of the face or of one of the extremities. Let us repeat these varieties. In the first, which constitutes the *apoplexie foudroyante* of the French, there is an enormous extravasation of blood in both sides of the brain; or, if it be only on one side, the amount of the effusion is frequently such as to burst through the walls of the ventricles and get into their cavities, and in this way we may have an effusion of one side getting into the other hemisphere, or exercising such pressure on it as may give rise to *general* symptoms. Such a case as this is, I believe, generally fatal; its progress, too, is very rapid, several persons under such circumstances having died in the space of an hour or less. In the second form, there is coma and loss of intelligence, and the patient recovers *with paralysis of one side*. Here the extravasation is never so great as in the foregoing case; the effused blood is confined to one side, and does not get into the ventricles. In the third form, the effusion is very much circumscribed, the signs of general congestion or extravasation are slight, the quantity of blood poured out is not, perhaps, larger than a nut, it is followed by partial paralysis, and there is little or no coma or loss of intelligence.

Let us take a brief review of the symptoms which attend each of these forms. In a case of the first description, we find a person,

hitherto in the enjoyment of health, suddenly attacked with symptoms of intense apoplexy. You will recollect that in my last lecture, I told you that apoplexy consisted in various lesions of the phenomena of the life of relation. In the most violent form of apoplexy, many authors are of opinion that there is a total paralysis in the functions of animal life. The patient falls down and remains in a state of complete insensibility, the eye no longer obeys the stimulus of light, no sound makes any impression on the ear, or odour on the sense of smelling, the sense of taste is destroyed, the skin may now be seared with a red hot iron without the slightest indication of suffering; in fact, sensation, one of the great phenomena of animal life, appears to be annihilated. If we examine further, we find that there is a total suspension of the intellectual functions, and that the patient is unconscious of any thing passing around him. If we go to the muscular system, we find that all that part of it which subserves to the purposes of animal life is completely paralysed. The neck, trunk and extremities have lost their power; and, if you raise the head, trunk or one of the limbs, they fall down like dead masses, as soon as the support is withdrawn. In some cases there is a certain degree of rigidity in the muscular system, in others not. We may observe also, that from the paralysis of the buccinators, the cheeks are alternately puffed out and sucked in during respiration. As far as my experience goes, I believe that this symptom is fatal. Here, then, we see that the great phenomena of the life of relation are suspended. The functions of organic life, however, still continue to be performed, the heart beats, respiration goes on, and the power of secretion remains; but, after some time, the functions of organic life are also suspended, and the patient dies. In some of these cases, we observe evident signs of determination of blood to the head, the face is swollen, and the lips livid; there is considerable turgescence of the vessels of the neck, with heat of the head, the skin hot, and the pulse full and strong. In other cases, however, we have a feeble pulse and a cold collapsed state of the surface.

Let us now turn for a moment to the pathology of this form of the disease. I have already mentioned, that the extravasation sometimes occupies both hemispheres of the brain, or that it occurs on one side, and, by tearing through the substance of the brain, gets into the ventricles, and produces symptoms referable to a lesion of both sides. With respect to the simultaneous double effusion, the following is a short notice of some cases taken from the "*Clinique Médicale*" of M. Andral. A man, about thirty-seven years of age, fell down near La Charité in a fit of apoplexy. He was immediately brought into the hospital, had prompt and careful attention paid to him, but without any effect; he lay in a state of profound coma, with complete suspension of the phenomena of animal life, and died in an hour and a half. On examination there was a double effusion of blood found in the brain, but it had not got into the ventricles. In another case, marked by simple intensity, there was an enormous effusion discovered in the substance of one

hemisphere, which burst into the ventricle, tore through the septum lucidum, and passed into the ventricle of the opposite side. In the next case, no distinct trace of optic thalamus or corpus striatum could be seen, their substance being completely broken up and destroyed by the effusion. I have told you that, after a rupture of the substance of the brain and the escape of the effused blood into the ventricles, persons have not recovered, but it is a fact, and a consolatory one indeed, that a person may recover from a *simultaneous double effusion*. A case in proof of this is given by Andral. A female who had been for some time a patient at La Charité, died of cancer of the stomach. The history of her case was, that nine years before she had an attack of apoplexy, she had fallen down in a state of insensibility, and remained comatose for a considerable time, that this was followed by *paralysis of both sides of the body*, which continued for two years, after which she gradually recovered the use of her limbs. In this case, two serous cysts, such as are met with in cases where patients have recovered from apoplectic attacks, were found, one in each hemisphere of the brain. In another case, the subject of which died of visceral disease, the patient had twenty-two years before an attack of apoplexy with double paralysis, and recovered with the loss of the use of one side; here there were two cysts also found. It appears, then, that though extravasation, with rupture of the walls of the ventricles, and escape of blood into their cavities, always proves fatal, a recovery may take place after a simultaneous double effusion.

Let us now enquire briefly, whether an apoplectic attack, followed by paralysis of both sides of the body, gives sufficient grounds to enable us to make the diagnosis of either of these accidents. Does it follow, if a person has an attack of apoplexy, succeeded by paralysis of both sides, that the effused blood has burst into the ventricles, or that a simultaneous double effusion has occurred? Andral inclines to this opinion as far as I can collect. Dr. Abercrombie appears to differ from him, and gives cases in illustration of his opinions. The following is one:—A private of the 10th Hussars had been complaining for some time of a pain in the head, for which he was blistered, and the pain soon went off. On the 22d of July, 1819, he was seized with giddiness and fell down; on being raised, he vomited, and complained of violent headache and faintness, but was quite sensible. He was very pale, and his pulse slow and languid. He was brought into the hospital, where he asked for some cold water, made a few inspirations, and expired. From the moment of his last seizure he had been paralytic of both extremities. Here we have an attack resembling the first form of apoplexy, so far as complete loss of power in the upper and lower extremities is concerned, but observe, the *patient was not comatose, and retained his faculties to the last*. On examination there was nothing found amiss with the brain, but, on removing the cerebellum, a coagulum to the amount of about two ounces was found under and surrounding the foramen magnum. Here the paralysis appears to have been produced by the pressure of the effused blood

on the upper part of the spinal cord. This case is an interesting one. It appears that the injury done to the functions of the life of relation was partial, there was a lesion of the muscular function, but there was no coma, and the intellectual faculties were unimpaired. As far then as a single case goes, we may come to the conclusion, that we are not to make the diagnosis of the first form of apoplexy, unless, in addition to the double paralysis, there are coma and loss of intelligence and sensation. The great points of diagnosis are coma, suspension of the phenomena of the mind, and paralysis of both sides of the body, both of motion and sensation. We now come to consider the symptoms of the second or milder form of the disease. A person falls down in a state of insensibility, but, when you come to examine him, you find that the coma is not so profound, nor is the paralysis and loss of sensation so complete. The eyes are to a certain degree susceptible of the impressions of light, signs of uneasiness are exhibited when strong pungent odours are applied to the nostrils, and indications of suffering are given if you pinch or burn the skin. All these circumstances prove, that the paralysis of sensation is by no means so complete in this as in the former case. You observe here, too, that instead of the cheeks being puffed out in the manner before described, there is only a partial paralysis of the muscles of the face, *and the mouth is drawn towards the sound side.* The patient, too, instead of dying in a comatose state, gradually regains his intelligence, and is only paralysed on one side, or one extremity. All these circumstances point out that the injury done to the brain is not so extensive, and the occurrence of paralysis on one side shows that the effusion is limited to a single hemisphere of the brain. All this, too, is borne out by pathological anatomy, which shows us, in the first place, that the extent of the effusion is much less, that it exists only on one side of the brain, and never bursts into the ventricles. The general congestion of the head also is much less than in the former case. In the third form, the congestion and other symptoms are sometimes very slight. A person in health may feel a stunning sensation in the head, followed by some thickness of speech and drawing of the mouth to one side, or slight paralysis of one arm or hand, but he has no coma or loss of intelligence, and the paralysis quickly disappears. Every thing connected with the attack shows that it is very slight, the effusion is extremely limited, and this is confirmed by pathological anatomy.

I have now given you a brief sketch of the three varieties of apoplexy; between these you will meet with many intermediate cases.

Let us enquire how far does the circumstance of paralysis point out the occurrence of an extravasation of blood into the substance, or on the surface of the brain; that is, how far we can say that this patient has effusion, because he has become suddenly paralytic. It would appear, that the mere suddenness of the attack will not alone lead to the formation of a certain and accurate diagnosis. You will find in various authors many instances of affections of the head,

not of an apoplectic character, in which there was sudden paralysis. Thus, for instance, there are many cases of tumours and encysted abscesses on record in which there was sudden paralysis, and where, if you should pronounce the disease to be apoplexy, you would be certainly wrong. We had lately, at the Meath Hospital, a remarkable instance of this. A patient, who had been for a considerable time labouring under aneurism of the innominata, in the course of the night became suddenly hemiplegic. On examining the brain, post-mortem, there was a circumscribed abscess found in one of the hemispheres, but no sanguineous effusion. If you look to the works of Abercrombie, Rostan, Lallemand, &c., you will find many cases detailed in which sudden paralysis occurred from other causes than apoplexy.

But are there no circumstances, which, combined with the suddenness of the attack, would lead us to form the diagnosis of apoplexy? Now it would appear that, as a diagnostic of apoplectic effusion, *suddenness of paralysis* is only to be relied on where there have been no premonitory symptoms of a local disease of the brain. In the great majority of cases of cerebral abscess, you will find that pains and cramps in some of the limbs, and pain of the head in the situation of the abscess, have preceded for some time the paralytic attack. But if a person in health, without any of these cramps or pains, gets a *sudden* attack of apoplexy, and becomes hemiplegic, you may make the diagnosis of apoplectic effusion with tolerable certainty. The fact of the paralysis occurring with an apoplectic seizure, renders it highly probable that the case is really one of the hæmorrhagic diseases of the brain. On the other hand, it is true that we may have apoplectic effusions ushered in by symptoms of irritation of the brain; as in the case of an apoplectic effusion occurring in the centre of a softening of the brain. The absence, therefore, of these premonitory symptoms appears to be necessary towards forming the diagnosis of simple apoplectic effusion.

LECTURE XXX.

Apoplexy from ramollissement of the brain—Supervention of apoplexy on encephalitis—Inflammation round the clot—Variety of paralysis consequent on apoplexy—Paralysis croissée—Different forms of paralysis—Origin—Phenomena of face and tongue—Paralysis of the tongue—Treatment of apoplexy—Blood-letting—Purgatives—Lotions, beneficial effects of—Emetics, dangerous effects of—Use of revulsives and stimulants—Treatment of paralysis—Efficacy of strichnine—Its *modus operandi*—Brucine, its proposed employment.

I left off at my last lecture in considering how far the mere circumstance of suddenness of attack with paralysis could be considered as leading to the diagnosis of apoplectic effusion, and endeavoured to show that mere suddenness of attack with paralysis was insufficient to form a diagnosis, except where they

occured in a person who had no previous symptoms of irritation of the brain;—these symptoms being pain of the head, and pains, spasms, and rigidity of the limbs. I wish to impress upon you, that you may have an attack of apoplexy with effusion ushered in by all these symptoms, particularly in cases where the apoplectic effusion is consequent on a localised inflammation of the brain. A portion of the brain, for instance, becomes inflamed and disorganised; local ramollissement takes place; and it may happen that this, acting as a point of attraction to the fluids, may lead to the occurrence of an apoplectic effusion in the originally affected portion; and in this way you will have apoplexy preceded by all the symptoms which characterise a partial encephalitis. You will perceive, then, that the absence of these premonitory symptoms is necessary towards forming a *certain* diagnosis of apoplexy with effusion. If these symptoms have preceded the attack, it is probably either circumscribed abscess of the brain, or it is local inflammation followed by effusion. Between these two forms of disease we have no means of distinguishing.

Before I speak of paralysis I wish to make some remarks on a condition of the brain which supervenes in certain cases of apoplexy. In cases where absorption of the clot takes place, we cannot suppose that any inflammatory condition of the brain exists; on the contrary, we have every reason to believe that a non-inflammatory condition of the brain is highly favourable to this process, for whenever any thing of an opposite character happens, we find that it prevents absorption. But sometimes cases occur, in which, at an earlier or later period, inflammation is set up round the clot. Now, what happens in many of these cases? Here let me repeat, that there are many exceptions to the rules given for forming the diagnosis of disease of the brain; the variety in the symptoms of cerebral affections being so great, that it is sometimes difficult to deduce from them rules of general application. In most cases we have apoplexy followed by paralysis with resolution; but, in cases where inflammation takes place round the clot, it has been observed that the paralysed limb which had been previously in a state of resolution becomes contracted, and then we have paralysis with contraction. This contraction generally comes on in a gradual manner, but, when the case is severe, it is frequently ushered in by violent spasmodic action of the affected limbs. We have, then, the following order of phenomena: first, paralysis with resolution, and then paralysis with contraction. In circumscribed inflammation of the brain, the phenomena are the reverse of these; we have, first, rigidity and contraction of the limbs, and then symptoms of apoplexy followed by paralysis with resolution.

With respect to the paralysis which is consequent on an attack of apoplexy, there is the greatest possible variety. In some cases there seems to be paralysis of all, or almost all, of the muscles of animal life; in others, it affects only the muscles of one side of the body. A rare and extraordinary form of paralysis has been described by the French writers, who have given it the name of

paralysis croissée. In this form of the disease there is an affection of both sides, but not of the symmetrical members; we find the left arm and the right leg paralysed, and *vice versâ*. This is an unusual form, in fact the rarest to be met with in practice. We may also have great varieties in the amount of the paralysis; in some cases both sides being affected, in others only one, while in others there is only a single extremity or one side of the face paralysed. We may also have complete paralysis of one side without any affection of the face. I remember a remarkable case of this kind, of which I shall give you an abstract. A gentleman, of stout muscular habit and a strong full pulse, had been suffering for a long time under an obstinate gouty affection. From a repetition of the gouty attacks he got a chronic swelled state of the lower extremities, which continued for some time, he being in other respects in the enjoyment of excellent health. The swelling, however, preventing him from taking his usual exercise, he applied for advice. Laced stockings were advised, the effect of which was, that the œdema subsided, and the motion of the lower extremities was restored. It is curious, that, between the period of the removal of the œdema and the paralytic attack which I am about to describe, this gentleman enjoyed excellent health. At the end of that time, on attempting to go over a step that led into the yard, he found he could not accomplish his purpose, and struck his foot against the stone. He immediately became alarmed and sat down, and soon after found that he had lost the power of using his arm. I saw him in a short time after the accident, and found that there was complete paralysis of the arm and leg, but no distortion of the face or tongue, or the slightest lesion of intelligence. He continued in this state for some time, and then recovered, but it was necessary to take a large quantity of blood from him. In the first bleeding, as the pulse was full and bounding, I took sixty ounces of blood from the arm, and I think it was owing to the activity of the measures adopted that he recovered so speedily. I mention the case merely to show that we may have paralysis of the leg and arm, without any affection of the face, or loss of intelligence. In some cases we find the paralysis affecting the tongue, face, and muscles of the eyelids; in some we have paralysis of the sphincter ani, or of the muscles of deglutition, or of the bladder, but these are rare, and the most ordinary form is paralysis of the muscles of one side, and distortion of the face. There is another circumstance, which seems to be so exceedingly frequent as to form a law, perhaps the most general of any in medicine, that paralysis occurs on the side of the body which is opposite to that on which the effusion occurs. If you have an effusion into the right hemisphere, you will have paralysis of the left side of the body, and, if the effusion be on the left side, the paralysis will be on the right. To this rule, however, it has been stated that there have been a few exceptions; how they have occurred it is totally impossible to explain; it is sufficient for us to know that such exceptions have been witnessed. Cases of this description have been very rarely seen since patholo-

gical anatomy has been studied with more diligence; it is however true, that a few have been detailed by men of great professional eminence. We want facts to throw light on this point, and, until this is accomplished, we must remain in ignorance of the cause of the anomaly. In the vast majority of instances, the paralysis is on the opposite side to that on which the effusion takes place, and this appears to be explained by the decussation of the fibres of the brain at the upper part of the spinal marrow, the fibres of the left side passing to the right, and *vice versa*. It is an interesting fact connected with this subject, that the muscles of the face follow the same law as the muscles of the extremities, and yet it is a fact, as you are well aware, that the nerves which supply the muscles of the face come off before the decussation of the fibres of the brain takes place. The fifth nerve, which supplies the face with muscular branches, is given off at a considerable distance from the decussation of these fibres, and yet we perceive that the muscles to which it is distributed obey the same law as those which derive their nerves from the spinal cord. Now, if this decussation was the only cause of the paralytic symptoms being observed on the side opposite to that in which the effusion occurs, the muscles of the face should be an exception to this law; but we find that they correspond with other parts of the muscular system in this respect. Thus, if a man gets an attack of apoplexy, followed by paralysis of the left arm, we find the left side of the face affected, and *vice versa*. We must conclude from this, that the mere decussation of the fibres is not the sole cause of this peculiarity, and must look for an explanation elsewhere, by referring it to the intimate communication which exists between both sides of the brain by means of its commissures. Many persons are not familiar with the phenomena of the face and tongue in paralysis; they are, however, simple and easily explained. Let this diagram represent the head—here we have the right hemisphere of the brain, here the left. Now, suppose you have an apoplectic effusion in the right hemisphere, the consequence is that you have paralysis of the left side of the body, according to the law already mentioned. What will then happen with respect to the face is, that the muscles of the left side being paralysed, and their antagonism destroyed, the mouth is drawn by the sound muscles of the opposite side from the paralysed side, and this is invariably the case. Recollect, then, that the mouth is always drawn from the paralysed side, and towards that side where the disease exists in the brain. But when you desire the patient to put out his tongue, do you find that the tongue follows the direction of the mouth? No; it goes towards the opposite side. This appears somewhat paradoxical at first, but is easily explained. The protrusion of the tongue is effected by the action of the genio-hyoglossi muscles, which are, as you all know, a pair of fan-shaped muscles, attached to the inside of the chin, the middle line of the tongue, and the body of the os hyoides. This diagram will represent it. Here is the muscle of the left side, and here is the right. When the patient puts out his tongue, this left half being paralysed,

and having lost its antagonism, the tongue obeys the action of this, the right half, and the fixed point of attachment of the muscle being to the right of the mesial line, the base of the tongue is brought forward, and to the right, and its point consequently deviates to the left or paralysed side. It has been remarked, also, that there is some variety with respect to the paralysis of the tongue; some patients can protrude it, others cannot. In some cases, too, the patient can put out his tongue well enough, but he cannot employ it in the articulation of sounds, and his speech is quite indistinct.

I might occupy your time for several lectures with these subjects; and, did my time permit, I could lay before you a vast quantity of interesting matter on the subject of paralysis from apoplexy; but, as the number of lectures is limited, all I can hope to accomplish is, to point out the great landmarks to you, and leave the rest to your own study and experience. With respect to paralysis of the extremities, the upper are paralysed more frequently than the lower; and, when both extremities are engaged, the upper are generally more completely affected than the lower. When a person recovers, also, we find that the lower extremities are the first to regain their lost power and sensibility. These circumstances have been attempted to be explained by considering the particular parts of the brain in which the effusion has occurred; but, as this has not as yet been sufficiently made out, I shall pass it over. I regret, also, that I have not time to enter into the subject of the different varieties of lesion of intelligence in cases of apoplexy. I must however observe that the varieties are infinite, and your trouble will be amply repaid by reading what has been written on this point by Dr. Abercrombie, and Dr. Cooke in his *Treatise on Nervous Diseases*. You will find in the latter work an extraordinary collection of facts with respect to lesions of the intellectual functions.

I shall now endeavour to get through the treatment of apoplexy as briefly as the important nature of the subject will admit. I shall commence by saying, in the words of Dr. Abercrombie, that the remedies for apoplexy are few and simple. The great point is to relieve the head from the accumulation of blood, to prevent further congestion, and to obviate inflammatory action; and for these purposes the only efficient means we possess is bleeding. There is no disease in which the efficacy of free and bold depletion by the lancet is more remarkable than in apoplexy. I agree completely with Dr. Abercrombie in thinking that the symptoms which denote serious apoplexy by no means contra-indicate the use of the lancet; for I have already shown, that serous apoplexy was nothing but congestion, that the serous effusion was one of the consequences of this congestion, and by no means the cause of the apoplectic symptoms. Dr. Abercrombie thinks that, in the commencement of the disease, you may bleed where the pulse is feeble as well as where it is strong and full, and gives many important cases in which the disease yielded to a copious abstraction of blood, though the state of the patient's pulse

and general system at the time were such as would deter many from bleeding. He gives three cases of persons about seventy years of age, on whom this mode of treatment was practised with success, and another of a person of spare habit, aged eighty years, whose life was saved by a bold and timely use of the lancet. There is also another case detailed of a patient who was worn down and dropsical at the time of the attack, and received considerable relief from bleeding. I do not wish you to conclude from this that you should bleed as boldly in the one case as in another; what I wish to impress is this, that in a vast majority of cases it is advisable to have recourse to the lancet. With respect to the first bleeding, I think that where the pulse is full and strong it should be large, and such as will produce some effect on the symptoms. This may be repeated afterwards to a smaller amount if necessary; but the subsequent bleedings should be rather local than general, except where there is any renewal of the cerebral and circulatory excitement, which must be always met with activity. I believe the cases in which you must make the largest bleedings are those in which there are symptoms of an hypertrophied heart. But where this is not present, one or two bold bleedings, followed by local depletion of the head, will be sufficient. In cases of apoplexy, you may either open a vein or the temporal artery, for the objections made to arteriotomy in phrenitis do not apply so much to cases of apoplexy. There is no violence on the part of the patient, nor is there the same chance of the vessel giving way. The head should be shaved and freely leeched, and the patient may be cupped on the temples or the back of the neck.

Next in efficacy to general and local bleeding seems to be the administration of strong purgatives. There are many cases on record in which the coma and other symptoms have resisted bleeding, both general and local, but have disappeared under the influence of active purgation. One of the great objects in the treatment of apoplexy should be to get rid of the coma as soon as possible; and for this purpose nothing appears to answer better than the early use of brisk purgatives. Dr. Abercrombie recommends croton oil as the best purgative that can be employed, and indeed it is an excellent one; but if the patient can swallow, you need not be very anxious about the kind of purgative you prescribe; any active purgative followed by a strong enema will do. Where the patient cannot swallow, you may mix the dose of croton oil with some mucilage, and pass it into the œsophagus by means of a gum elastic tube.

After purgation, the next thing is to apply cold to the head by means of cold lotions, or iced water, or by pouring a stream of cold water on the head. This is a measure of great efficacy, and one which you may employ with safety and advantage.

In cases of apoplexy, where the coma has resisted free bleeding, both general and local, and where purgation and cold applications to the head have been employed without any decided effect, it seems advisable to apply a blister to the head or nape of the neck.

You will recollect that I told you that blisters were always dangerous in the early periods of all acute visceral inflammations. This, however, does not apply so much to cases of hemorrhagic effusion like apoplexy, in which blisters may be employed at an earlier period than in cases of active inflammation. I would advise you, therefore, to use blisters in cases of apoplexy attended by persistent coma, having first put into practice the means already mentioned.

Many persons advise the use of emetics in apoplexy, but the facts bearing on this point, to which I have drawn your attention when speaking of inflammation of the brain, will also apply here. You may take it as a general rule, that where congestion of the head exists, vomiting will always increase it, and must be therefore exceedingly dangerous. As far as theory goes it is totally against this practice, and I believe experience also is opposed to it. In a number of cases of disease of the brain, where emetics were employed, it has been found that an unfavourable result ensued, and there are some cases of apoplexy on record in which the exciting cause was a fit of vomiting.

Suppose that, after having taken away blood, purged actively, used cold applications, and blistered the head, the coma still remains, accompanied by a feeble pulse and cold skin, what are you to do? I believe, under these circumstances and these alone, you may venture on the use of internal stimulants. Though this is at best but a forlorn hope, still the practice appears rational; we have analogy to guide us in the use of stimulants in such cases, and there are cases on record of persons who have recovered from this state by their judicious employment. The remedies most generally prescribed for this purpose are camphor, musk, and carbonate of ammonia. In the cases of typhus, we know that these remedies have frequently succeeded in removing the coma; but I repeat, that you should never have recourse to stimulants until the period for depletion has passed by, and all the ordinary means have failed.

I shall now suppose that we have succeeded in removing the coma, that consciousness has returned, and that nothing remains but paralysis of one side. Our great object is to get rid of the paralysis as soon as possible. Here you will recollect that you have to deal with paralysis depending on extravasation, a paralysis which, as far as we know, will not disappear under any form of treatment until the extravasated blood has been absorbed. The first thing then you have to do is to adopt measures to prevent a return of the attack. This is to be effected by carefully restricting the patient in his diet, by avoiding all causes of cerebral irritation, whether physical or moral, and by obviating every thing capable of exciting the circulation. But you should not be content with this: you should from time to time employ local depletion, which in cases of this kind has a double utility. It tends to prevent a repetition of the attack, and, by lowering the circulation, keeps the brain in that non-inflammatory condition which is most favourable towards pro-

moting the absorption of the coagulum. In many cases, also, you will find it of great advantage to establish a drain in the vicinity of the disease, and a great deal of good may be done by putting a seton, or an issue, in the neck. You must also pay constant attention to the state of the bowels and urinary system in cases of paralysis; keeping up a steady but mild action of the bowels has an excellent effect, and I need not impress upon you the necessity of paying strict attention to the bladder.

The paralysis which supervenes on an attack of apoplexy, is to be treated always in the first place by means directed to the head, and the brain is to be put in such a state as will favour the removal of the clot by the means already recommended; in addition to which it will be necessary that the body and extremities should be kept in a warm temperature. But there is this very singular circumstance connected with some cases of paralysis, that a period will arrive when, although the original disease of the brain has been removed, and the clot absorbed, the paralysis still continues. It is not easy to explain the circumstance; but it has been observed in many persons who have been paralytic, that the clot was completely absorbed, and no existing trace of disease discoverable, such as would account for the continuance of the paralysis. In cases like this we must adopt a different mode of practice, and have recourse to measures capable of exciting the brain, and we have reason to believe that whatever will excite the brain and restore its energy (I must use this phrase for the want of a better) will cure the paralysis. We find that in some cases where the brain of a patient, under such circumstances, has been exposed to any sudden stimulus, whether physical or moral, the symptoms of paralysis have disappeared, sometimes gradually and slowly, at other times rapidly and at once. Now, this disappearance of the symptoms shows that the paralysis did not then depend on the presence of a clot, for if an unabsorbed coagulum remained in the situation of the original extravasation, the paralysis would not disappear. But it has been frequently observed, that a patient, labouring under paralysis, may get rid of his symptoms suddenly, or that, at a certain period, they begin to decline, and then go away altogether. From a consideration of these circumstances we are led to divide the treatment of paralysis of this description into two parts, and endeavour first to excite the brain itself, and next the nerves which supply the paralysed limbs. For this purpose several remedies, supposed to be capable of stimulating the brain, so far as its action on the muscular system is concerned, have been recommended, the most important of which is the *nux vomica*, or its active principle, strychnine. The researches and experiments of modern medicine have already established the efficacy of strychnine in such cases, but you will recollect, as I before stated, that this powerful remedy can be employed with safety only in cases where the paralysis continues after the disappearance of organic disease of the brain. Until that period arrives, and all symptoms of congestion and excitement are removed, it would be improper to prescribe the use

of strychnine. One of the most recent publications on this subject is from the pen of Dr. Bardsley of Manchester, in which you will find an exceedingly interesting series of cases treated with strychnine, and many of them with the most decided success. In most of these cases you will find that Dr. Bardsley, even where the disease has been of some standing, *precedes the use of strychnine by measures calculated to deplete the head*, even though the cases were chronic. Hence, whenever you are about to prescribe this remedy, you should be satisfied that depletion has been sufficiently performed. You may be called to treat a patient for paralysis after an apoplectic attack. Here you must consider how far you are to premise the use of strychnine by depleting measures, and you must also reflect that we here have shadowed out one of the most important principles in medicine, *that in almost all cases where a cure is to be attained by stimulation, it will be effected more readily, and with much more certainty, when preceded by local depletion, no matter how long the disease may have lasted.* The efficacy of strychnine in paralysis seems to be dependent on the antecedence of local or general depletion.

Strychnine being an exceedingly active remedy, and having a most powerful effect in stimulating the brain, it being also one of the accumulative class of medicines, it will be proper to commence its exhibition with a very small dose, and watch its effects with care. The following is the formula which I would recommend you to employ. You take a grain of strychnine, and your object being to divide it into a number of equal parts, (say sixteen,) to ensure an accurate division, you dissolve it in a small quantity of alcohol, and, having mixed this solution with a sufficient quantity of bread crumb or conserve of roses, you divide it carefully into sixteen equal pills. In this way you may be tolerably certain that each pill contains one sixteenth of a grain. Begin at first with one pill a day, next day you may give two, and so on until you have brought it up to half a grain or a grain, watching carefully its effects. Now, what are these effects? They are very analogous to the phenomena produced by inflammation of the brain taking place in the vicinity of the clot, namely, spasms of the muscular system.

It is also a curious fact, that these spasms are principally observed on the paralysed side; in other words, that the portion of the brain which has been affected by disease is more sensible to the stimulus of the strychnine, the consequence of which is spasmodic twitches in the paralysed limbs. The great nicety of practice in the treatment of paralysis in this way, is to keep up a certain degree of this irritation without letting it proceed to any degree of violence, and to omit it whenever the following symptoms become manifest—headache, giddiness, weakness and sickness of the stomach, and too violent spasmodic twitches of the limbs.

There is a great difference with respect to susceptibility of the effects of this remedy in different individuals; in some the effects speedily appear, and you are obliged to intermit its use; others will

bear large doses for a considerable time, and you may push the strychnine until a grain or a grain and a half is taken in the day. I have myself given to one patient a grain every day for the space of a fortnight without any intermission. In all cases, however, it will be necessary to watch the symptoms. There is one effect of strychnine which appears to be unfavourable, and whenever it occurs you should either omit the medicine or diminish the dose. Along with or succeeding the spasms, *there is a tonic rigidity of the limbs*; when this occurs you should be cautious in the administration of strychnine. The length of time which it should be continued will of course vary according to circumstances, but you should be aware that it requires a considerable period of time to produce its effects. In all Dr. Bardsley's cases, and in all those treated at the Meath Hospital, it has been continued for a considerable time, certainly more than a month. It is also necessary for you to recollect that strychnine is one of those medicines which are termed accumulative, that is to say, remedies the operation of which, after remaining latent for some time, suddenly explodes with great violence. When this occurs, the strychnine must be immediately given up, and steps taken to control its effects. One of the best things for this purpose is the carbonate of ammonia with some mild anodyne. I have seen very severe spasms from the use of this medicine. In one case these spasms were so violent as to roll the patient nearly out of bed.

It has been proposed to employ brucine as a substitute for strychnine. Of this remedy I can say but very little; I have given it but very seldom, I believe in only two cases, and in these without any sensible effect. It is much weaker than the former remedy, one fourth of a grain of strychnine being equal to six grains of brucine. Other remedies have been proposed for the same purpose, among the rest, iodine, which has been recommended by Dr. Mansfield.

The next class of remedies are those which are employed for the purpose of exciting the nerves of the paralysed limb. As my time, however, has expired, I must postpone the consideration of these until our next meeting.

LECTURE XXXI.

Local treatment of paralysis—Flesh-brush, shower-bath, &c.—Application of moxa—Cases in which it is useful—Professor M'Namara's plan—Acupuncture with galvanism—Electro-puncturation—Method of applying—Powerful action of a small battery—Mr. Hamilton's observations—Value of galvanism and electricity—Use of, in paralysis of the muscles of the face—Paralysis from disease of the arterial system—Case of, by Dr. Graves—Diagnosis of this affection—Pathology of Pott's gangrene—Dupuytren's mode of treatment.

Having spoken of the general treatment of paralysis after apoplexy, we come now to the local management of the disease, or that portion of its treatment which consists in the application of stimulants to the nerves and their origins. Local stimulation of paralytic limbs may be performed in a variety of ways; all the usual stimulant embrocations may be employed for this purpose with the best effects. I shall not take up your time in detailing the different kinds of liniments which are used on such occasions; they are universally known, and may be varied *ad infinitum*. The flesh-brush, the shower-bath, either tepid or cold, occasional blisters to the spine, or along the course of the nerves, croton oil and terebinthinate frictions—all these are measures that may be employed with advantage. The use of the moxa has been also strongly recommended, and appears to be decidedly beneficial. The efficacy of all these remedies, however, seems to depend chiefly on the particular stage and nature of the disease, and hence their good effects are most apparent in those cases where the paralysis no longer depends on organic disease of the brain, but seems to be connected with that peculiar state of the nervous system which arises from a long interruption of the power of transmitting volition. It is in cases like this that the application of the moxa has been found to produce the most favourable results. Where the lower extremities are affected, it may be applied over the sciatic nerve on the loins, or a little below and to the outer side of the popliteal space over the track of the peroneal nerve. In case of paralysis of the upper extremity, you may apply it to the back of the neck, or in the neighbourhood of the brachial plexus.

A gentleman who does me the honour of attending these lectures, has related to me the particulars of a remarkable case, which I shall mention *en passant*. A young female was subject to repeated violent attacks of spasms with contraction in one of the upper extremities. She had laboured under this affection for a long time, and tried various remedies without benefit. At the suggestion of this gentleman she tried cupping in the neighbourhood of the shoulder and brachial plexus, and found that it produced decided relief to the symptoms. In this case it is highly probable that the disease was seated in the brachial plexus, and had no connection with the brain, for it had continued for a great length of time (more than three years, I believe) without any remarkable variation in its symptoms. If the spasms of the arm had been produced by

irritation of the brain, she would in all probability have had paralysis long before this period; this, however, did not occur, and the probability that the disease was seated in the brachial plexus is still further confirmed by the fact, that the spasms were relieved by local bleedings. Here we have the spasms relieved by antiphlogistic means, but in a case of atony of the same nerves most benefit would be derived from the use of stimulants. The more completely the paralysis is of this description, the more sure will be the effects of local stimulation. You will sometimes meet with cases of paralysis from pressure on the nerves without organic disease. Thus there is a case on record of a person who lost the use of one of his upper extremities, from having leaned too long over a bench at a public meeting. I recollect the case of a man, who during a fit of intoxication fell asleep with his arm thrown over the back of a chair, and awoke with perfect paralysis of the hand. Cases like these are seldom of long duration, and are much improved by the application of the moxa. I may state, however, that permanent paralysis has been induced in this way. The best way of using the moxa is, not to make a deep eschar, but to touch the parts slightly, and repeat the application frequently. In the case of paralysis of the hand, immediate relief followed the use of the moxa to the back of the wrist.

While on this subject I may advise you always to employ the moxa in the mode first, I believe, devised by my friend and colleague, Professor M'Namara. The top of the moxa is to be dipped in a strong solution of the oxymuriate of potass, which is to be allowed to dry upon it. The moxa being fixed to the part by a little gum, a drop of strong sulphuric acid will produce immediate ignition. In this way you prevent all the alarm which the patient feels at seeing a lighted candle brought to the bed-side. The same rule is to be observed when you employ electricity, the best mode of using which is to place the patient on an insulated stool, and draw sparks from, or shocks through, the affected limbs. Electricity frequently does much good in such cases; but, in order to obtain decided benefit from it, you must persevere for some time in its employment. It has been lately proposed to employ the stimulus of electricity and galvanism in a different way, by transmitting it directly to the muscles of the affected limbs by means of needles, which are to be inserted into different parts of paralysed extremities, and which are intended to act as conductors for transmitting the galvanic influence. This has been termed *electro or galvanic puncturation*, and forms an excellent mode of applying the stimulus of galvanism. I have made many experiments as to its effects, to which I shall briefly direct your attention.

The first thing to be considered is the manner of its application. The following is that which I use at the Meath Hospital:—Having procured two fine sewing needles, your first step will be to take the temper out of them; for, if you employ them in the tempered state, you will run the risk of their breaking in the flesh, and this would be very disagreeable. You can easily take the temper out

of them by holding them in a candle until they become red hot, and then letting them cool gradually. The next thing is to place a head which will remain firm on the needle, and for this purpose you pass a small portion of thread through the eye, and then cover it with a bit of melted sealing wax. Having thus formed a head for the needle, you sharpen its point, and polish it by the emery pincushion, and the sharper it is the better. There is nothing more simple than to introduce the needles. You make the part of the skin tense with your finger and thumb, where you intend to introduce them, and, placing the point of the needle perpendicularly on it, you press it downwards in a slanting direction, using, at the same time, a rotatory motion, and thus easily pass it in; when you have pierced the skin and fascia, there is no difficulty in introducing it into the muscular fibres. The distance between the needles must be regulated according to circumstances. You then proceed to send the galvanic fluid to the part, and, for this purpose, the best mode is to employ a small galvanic battery with a limited number of plates. If you have plates of from two to three square inches you will find that from fifteen to twenty of these, in a state of ordinary action, will be quite sufficient, particularly in the commencement of the treatment. It is a curious fact, that the intensity of the shock is increased to an extraordinary degree by means of the needles. A battery which in the usual manner would not communicate any shock, will, when used with the needles, give a violent one, and communicate such a stimulus to the nerves as will throw the whole limb into violent spasms, and cause a copious perspiration to break out over the body. I have seen very great effects from a feeble battery in this way, and it would appear that this is the result of the *direct* transmission of the galvanic influence to the muscular fibre. In most cases a perspiration is brought on, the limb convulsed, and sometimes the whole body is thrown into spasms. As an illustration of the power of the battery when used in this way, I shall mention the following case:—A patient who was under the care of Mr. Hamilton, laboured under amaurosis; he was anxious to try the effect of galvanism, and with this view inserted one needle in the upper part of the back of the neck, and another over the orbit, so as to direct the trajét of the fluid across the base of the brain. He intended at first to use a small battery of twenty-five plates, but it struck him that even twenty-five might be too much. He made the experiment with three pairs of plates, and, the shock being given, the patient, to his astonishment, fell back as if he had been stunned by a violent blow on the head, and remained for nearly a minute in a state of insensibility. In other cases, too, where the galvanism was applied in the vicinity of the head, I have found that severe headache, giddiness, and even a stiffness of the muscles of the face, were produced; all showing its powerful action on the nervous centre.

Some singular circumstances connected with this subject were observed in the Meath Hospital. It was found that after a certain number of shocks had been communicated to the parts, when you

came to withdraw the needles there was a very remarkable difference in the case of removing them. The needle through which the positive galvanic influence had been transmitted, was found to be strongly fastened in its situation, while that to which the negative pole had been applied, slipped out with the greatest ease. This result was constant. In some cases, where half a dozen shocks or so have been given, the extraction of the positive needle has been only accomplished with considerable pain to the patient.

It has been suggested by a distinguished scientific friend of mine, that this results from the coagulation of albumen at the positive pole. Mr. Hamilton, however, who performed most of the operations for me, thinks that the true explanation is the paralysing effect of the negative pole on the muscular fibre, while the positive needle is firmly grasped by the increased contraction. Further researches are necessary on this point. Another fact connected with this subject is, that when the needles have been inserted into a large muscular mass, the positive needle is powerfully retracted, and carried, as it were, into the muscles. In one case, where the needle was inserted into the lumbar muscles, in a patient labouring under sciatica, more than one twelfth of an inch of it was drawn in at each shock; so that, after a certain number of shocks, it passed up to the head. This is one reason for using the sealing-wax head, in order to prevent the complete passing in of the needle.

With respect to our experience of the value of this mode of employing electricity or galvanism, I have to remark that, if galvanism or electricity can be of any use to paralysed limbs, this is one of the best modes in which it can be applied. The apparatus is simple, can be prepared in a moment, and does not depend on the state of the weather, like the ordinary electrical machines. There is another advantage, also, it is not so likely to excite alarm in the mind of the patient. We have employed it in several rheumatic and paralytic cases in the Meath Hospital, but have not as yet been able to say that decided benefit has accrued from it to the majority of the patients on whom it has been tried. This is more particularly true with respect to paralytic patients; in the rheumatic cases we have found it more beneficial. In a remarkable case, where the deltoid muscle was paralysed and atrophied from some affection of its nerves, Mr. Hamilton tried it for a fortnight without any good effects. In a case of senile amaurosis, its effect was to produce flashes of light before the eyes, lachrymation, and contraction of the pupil, but after a fortnight's trial there was no improvement in the sight. We have had, however, distinct and unequivocal proofs of its value in one case of paralysis of the muscles of the face, which had all the characters of that described by Sir C. Bell, as resulting from an affection of the seventh pair of nerves. I have not the notes of this case at present, but shall bring them down and lay them before you on to-morrow. I may however observe, at present, that this patient had been for a long time labouring under an affection of one side of the face, and had used a variety

of remedies. Those principally employed were stimulating liniments and the internal use of strychnine, from which he derived some slight benefit; but the application of the galvanic fluid, in the way I have mentioned^d, was followed by decided and rapid improvement. Indeed, from the time it was first applied, the patient recovered rapidly, so that in a very short time all the deformity of face disappeared. Now the value of the application is to be estimated in this way. Here we have a case of paralysis of a local nature, and not depending upon any disease of the brain; in this case the galvano-puncturation was tried, and found to be most beneficial. The conclusion, then, as far as a single case goes, is, that this mode of treatment is best adapted to the form of paralysis just mentioned, in which we find an affection of some of the muscles remaining after the original disease of the brain has been removed. The same observation, I need not tell you, applies to all other remedies which are employed for the purpose of local stimulation.

Before I leave the subject of paralysis, there are two points to which I wish to call your attention. One of these involves the consideration of a remarkable form of paralysis in which the disease appears, as far as we can see, not to depend on any primary lesion of the *nervous system*. In this form we have a paralysis, not the result of any disease of the brain or nerves, but connected with an affection of the vessels of the part. This is a very singular disease, and I am anxious you should be acquainted with it, for I believe it is by no means so rare as many persons think. The other point to which I would direct your attention refers to the influence of magnetism on the human body; of this I shall speak on a future occasion, confining myself for the present to that form of paralysis which is connected with disease of the *vascular system*.

So as to give you some idea of this affection, I think I cannot do better than read for you the notes of a case of it, published by Dr. Graves and myself in the fifth volume of the Dublin Hospital Reports.

A man, aged 44 years, was attacked in December, 1828, with alternate sensations of cold and burning heat in the toes of the right foot. These extended to the leg, of which the power became diminished. Pains in the foot next occurred, and in a month the part became cold and wholly deprived of sensation.

On the day of his admission the pain suddenly extended to the calf of the leg; and from this time he lost all power of motion in the leg. On admission, the temperature of the body, with the exception of the affected limb, was natural. The pain had extended to the thigh during the night. The temperature of the limb was but 58° of Fahrenheit. Slight œdema existed about the ankle. There was complete loss of sensation from the middle of the thigh to the toes; the patient could rotate the thigh slightly, but there was no other voluntary motion possible. The femoral artery appeared like a hard cord, painful on pressure, and without

pulsation. By the stethoscope we found that pulsation was also wanting in the common iliac on this side, while that of the left iliac was plainly perceptible. The patient died on the fourth day after admission, the limb having become purple, tender, and covered with vesications.

On dissection, the right common iliac appeared distended and livid, and was completely plugged up by a dark clot, extending to the external and internal iliacs, and engaging the gluteal and obturator arteries. The same occurred in the femoral and profunda, and extended, as far as they could be traced, to the tibial arteries, and to the peroneal. The lining membrane of these vessels was soft, villous, and red; the clot in some places being separated from it by a layer of puriform matter. No disease in the veins. A large portion of the vasti and rectus muscles was *white* and hardened. Here you perceive a train of symptoms, some of which might be referred to disease of the brain, if the man had any cerebral symptoms, which was not the case, for his intellect was sound, and he had no evidence of cerebral disease except the paralysis.

His constitutional symptoms were emaciation, prostration of strength, and loss of appetite. The temperature of the body was natural, but, on examining the limb, we found (and this is a point of great importance) that it was as low as 58° of Fahrenheit; in fact, it was quite cold. There was also complete loss of sensation from the middle of the thigh to the toes, and though he could rotate the limb slightly, it was, in all other respects, powerless. Here we have paralysis of motion and sensation in one of the extremities, *with remarkable coldness of the limb*. On making an examination along the track of the femoral artery, we found that it was painful on pressure, *without any pulsation*, and conveying to the finger the feel of a piece of hard cord. From a consideration of those circumstances, we came to the conclusion that it was not pervious, and that this would account for the state of the limb. In this case, also, we made another remark, and this, I believe, is the only instance on record in which such a diagnosis was made. Up as high as the groin the pulsation of the femoral artery could not be felt, and we were anxious to ascertain how far further the disease extended. The state of the femoral artery in the left groin was natural. On making an examination with the stethoscope, we found that the pulsations of the aorta were perceptible down to its bifurcation, but when the stethoscope was applied below this on either side, we observed that there was no pulsation in the right common iliac artery, but on the left side it could be traced distinctly down to the groin. Here then we had a train of phenomena, such as ordinarily occur in paralysis affecting the right lower extremity, and along with this an obstruction to the circulation in the thigh and leg. From these circumstances we made the diagnosis of obstruction of the right iliac and femoral arteries. On dissection, we found that the aorta was healthy to within about six inches of its bifurcation; below this point it was

partly filled by a red clot. The left common iliac was healthy, but the right was plugged up with a dark red clot, which extended into the internal iliac and obturator arteries, filling up also the femoral and its branches. The case, in fact, was nothing more or less than one of chronic arteritis.

This remarkable form of disease has been also observed by other authors. You will find it well described in Rostan's work on diseases of the brain, where he mentions that this loss of sensation and motion in a limb is sometimes produced by obstruction of its vessels. In persons advanced in life, the arteries are also frequently obstructed by the formation of ossific deposits within them, producing loss of power, coldness, and diminution of sensation, as in the foregoing case. A similar effect may occur from the pressure of an adjoining tumour on the trunk of a principal artery.

Paralysis resulting from disease of the arterial system is distinguished from paralysis caused by cerebral disease, by the following marks: first, by the colour of the integuments of the affected limb, which, in a case of the former description, are generally of a violet hue, or of a much deeper tinge than in the latter case, or in a state of health. It is very rare to find the two limbs of the same colour, as we do in cases of cerebral paralysis. Another mark is, that the temperature of the limb is always lower than that of the healthy one; but the distinctive sign of this form of paralysis is *the absence of pulsation in the arteries in parts where it should be naturally observed*. If to this description you join the absence of cerebral symptoms, you will seldom fail in making a correct diagnosis. I have had two cases of this disease under my care; one of them occurred in the upper, the other in the lower extremity, and, from observing the characteristic marks already detailed, I had no difficulty in making the diagnosis. It is to that peculiar form of this disease, which is considered by some authors to depend on ossification of the arteries, that the name of "Pott's gangrene" has been applied. A great deal of light has been thrown on this disease by the researches of modern pathology. It is now pretty well established that we may have this gangrene, not only in old persons from ossification of the arteries, but also in the young from arteritis. In truth, the pathology of Pott's gangrene appears to be one of two changes—either an arteritis or ossification of the arteries themselves; and of these two causes the first is by far the most frequent. You will see at once the importance of this view of the question, for if the gangrene occurs in a young person, and is connected with inflammation of the arteries, it is a disease more or less under the control of medical treatment; but if it be produced by ossification of the arteries, the results of treatment are far less likely to be successful.

We have, then, in a case of paralysis of this description, more or less loss of sensation and motion, coldness of the limb, and absence of arterial pulsation. With respect to coldness, it may be said that it is of little value as a sign, being frequently observed in cases of cerebral paralysis. To this it may be replied, that though coldness

is sometimes present in cases of ordinary paralysis, still it is never so remarkable as in this form of the disease, and the temperature of the limb is but a few degrees below the standard of health. Dr. Abercrombie makes a very interesting conjecture on this subject. He says the temperature of paralysed limbs is generally considered to be lower than that of the healthy ones, and, indeed, such is the case; but the true explanation of this occurrence is, that in this condition the limb loses its power of preserving a medium temperature, and hence it is, that, according to the temperature to which it has been exposed, it becomes hotter or colder than the healthy limb. A case is mentioned, of a medical man who laboured under paralysis of one of the upper extremities. This gentleman, on one occasion, after having applied some warm bran to the paralysed limb, was astonished to find, on touching it with the sound hand, that he could not bear the heat, though he was at the same time unconscious of any increase of temperature in the paralytic extremity.

The symptoms, then, of this form of paralysis are, diminution or abolition of sensation and the power of motion, a dark or violet hue of the skin, remarkable coldness, and absence of pulsation in the arterial trunks which supply the affected limb. These, with a tendency to the formation of gangrene, are the characteristic marks of the disease, and, by bearing them in mind, you will seldom err in making a diagnosis. In the great majority of cases the disease is confined to one extremity; but Rostan gives some cases in which it was more general. We might also add to the diagnosis, that paralysis connected with disease of the brain often comes on suddenly, while in this case its invasion is slow and gradual. It is however true, that some cases of paralysis, depending on this cause, have come on so suddenly as to render this circumstance of less value as a diagnostic.

With respect to the treatment of this form of paralysis, if the patient be young and the disease recognised at an early stage, it is possible that you may be able to arrest it by free local depletion and other antiphlogistic means. In the case which was under treatment in the Meath Hospital, the symptoms had lasted for a considerable time before the disease exhibited any remarkable violence. The man was admitted on the 7th of February, and at this time the disease had been five weeks in existence, having begun at the lower part of the limb, and extended gradually upwards until it involved the whole leg and thigh. Yet it is very probable that this patient might have been saved, if proper means had been taken to arrest the inflammation of the vessels at an early period. Baron Dupuytren has published a case, in which it appeared that this disease was setting in, but was checked at once by bold antiphlogistic treatment directed to the affected limb.

LECTURE XXXII.

Paralysis from arterial disease—Singular cases of, by Rostan—Diagnosis of paralysis from arterial obstruction—Magnetism, use and action of—Effect of magnetism in disease—Result of trials in the Meath Hospital—Paraplegia—Mechanical hyperæmia—Occurrence without disease of the cord or vertebræ—Cases by Mr. Stanley—Effects on urine by division of the spinal cord—Ammoniacal urine—Caries of the vertebræ—Diagnosis of paralysis with disease of the kidney—Prognosis in paraplegia.

At my last lecture I spoke of that form of paralysis which depends on arterial obstruction, and mentioned, as one of the principal diagnostics, a remarkable coldness of the diseased limb. I quoted for you a passage from Dr. Abercrombie's work, in which he suggests that it is probable that the actual condition of paralytic limbs, in the usual acceptation, so far as temperature is concerned, depends upon their having lost that power which animal bodies possess of preserving a medium temperature; so that their temperature becomes elevated or lowered, according to that of the surrounding matter. The general rule in cases of this description is, that the temperature of a paralysed member is a little lower (say two or three degrees) than the rest of the body; but when we find a limb reduced to the temperature of 58° , as in the case I mentioned, it is quite a different thing, and, under such circumstances, the great probability is, that the paralysis is connected with arterial obstruction.

You will see, in Rostan's works on the Softening of the Brain, the reports of two cases of this disease, occurring in patients of extremely advanced aged. In one, there was complete paralysis of the right arm, which was cold and livid. The fingers were threatened with gangrene, and no pulsation could be felt in the radial artery. By stimulating frictions, a certain degree of warmth and motion was restored, and it was even thought that pulsation could be perceived. By degrees, the power of the left arm, and of the lower extremities, began to fail, with diminution of the force of the pulsation. On dissection, extensive disease of the arteries was found; the right brachial, at the insertion of the deltoid, was obliterated by a mass of fibrin, below which the vessel was contracted and closed; the left brachial artery was also narrowed, but without any clot; and this condition was further met with in the crural vessels. The cerebral arteries and the aorta were diseased. In the second case, the patient, aged 80, was attacked with violent pains in the left leg, which became cold and bluish. There was no lesion of intelligence, and the corresponding arm was unaffected. In fifteen days, the pains having augmented, a certain degree of paralysis supervened, which, however, was never complete. On dissection (the disease having lasted a month), the crural artery was found extensively obliterated by a fibrinous clot. Here you observe that, notwithstanding the great age of both patients, the disease was not *ossification*, but, in all probability, *arteritis*.

At our last meeting, I forgot to mention the particulars of a case, bearing on this part of the subject, and which goes to prove that even complete coldness of the affected limb is not, in itself, sufficient to establish the diagnosis of paralysis from arterial obstruction. I have had lately under my care, a gentleman who has been for the last four or five years labouring under paralysis of the lower extremities, unaccompanied by any symptoms indicating disease of the brain. His intellect remains not only unimpaired, but in a state of high activity; and, what is equally singular, he has had none of the usual symptoms of disease of the vertebræ or spinal cord. His limbs, however, are quite powerless, and are of an *icy coldness*; and yet you will hardly believe me, when I tell you that I have repeatedly felt the femoral, popliteal, and even the anterior tibial arteries, pulsating distinctly. This is a singular fact, but I have verified it by a number of observations. You will perceive, then, that in taking a remarkably diminished temperature as a diagnostic of paralysis from arterial obstruction, we must admit that, as a sign, it is only valuable *when combined with absence of arterial pulsation*. In this case, the fact of such extreme coldness of the lower extremities, at the same time that their circulation continues with undiminished activity, becomes of great importance, as tending to prove that the temperature of the body depends more upon the state of innervation than on arterial action. There are, indeed, many facts which go to prove that animal heat is more closely connected with the nervous system than with the circulating.

I spoke of the employment of electricity and galvanism in the local treatment of paralysis. While on this subject, I shall take an opportunity of briefly drawing your attention to the use of magnetism in certain cases of nervous disease. Here let me be understood, I am not going to lecture on *animal magnetism*; it is, at present, a theme unsuited for the practical physician; no one more firmly disbelieves, no one more thoroughly despises, than I do, the countless absurdities which have emanated from the imaginative disciples of animal magnetism. But as in almost every human hypothesis there is a fraction of truth, so in the doctrines of animal magnetism there is, perhaps, something which may not be entirely visionary; and it is possible that there may be some modification of the nervous influence, communicable from one person to another: this is one of the doctrines of animal magnetism. Another leading doctrine is, that organs, which are adapted by nature for the discharge of some peculiar function, appear in their magnetised state to take on a new function. Now, without saying that we are to believe in this, or in the extraordinary romances which are given in illustration of it, still it is right to admit the possibility of its occurrence, to a certain degree; because we frequently observe, in pathology, many instances of organs taking on functions, not merely new, but even totally repugnant to our ideas of their structural arrangements. We may, then, I think, without going too far, admit the possibility of a communication of some modification of the nervous influence from one person to another, and that

organs under this influence may take on new functions; but, in the present state of this subject, this is as far as we can go.

But we have to deal, at present, with a more tangible and important subject—namely, the action of magnetism, in its proper acceptation, on the human body. You are aware that the term *animal magnetism* was first applied to the results of certain effects on the human system, which were supposed to be brought about by the aid of metallic contact; and you are all acquainted with the history of the metallic tractors. The term *magnetism*, however, is totally inapplicable to the communication of nervous influence from one individual to another; nor have we any grounds for connecting such phenomena with magnetism, in its proper acceptation.

That a magnet should act on the human body, is neither extraordinary nor incredible. You know that electricity, and its modification galvanism, have a powerful influence on the system; and modern researches have shown that there is a close connection, if not an absolute identity, between electricity and magnetism. Now, on this subject of magnetism, the researches of some eminent men have been, latterly, employed, and the results of their labours have been received by some with an undistinguishing credulity, and by others with unphilosophical skepticism. One of the principal things which has prevented medical men from entering on this subject is, that many persons have confounded the results of magnetic action on the human body with the absurdities of animal magnetism and metallic tractors. There is, however, I need not repeat to you, an essential difference between them. In the Meath Hospital I have lately made a number of experiments, with a view to ascertain the effects of a powerful magnet on the human body. The magnet which I used, was one of considerable power, being, in its highest state of action, capable of supporting a weight of more than twenty pounds. Now, in almost every instance where this instrument was used, we found that, when brought near to sensible surfaces, phenomena were produced which were very similar, indeed, to those of electricity. These phenomena also appeared in so many cases, and with such a remarkable constancy, that they could not be accounted for by any supposition of accident. We have applied it in cases of rheumatism, sometimes to a healthy part of the body, sometimes to the part affected. In one of these cases, the application of the magnet was followed by a very rapid subsidence of the morbid symptoms, and the patient got well in a few days. Here let me remark, that there is no one more opposed than I am to the publication of the result of a single case as a proof of the success of any particular remedy; and, in putting forward this case, I do not wish it to be received as an instance of a decided cure of rheumatism by magnetism. The only reason why I quote it, is, because in other cases of rheumatism we had distinct evidence of the influence of the magnet to a greater or less extent. The patient, a stout man, of good constitution and in the prime of life, was brought into the hospital for an attack of rheumatism in

the back and left shoulder, which had come on after exposure to wet. The first seizure was three days before admission. When brought in, he had severe pain in the back and shoulder, increased on pressure; he could neither elevate his arm to his head, nor could he bend the head towards the shoulder, without great difficulty and suffering. The value of this case consists chiefly in this, that except using the magnet there was no other remedy employed; if any other medicinal agency had been used, it would have been difficult to attribute the merit of the cure to magnetism. I applied, in this case, the large magnet to the shoulder, within a short distance from the skin. In about half a minute the patient remarked that he felt a kind of pricking sensation immediately under the magnet; this was succeeded by a feeling of heat in the part, which became increased by continuing the application, while at the same time the pain was sensibly relieved. The sensation of warmth continued in the shoulder for about ten minutes after the magnet had been removed, and the patient declared that he received great benefit from the application. On the following day, the magnet was applied again with precisely the same results; the same thing was done on the third day, when the pain was very much reduced, and the arm became more movable. On the fourth day, the mobility of the limb was increased, and he could bend his head in the direction of the shoulder with very little inconvenience. On the eighth day, the power of motion was restored, the pain gone, and the patient left the hospital quite well. I have heard nothing of him since; but if he had not experienced permanent relief, it is very probable he would have returned again, for he seemed quite pleased with his treatment. If this was the only case in which the magnet had been employed, it would prove nothing either for or against its use. It might be said, that the cure, in this instance, was the result of keeping the patient in a warm bed, and that any good, supposed to be effected by the magnet, might be attributed to the influence of imagination. But to this it may be answered, that the sensations observed by this patient were *exactly the same as in others*, each having noticed the peculiar pricking sensation in the part to which the magnet was applied, and the subsequent feeling of warmth. Again, it is to be remarked, that although the symptoms in this case were severe, the cure was extremely rapid; and when you recollect the obstinacy of most affections of this kind, you must allow that a week was a very short space of time for its accomplishment.

The following is another equally interesting case. A woman was admitted into the Meath Hospital, labouring under paralysis of the right side of three weeks' standing. The history of her case was, that she had fallen into a state of mental despondency, after the death of her husband, who was her only support; she then got symptoms of derangement of the stomach, and hypochondriasis, followed by an attack of paralysis, which deprived her of the power of using one side. On examination, we found that, as far as motion was concerned, the paralysis was not complete, but that

there was a total loss of sensation in the side affected. I was curious to observe what the effect of the magnet would be, and accordingly applied it to the spine, moving it upwards and downwards along the cervical and dorsal regions, at about half an inch from the surface. After it had been applied for a few seconds, she remarked that she felt a sensation "of wind" passing over the left shoulder, but not over the right. Observe, it was on the right side of the body that the paralysis existed in this case; and you will also recollect that it was chiefly a paralysis of sensation. Now, it is quite contrary to chances that she should have described it in this way, if she had not really felt it in this situation. The action of the magnet was naturally felt on the left, which was the sensible side, for, on applying it to the opposite or paralysed side, she said she no longer felt the sensation of "the wind." During the operation, she saw the instrument; and if disposed to draw upon her imagination for a description of her sensations, she might say that she felt the aura on the paralysed side; but this was not the case, for she stated that it was no longer perceptible, when the magnet was moved from the sound to the paralysed side. In this case, I must tell you that magnetism was not the sole remedial agent employed, for she had been also leeches and blistered. She was admitted on the 12th of August, and on the 17th of the same month, the power of motion in the upper extremity was so much increased, that she could grasp objects with force, and place her hand on the top of her head with facility. On this day, after having applied the magnet, she immediately exclaimed that she felt the wind for the first time over the right shoulder, and, on examination, it was found that sensation, to a certain degree, had returned to the right side; and here you will perceive that the first manifestation of the return of sensibility was denoted by her feeling the magnetic influence in the paralysed side.

I shall not take up your time any longer by detailing cases; it will be sufficient to state, that it was used in many other instances of a similar kind, and in all with the same result. Each patient described the same sensation, with very little variety; in some, it consisted in a feeling of pricking or tingling; in others, of an aura passing over the part. Some stated that they felt a sensation of warmth in the part, some time after the magnet had been applied; and some, a kind of suction, as if the skin was drawn towards the magnet. When it was applied over a very sensible or a blistered surface, the patients felt the pricking sensation to amount to pain, and the feeling of warmth and suction was proportionally increased. I have also to observe, that it was employed on a set of patients, the majority of whom were totally ignorant of its nature and effects; and yet it is very remarkable that there was an almost universal accordance in their descriptions of the sensation produced. It has been stated, by some writers on the subject, that the sensations differ according to the pole of the magnet employed. This statement does not accord with our experience; for in every one of our cases the sensation was the same, whether we made use of one

pole or the other. As far as our experience goes, it is, I think, fair to conclude that a very perceptible influence may be produced on the human body by the application of the magnet; it is another matter to ascertain how far it may be rendered available as a therapeutic agent. The cases in which I think it might be employed with advantage, are cases of nervous and spasmodic affections, and muscular rheumatism. That the sensations described by our patients had an actual existence, and were not the result of imagination, I am firmly convinced.

Before I leave the subject of paralysis, I wish to draw your attention to one more form of the disease, by no means uncommon; I allude to that in which *both* the lower extremities are exclusively engaged. This is a disease, or symptom, which may arise from a great number of causes, and be observed under a variety of circumstances. Generally speaking, however, it will, in almost every instance, be found to depend on some cause which engages the spinal marrow, either primarily or secondarily. I believe that this paraplegia, as the result of disease of the brain, is never met with except in combination with paralysis of the upper extremities. General paralysis may be produced by cerebral disease; and in describing the various forms of paralysis which depend on disease of the brain, this form has been particularly noticed; but when paralysis of the lower extremities *alone* occurs, it is generally the result of some lesion of the spinal marrow, either organic or functional, below the situation in which the brachial nerves are given off. Among the causes by which this paraplegia is produced, the following are the principal: inflammations of the membranous coverings of the spinal cord, with effusion of lymph or serum; spinal apoplexy, ramollissement from inflammation of its substance; pressure on the cord, by solid tumours from a variety of causes; the bursting of abscesses or aneurismal swellings into the vertebral canal, as occurs in some cases of aneurism of the abdominal aorta. Thus, during the progress of a case of this description, it has been observed that the patient suddenly became paraplegic, and, on examination after death, a quantity of blood, which escaped from the aneurismal tumour, has been found compressing the spinal marrow. Lastly, recent investigations have established the fact that we may have paralysis of the lower extremities, and yet, on dissection, *we cannot detect any traces of disease in the bones of the vertebral canal, or in the membranes or substance of the spinal cord.* Hence you see how cautious you should be in making the diagnosis, so common among surgeons, of caries of the vertebræ, in cases of paralysis of the lower extremities. The truth is, that in the present state of medicine on this subject, we labour under very great difficulties; the diagnosis of these affections is exceedingly obscure; it is a subject still open to investigation, and, I need not remark, that it is one of paramount importance.

Paraplegia is one of the most miserable diseases to which the human body is liable. It is almost always obstinate and unmanage-

able, and in the majority of cases incurable. How far the fatality of the disease depends upon the want of an accurate diagnosis, and a correct plan of treatment, must be determined by future observations; but it is a fact, that a vast proportion of paraplegic patients die, and under the most melancholy circumstances. In many cases, the formation of gangrenous sores on the back and loins is a common occurrence. For this there are two reasons: first, the vessels of those parts exposed to pressure from position fall into that state which Andral terms *mechanical hyperæmia*, the result of which is that they are unable to unload themselves, a stasis of blood follows, and this leads to mortification of the part; secondly, there is a lesion of innervation. Hence it is, that the great majority of patients of this kind die with gangrenous sores on the back and loins. They have also most constantly paralysis of the bladder, or its sphincter, or both, producing retention of urine, or retention with incontinence, or stillicidium urinæ. The sphincter ani, too, is generally paralysed, and we have a most melancholy and disgusting source of annoyance. The frequent passing of urine and fæces, keeps the unfortunate sufferer in a state at once pitiable and loathsome; and when, in addition to his other calamities, the gangrenous sores form, the supervention of low diffused erysipelatous inflammation may prove fatal; or he may be carried off with symptoms of typhus fever, from the absorption of putrid matter.

While on the subject of paraplegia, I am anxious to lay before you a sketch of some important opinions lately put forward by Mr. Stanley, of London. In the last number of the *Medico-Chirurgical Transactions*, this gentleman has written a most interesting paper, in which he gives the history of several cases of paraplegia, the majority of which were supposed to be examples of caries of the vertebræ, but in which, on dissection, no disease could be discovered, either in the bones of the vertebral canal, or in the membranes or substance of the spinal cord. You will ask, were there no pathological phenomena in these cases? There were; but they belonged not to the spine or its contents, but to an organ in its immediate vicinity—the kidney. From a candid review of Mr. Stanley's cases, there appears to be reason to believe that disease of the kidneys may produce all those symptoms which have been attributed to lesions of the spinal marrow, or caries of the vertebræ. In the four first cases, the symptoms given as of caries of the vertebræ were present, and the cases treated as such. On dissection, no caries, or disease of the cord, could be discovered in any of them, but the kidneys were found to be the seat of extensive disease. The fifth case was a remarkable one; the patient had been admitted for retention of urine, the consequence of severe gonorrhœa, which had been checked by injections. The bladder and sphincter ani became paralytic, and he lost the power of the lower extremities to a certain degree. He also complained of severe pain at the fifth lumbar vertebra. He distinctly traced the pain from the bladder to the left kidney, and then to the right. Paralysis of motion, and, nearly, completely of sensation of the lower limbs,

next supervened, and in about a fortnight he died. On dissection, the kidneys were found in a state of inflammatory softening, and with numerous minute depositions of pus. The bladder was inflamed, but the brain and spinal cord were perfectly healthy. In the sixth case, a patient, while in progress of cure of a gonorrhœa with phymosis, was suddenly seized with paraplegia. The functions of the brain were unaffected. He had suffered for a day or two from pain in the loins. Sixteen hours after this attack, he suddenly died.

From considering the former cases, Mr. Stanley predicted that inflammation would be found in the kidneys. A slight turgescence of the vessels of the cord, with a little transparent effusion in the theca, were found, but the kidneys were in a state of the most intense engorgement. In this case, it was remarkable that, from the period of the paraplegia, there was an inordinate secretion of urine. The seventh case was that of a patient who, for two years, had been labouring under pain of the back, increased by pressure, and incontinence of urine. On dissection, there was some vascularity and effusion of the cord, but both the kidneys were almost entirely destroyed by disease. In addition to these, Mr. Stanley mentions four more cases, which were seen by a friend of his, Mr. Hunt, of Dartmouth, which corroborate his opinions.

Here, then, we may have well-marked paraplegia, without any perceptible organic change in the spinal cord, or its investments, but presenting distinct traces of disease of the kidneys. This leads us to observe the very close connection which exists between the kidneys and spinal cord, a connection which has been long recognised by medical practitioners, but only in a limited point of view; for though they were of opinion that disease of the kidneys, and a discharge of ammoniacal urine, were the results of spinal disease, they never seem to have reflected that the reverse of this might happen. It seems now, however, to be almost completely established, that disease of the kidneys may produce symptoms which are referable to disease of the spine; and Mr. Stanley has the credit of having been the first who directed the attention of the profession to this circumstance, and his paper must be considered as one of the most important, and practically useful, which has appeared for a length of time. You should all peruse it carefully. The fact that disease of the spine will give rise to affections of the kidney, is long known, and has been proved by numerous experiments. Thus Olliviet details the experiments of a German physiologist, M. Kreimer, who, by dividing the lower part of the spinal cord in animals, made the urine almost immediately ammoniacal. You will also find, in Dr. Prout's work, that an ammoniacal state of the urine may be rapidly brought on by injuries of the back, from falls or bruises on the spine. It is, indeed, singular how quickly those profound functional lesions of the kidneys supervene on injuries of the spine, sometimes appearing in four or five days, sometimes sooner. Medical men have hitherto been in the habit of looking at this matter only in one point of view; they know that disease of

the spine will produce disease of the kidneys, and here they stop; but it has been shown that the reverse of this may happen, and that renal disease may produce very remarkable lesions in the functions of the spine. Of this very curious occurrence, we have many analogies in pathology. Thus, for instance, in several cases of cerebral disease, but chiefly in hydrocephalus, we have vomiting; here we have functional disease of the stomach depending on an affection of the brain. Take the reverse of this—observe the delirium which attends a case of gastro-enteritis—here you have the functions of the brain deranged in a most remarkable manner, and this produced by sympathy with an inflamed mucous membrane. The truth is, that in the spine and kidney, as well as in various other parts of the system, we have two organs which are so closely connected by sympathy, that disease of one will bring on serious functional lesion of the other.

Observe, then, the great importance of these enquiries. When you meet with a case of paraplegia, you are not at once to conclude that it depends on disease of the spine, or caries of the vertebræ. You must carefully investigate its history, and ascertain whether it may be referred to either of these causes, or whether it may not rather depend on disease of the kidneys. That it may depend on the latter cause is now established, for the cases are too numerous for us to suppose the complication accidental. You will observe the importance of making an accurate diagnosis, when you consider that this point will most materially influence your treatment. In the one case, your treatment will be directed to the bones and cartilages of the spine; in the next, to the spinal cord itself; and, lastly, to the kidney, a parenchymatous organ, to which there is a great determination of blood. No one will venture to assert that the principles of treatment in each of these cases are the same; and the chances are, that, if you do not make a correct diagnosis, you will practise improperly and without success. I have now seen a number of these cases, but there were only two of this description in which I was fortunate enough to obtain a post-mortem examination. I cannot say that my dissection exhibited remarkable disease of the kidneys (they were large and very vascular), but, from the many points of resemblance they bore to Mr. Stanley's cases, I was led to conclude, that if they were not examples of actual chronic disease of the kidney, they were cases of lesion of function in the spine, unaccompanied by any organic change to account for the symptoms. I shall briefly detail these cases: the first was that of an unfortunate man from the country, who was discovered by two friends of mine under peculiar circumstances. While on an excursion, they were requested to visit a poor man who was lying ill at a remote farm-house. They heard he had been labouring under a dropsical affection for a long time, and had been treated for ascites. On arriving at the cottage, they found the man lying in bed, with his abdomen very much enlarged; and, on further investigation, discovered that he was quite paralytic of the lower extremities. On examining the belly more particularly, they found that the

swelling was produced, not by ascites, but by *an enormously distended bladder*. He had, also, stilloidism urinæ, with paralysis of the bladder; and this having been mistaken, by the medical practitioner who attended him, for suppression of urine, he had prescribed diuretics, and continued this plan of treatment for some weeks, totally overlooking the paralysis of the bladder. As little or nothing could be done for him in the remote situation in which he lived, it was determined to send him up by easy stages to Dublin, and procure him admission at one of the public hospitals. On his arrival, he was received at the Meath Hospital; and, when I visited the wards next day, I found that he was quite paralytic of both lower extremities, that the bladder was in the state above described, and that his health had suffered considerably, and that bed-sores had formed on his back, and were increased by his journey. I prescribed cupping and blistering, which were productive of some slight relief; but in the space of a few days he began to exhibit symptoms of low typhus, as if from the absorption of pus, and sank rapidly. On examining his body, we could not detect any traces of disease in the bones or cartilages of the spine; neither did the cord, or its membranes, present any marks of organic lesion, except that towards its lower portion, where it begins to spread out into the cauda equina, it was perhaps a little softer than natural. I regret very much that I did not note the circumstances of this case more fully; but, as far as my recollection of it goes, the general features were as I have just mentioned. I had another case, some time since, in the Meath Hospital, in which the following circumstances were observed:—The patient, a labouring man, generally employed about the quays, was brought into the hospital with paraplegia of some standing. The first symptom in his case belonged not to the spine, but to the urinary system; he had had an attack of gonorrhœa, for which he had used stimulants and balsams; and, in some weeks after, without any injury to the spine, he lost the use of his lower extremities. During his stay in the hospital, the urine was intensely ammoniacal. On examining his body after death, we could not discover any disease of the bones or spinal marrow. A layer of substance, resembling fat, or organised lymph, was found lying on the theca of the spinal marrow, but it was so very small as to be scarcely sufficient to account for the symptoms. The kidneys were pale, flabby, and without any vascularity, but did not present any marked traces of organic lesion.

Here, then, were two cases which, before the publication of Mr. Stanley's paper, would be considered as examples of organic disease of the spinal cord, or its investments; and yet, on dissection, we can find nothing to establish this opinion; and, in the last one, the first affection was of the urinary system.

Is it possible that a *functional* disease of the urinary system may produce also a *functional* disease of the spinal cord?

With respect to the diagnosis of caries of the spine, I wish to make a few observations. The diagnosis where there is distortion of the spine is extremely easy, but this does not hold where the

caries is accompanied by distortion. Let us enquire. Are there any circumstances which would enable us to arrive at the diagnosis of caries without distortion? One symptom, not observed, as far as I can see, in paralysis, *connected with disease of the kidney*, is, that the patient feels exquisite pain on motion. This is an exceedingly common symptom in caries of the vertebræ, but I am not aware that it occurs in cases where the disease is situated in the kidney, or the spinal cord itself. There is another remarkable circumstance:—When the patient attempts to move, he often feels a cracking sensation in the affected portion of the spine; and this has not only been observed by the patient himself, but is also perceptible to his medical attendants. When this occurs, it may, I think, be looked upon as a diagnostic symptom. The exquisite pain on motion, the tenderness of the spine on pressure, and the crackling sensation, these might be sufficient to make the diagnosis of caries of the vertebræ, even in cases where there was no distortion. But if you had a case of paralysis of motion and sensation of the lower extremities, and if these symptoms came on without any injury of the spine—if there was little or no tenderness on pressure—if the patient felt scarcely any pain in turning or moving, and if he had at the same time symptoms of disease of the kidney or bladder, and ammoniacal urine—under these circumstances the great probability would be, that it was not a case of caries of the vertebræ, or original disease of the spinal cord, or its investments, but a lesion of function of the spine, connected with organic or functional derangement of the kidneys. It must be acknowledged, however, that the diagnosis of this affection is rather obscure. The circumstances which I have just mentioned, might enable you to get rid of the opinion that it was caries of the vertebræ or organic disease of the spinal cord, and that it was probably such a case as Mr. Stanley has described; and if you could arrive at this diagnosis at an early period of the case, it would be a matter of great importance. By doing this, you would then be aware that you had to deal with an inflammatory affection of a highly vascular organ; you would not be led away from the real state of the case, or waste time in treatment calculated to stimulate the spine, or remove disease of the vertebræ. Your plan would be simple, and your treatment defined, and all your efforts would be directed towards removing the disease of the kidneys. You will easily perceive that diagnosis is here of vast importance; unfortunately, it is still involved in obscurity.

The prognosis of cases of paraplegia, when once complete paralysis is established, should be always unfavourable. The fact of paralysis occurring, is sufficient in itself to prove the existence of extensive disease in most cases. There may be, however, some cases susceptible of cure, and this particularly occurs in young females, in whom a perfect cure has been frequently accomplished by the use of stimulant embrocations to the loins. I have seen one case of this kind, in which the patient was paraplegic for a year and a half, cured by the application of hot oil of turpentine over

the lower part of the spine. Simple as the treatment may appear in this case, its success was rapid and complete. Mr. Crampton has mentioned to me the particulars of another case, in which the patient's limbs were quite rigid, and could not be moved without great difficulty; in this case, complete relief was obtained by applying Pearson's liniment over the lower part of the spine. This liniment produced powerful counter-irritation, and an eruption of bullæ over the body, which were speedily followed by relief. The patient is now in the enjoyment of perfect health; and since the period of her cure, which is now better than six years ago, has had no return of the disease.

LECTURE XXXIII.

Sudden paralysis from abscess of the brain—Curious case of paralysis without effusion—Previous symptoms of—Demonstration of the cellular tissue of the brain—Compressibility of the brain—Inaccuracy of the opinions of Drs. Abercrombie and Clutterbuck—Pathological states—Arachnitis without delirium—Traumatic apoplexy—Case of paralysis of the portio dura—Peculiar appearance of the affected side of the face—Use of the electro-puncturation—Bad effects from—Mechanical support of paralysed parts—Neuroses, active and passive—General pathology of—Principles of diagnosis—Case of neuralgic liver—Neurosis from moral causes.

Before I leave the subject of organic affections of the brain, I wish to exhibit a few preparations illustrative of some of the principal diseases dwelt on in the preceding lectures. You will recollect that, in a former lecture, I alluded particularly to the question, how far we are able to judge of the existence of apoplectic effusion by the *sudden* occurrence of an attack of paralysis. I endeavoured then to impress upon you that we may have sudden paralysis from other causes, as well as apoplectic effusion, and stated that there were numerous cases of sudden paralysis, with disorganisation of the brain, on record, depending, not on apoplectic effusion, but on circumscribed abscess of the brain; and that, consequently, the diagnosis of apoplectic effusion from *suddenness* of attack was only valuable when it came on unpreceded by symptoms of local disease of the brain. I alluded to a remarkable case of aneurism of the arteria innominata, in which the patient, soon after the date of his admission into the hospital, had become suddenly hemiplegic. This was a case in which one would be led to expect an effusion of blood into the brain, as the circulation of the head was evidently impeded by the pressure of the aneurismal tumour on the great veins, and as there was a remarkable distension of the jugular and other superficial veins. Of the existence of the aneurism there was not the slightest doubt; the tumour could be felt pulsating below and above the clavicle on the right side, compressing the trachea, so as to cause stridulous breathing, and producing a varicose state of the veins of the neck by its pressure. We accordingly

made the diagnosis of aneurism of the arch of the aorta, or of the arteria innominata. In this case two circumstances—the sudden paralysis of one side, and the obstruction to the circulation of the neck and head—would, as I have said before, lead to the supposition of an apoplectic effusion. On dissection, however, the paralysis was found to depend, not on this circumstance, but on the existence of a circumscribed abscess in the anterior part of the opposite hemisphere of the brain. I have the pleasure of exhibiting to you to-day this interesting and important preparation. It is too large to send round, but you can all inspect it after lecture. Here is the aorta from its commencement at the left ventricle—here is the enormous aneurism of the arteria innominata compressing the trachea—so that it has not only pushed it far to the left side, but has flattened it in such a manner as to produce a curious alteration in the appearance of its musculo-membranous structure. The terminations of the rings of the trachea are brought close together, and the muscular parts are folded in between them. There is another circumstance here deserving of your notice: the right carotid, you see, is obliterated; it is interesting to connect this fact with the absence of true apoplectic effusion. The case altogether is a curious one, and presents two remarkable circumstances—great mechanical obstruction to the venous circulation of the head, and sudden paralysis *without effusion*. It is, however, to be remarked, that in this case, though the paralysis was sudden and unexpected, it was preceded by some symptoms of local disease of the brain. The patient had pain in the head and limbs of one side, accompanied by a sense of formication. These symptoms were remarked some days before the attack of paralysis, but their importance was not at that time thoroughly estimated, in consequence of the greater share of attention being directed to the aneurismal disease.

In this bottle, you have a specimen of that species of ramollissement which supervenes on local inflammation of the brain. We have every reason, I think, to believe that when this disease occurs in the young, or in the adult, it is the result of an active inflammatory process; and that softening of the brain has in it nothing more specific or peculiar than softening of the liver or lungs from acute inflammation. Here, you see, is the disease—an irregular cavity filled with broken up cerebral matter, somewhat resembling cream in appearance and consistence. I may remark here, that it is in cases of this description that we are able to demonstrate the existence of the cellular membrane of the brain. This cellular membrane is extremely fine and delicate—so much so, indeed, that some anatomists of high authority have asserted that the brain possessed no interstitial cellular tissue. This preparation, however, gives a proof of the great light which pathology frequently throws on obscure points of anatomy and physiology; for, though the interstitial cellular tissue cannot be seen in the sound brain, we are able, in the preparation before us, to demonstrate its existence with certainty. It is, however, to be observed, that it is only in recent cases of ramollissement that this phenomenon can be

examined with advantage; for, in those of long standing, the cellular membrane shares in the general destruction, more or less, and gives way. But if you get a case where the softening is recent, and then take the softened portion of the brain and expose it to the dropping of a filter, you will find that the soft cerebral matter will be gradually washed away, leaving behind it a delicate tissue; and in this way you can prove the existence of cellular membrane in the substance of the brain, like that of other parenchymatous organs.

Here is a specimen of apoplectic effusion: see how extensively the substance of the brain has been torn; the cavity formed in this way is, you will perceive, filled up with a large clot. Now, there is one consideration which strikes us at once, in looking at an effusion of this kind into the substance of the brain, whatever may be its situation or extent—and this is, *that the brain must be a very compressible organ*. Here we see the brain torn, a cavity of large size formed, and this completely filled with blood. Now, it is obvious that the rest of the brain must give way, in order to give room for the formation of this cavity. If, then, it be true that the brain is compressible, so far as to admit of the formation of a large cavity, it necessarily follows that, contrary to the opinion of Drs. Abercrombie and Clutterbuck, the quantity of blood in the brain may vary, and be greater at one time than another. These authors think that the quantity of blood circulating in the brain never varies, but here, you will perceive, we have a remarkable cavity; and it is plain that the rest of the brain must have yielded before it could be formed; and it follows, as a natural inference, that the brain must be compressible, and that, consequently, the quantity of blood contained may vary at different times. It may be argued against this, that the illustrative proof in this case is derived from a pathological condition, and that, under such circumstances, the brain has room for the formation of a cavity, by the emptying of some of its vessels. Here, it is urged, is a cavity, but the emptying of the vessels of the brain compensate for it; thus room is found, and there is no increase in the quantity of blood circulating in the brain. This, however, I look upon as a mere *petitio principii*; nor have we any reason to think, that, in a case of apoplectic effusion, there is any corresponding emptying of the vessels, for dissection almost always shows a surcharged state of all the vessels. The result, then, in my opinion, is, that the brain is compressible, and may admit a larger quantity of blood at one time than it does at another. On this subject I advise you to consult Dr. Mackintosh's work on the Practice of Physic, and also the review of Dr. Clutterbuck's Essay on Apoplexy, in the London Cyclopædia of Practical Medicine, as given in the Dublin Medical Journal, Vol. II.

Here is another specimen. You see the brain, with a small clot in its substance about the size of a hazelnut. The patient in this case did not die suddenly; the clot has nearly lost the appearance of blood, and the processes of absorption were going on. I have

got here another preparation illustrative of this effect on one of those effusions. I told you, in a former lecture, that in these cases one of two things occurred; either the clot was wholly absorbed, and a serous cyst left in its place, or it was not absorbed, and became to a certain extent organised; and that this might be the history of many of the anomalous tumours we meet with in the brain. Here is a clot, fully as large as a walnut, in which no absorption has taken place, and you perceive it has been converted into a mass of a dense solid texture. You can easily conceive that the brain, having a clot of such a texture in its substance, could not easily recover its functions, and that the paralysis would be persistent. Here is another large apoplectic effusion, in which a certain degree of change has been produced; the clot, you see, has lost a good deal of its colour, and is not so red as in its recent state. Here is an excellent specimen of circumscribed abscess of the brain in the centre of one of the hemispheres. In this you can, to a certain degree, demonstrate the cellular membrane of the substance of the brain; but, if the preparation had been manipulated in the way I have mentioned, I have no doubt that it would exhibit it much better—that is, provided the abscess was of recent occurrence. If it should happen to be of long standing, the cellular membrane generally gives way, and you have nothing but a cavity filled with softened matter. This large preparation exhibits an enormous effusion on the surface of one of the hemispheres of the brain; it was the result of an injury of the head, and was accompanied by paralysis of the opposite side. It furnishes an example of what has been termed traumatic apoplexy. The other preparations on the table do not apply so immediately to the illustration of apoplectic effusions, and I shall pass them over.

Here is a preparation which I would draw your attention to, as it belongs to a very remarkable case. I mentioned before, that, in certain cases of arachnitis, where the disease was chiefly situated at the base of the brain, it had been observed that there was seldom delirium. In this case the patient complained of pain along the base of the skull, which occasionally remitted and then returned with violence, and it was at first supposed to be neuralgic. He continued in this state for some time, having a recurrence of violent pain in the ear, temporal and mastoid regions, which lasted for several days; when, all of a sudden, without any preceding delirium, he became comatose, and died shortly afterwards. On dissection, the arachnoid covering of the brain was found to be in a state of extensive disease over the inferior surface of the anterior lobe, and towards the posterior part of the base of the brain. Here we had a case of extensive and fatal arachnitis, with total absence of delirium.

When speaking of the employment of galvano-puncturation in the local treatment of paralytic affections, I stated that the case in which the most decided benefit was observed occurred in paralysis of one side of the face, apparently unconnected with actual disease of the brain, and of a local nature. This is in accordance with

what has been observed with respect to the efficacy of all local measures employed in the treatment of paralysis; and, accordingly, the more the affection is purely of the nerves of the part, the more satisfactory should be the results of galvanism. I shall read for you the notes of this case, and I am happy to have it in my power to lay before you two excellent drawings of the patient's appearance before and after the use of the galvanic battery, from the pencil of my friend, Mr. Berthon.

"A bricklayer, named T. Hogan, got an attack of erysipelas of one side of the face, accompanied by a feeling of pain and stiffness between the angle of the jaw and mastoid process of the right side. This was followed by an attack of paralysis of the right side of the face, and he presented himself for admission at the Meath Hospital in the following state. The features at the right side of the face are blank, unmeaning, and motionless, while those of the left side retain their natural cast, except that their lines are more strongly marked, and the angle of the mouth is drawn upwards and towards the left side to a considerable extent. The skin of the right side of the forehead is smooth, that of the left furrowed and puckered. The lids of the right eye are half closed, and he has not the power of moving the upper one. When desired to close his eyelids, the eyeball rolls upwards, and the transparent cornea is carried behind the curtain of the upper lid. By this movement the patient excluded all objects, and was under the impression that he had shut his eye. The lower lid hangs down, and is partly everted, exposing the conjunctiva, and allowing the tears to trickle down the cheek. When he breathes, the right cheek is puffed out; it becomes still more distended by an attempt at blowing; and, when attempting to drink, a quantity of the fluid escapes at the right angle of the mouth. On being requested to draw the right angle of the mouth towards the corresponding ear, not the slightest movement was made, except by the muscles of the opposite side. In masticating his food, the morsel gets between the cheek and gum of the right side, and he is obliged to put in his finger to dislodge it. Sensation on the paralysed side is unimpaired, and there is no deafness, alteration of taste, or loss of vision. With respect to the muscular functions of the tongue, they appear to have suffered no injury, and he can direct it with facility to either side. Here was a fact to show that the paralysis of the face in this instance had no connection with cerebral disease. In the majority of cases of paralysis of one side of the face, from disease of the brain, there is lesion of motion of the tongue, but here it was in its natural condition. There is a slight degree of thickness of speech, which can be remedied by supporting the paralysed cheek with the palm of his hand. He complains of a feeling of stiffness in the jaws, and cannot open his mouth more than what would be capable of admitting a teaspoon. He has had some tenderness over the mastoid process. He can press hard substances with his teeth; and the temporal, pterygoid, and masseter muscles seem to be as strong on the paralysed as they are on the sound side. He can move the

lower jaw so as to incline the symphysis to either side, but more to the left than to the right. His general health is good, and his bowels regular." I shall hand round the drawings of this case for your inspection. You will observe the peculiar appearance of the left side of the face, with the mouth drawn upwards, and the skin of the nose and forehead thrown into deep wrinkles. There can be no doubt that the exciting cause of the paralysis in this instance was connected with the erysipelas of the face. He had no symptoms of any cerebral affection, and the paralysis was limited to those muscles which are supplied by the seventh pair of nerves. The tongue was quite unimpaired in its motions, and there was no lesion of taste, hearing, or smell. It was, in fact, a case of purely local paralysis, and bore a decided analogy to those cases which have been so accurately described by Sir Charles Bell as depending on an affection of the seventh pair of nerves.

The treatment of this case was in accordance with the views already detailed; the diagnosis was paralysis of the seventh nerve, and the treatment founded on this diagnosis proved eminently successful. The first thing done was to apply a few leeches to the ramus of the jaw; we then used stimulating applications, and he used for some time the liniment. camphoræ compos. with extract of belladonna. After this he was put on strychnine, which did him some good; but there was so great a susceptibility of its action, that we were ultimately obliged to give it up. The last remedy employed was electro-puncturation, under the use of which he improved rapidly. On the 5th of March the galvanic battery was first applied; the needles at that time were inserted—one in front of the ear, and the other near the symphysis of the chin; subsequently, they were inserted in various parts of the right side of the face, following the different branches of the portio dura. On the first application of the galvanic influence, he had spasmodic twitches of the paralysed muscles, and soon afterwards he began to complain of a burning sensation in the cheek and pain in the head. Here, it would appear that headache was the result of the proximity of the stimulus to the brain. On the 11th, the symptoms were nearly the same, and his general health continued to improve. On the 15th, the application of the galvanic influence was followed by severe headache, which lasted for half an hour. On the 19th, his appearance was much improved, and the galvanic battery was not used. On the 20th, it was again applied, and in an hour afterwards he had rigours and slight headache. On the 21st, after using the battery, he had rigours again, followed by headache and a pricking sensation in the cheek. On the 24th, he left the hospital in a remarkably improved state. Expression had returned to the side of the face which had been previously unmeaning and blank; the furrows which had deformed the opposite side were removed, the thickness of speech diminished, and the paralysis of the buccinator had been so far relieved that he was able to manage soft articles of food without being under the necessity of removing them from between the cheek and gum with his finger. In this case we em-

ployed an adjuvant which should be mentioned: we supported the paralysed parts for some days with strips of adhesive plaster, to restore the position of the mouth. This was done on the principle recommended by Dr. Pemberton, in the treatment of paralysis of the fore-arm and hand from painters' colic. By applying strips of plaster near the angle of the mouth, and drawing them back and fixing them behind the ear, we endeavoured to counteract the preponderating antagonism of the muscles of the opposite side. For the report of this case, I am indebted to Mr. K. Ellison, of Liverpool, who had the charge of the patient in the Meath Hospital, a gentleman whose talents are only equaled by his untiring zeal in the study of pathological medicine.

The foregoing case is interesting in two points of view; first, as to its peculiar phenomena, and, in the next place, as to the success of local treatment. It also shows that we may go too far with electro-puncturation, particularly when it is applied to parts which are situated close to the brain. You recollect that, in Mr. Hamilton's case of amaurosis, three pairs of plates were capable of producing a degree of stunning and insensibility which lasted for some time. In this case the rigours and headache showed that the galvanic fluid had a powerful effect, and would lead us to be cautious in using it too freely, where the parts to which it is to be applied are situated in the vicinity of the brain.

We have now taken a very brief sketch of some of the most important organic affections of the brain; but, in the study of disease, we constantly meet with a vast number of cases presenting the most extraordinary nervous phenomena, and yet we are unable to discover, by the closest pathological investigation, any appreciable lesion of the nerves, spinal cord, or brain. These are the class of diseases which have received the name of *neuroses*. We find, in most of these affections, a remarkable alteration in the nervous functions without any perceptible or constant organic change; we find, too, that this alteration may be connected with an exaltation or a depression of the nervous power; and from this circumstance results the division of neurotic affections into active and passive—active where the nervous power is elevated, and passive where it is depressed. The spasms which accompany an attack of flatulent colic, the exquisite pain of tic douloureux, and the wild intellectual exaltation of mania, are examples of active neuroses. A patient in the second stage of painters' colic will have paralysis of motion and sensation of the fore-arm; there is here an obliteration, or at least a diminution, of the nervous function, and the disease furnishes us with an example of passive neurosis.

Of these two classes the active are certainly the most interesting in many points of view. We find, under the class of active neuroses, some of the most extraordinary diseases to which the human body is subject; all the different varieties of spasmodic affections—chorea, epilepsy, tetanus, hydrophobia, tic douloureux, hysteria, and a host of others, come under this denomination. It is a melancholy reflection then, that, in the present state of medical science, we are

not only ignorant of the ultimate cause of most of these diseases, but even of the causes of the variation in their phenomena.

You will recollect that, in a former lecture, I threw out a conjecture on this point, and stated that there might be changes of an organic nature connected with these affections, not appreciable by any mode of investigation at present known; and that it was possible that there might be a change in the nervous substance, quite independent of any addition or subtraction from the component sum of their organic molecules, but in all probability connected with a new and different arrangement of these molecules. The analogy in this instance is drawn from chemistry, and, I think, may obtain here, as well as in the phenomena of Isomerism in inorganic bodies. You are aware that many bodies, which seem to present exactly the same component elements, are found to be extremely different in their properties, and that this difference has been accounted for by supposing that it depends, not on any addition or subtraction of the component molecules, but on *some difference in their mode of arrangement*. Now, if this happens in the case of inorganic bodies, there is no reason why it might not also occur in organic substances; and, if so, we may understand why a state of the brain and nerves, which appears to us to be normal and healthy, may still be essentially different, and give rise to the most extraordinary phenomena.

I shall not detain you with any further remarks on this subject—it is too obscure to be treated of in a course of lectures on the practice of medicine; let us turn to the consideration of the actual state of our knowledge on the subject of nervous affections. In the first place, we know that in the neuroses there is no change discoverable which could account for the symptoms; and that, if we examine the nervous centres to explain the phenomena of paralysis in one instance, of epilepsy in another, of mania in a third, of hydrophobia in a fourth, and so on, the most minute investigations will not, in the majority of cases, point out any deviation from the healthy condition sufficient to account for the phenomena. We find, too, not only that the state of the brain does not present any constant difference in the foregoing diseases, but also that there is often not the slightest trace of any thing like inflammatory action—a fact borne out by the most extensive experience—and showing that treatment which would relieve ordinary cases of inflammation of the brain will here prove useless. There is one curious circumstance connected with these neuroses which you should be aware of, and this is—where the patient, after suffering from a nervous affection for a long time, dies, you may find organic disease on dissection; but here there appears reason to believe that organic changes of the cerebro-spinal centres, taken in the proper acceptance of the term, *are, in reality, the result of some state of these centres which existed previous to the attack, and was the cause of all the nervous phenomena*. We arrive at this conclusion for two reasons: first, because such changes are by no means constant;

and, secondly, because they are only found where disease has been a long time in existence.

Nervous phenomena, independent of organic lesion, have been divided into two classes—namely, neuroses of the nerves of animal, and neuroses of the nerves of organic, life. With respect to animal life, or the life of relation, we may have its neurotic affections of an active or passive kind; we may have pains, spasms, and exaltation of intellect, under the active form; under the passive, we may have extinction of muscular motion, sense, and the intellectual functions—the life of the individual being still preserved. With respect to the system of organic life, it would appear that, if we are to speak in general terms, we must admit that there is no passive paralysis of the nerves of organic life, they being liable to the active neuroses alone—for a passive neurosis of the ganglionic system implies death; yet, to a *certain degree*, as I have formerly stated, such a passive neurosis might exist in the visceral nerves. But we may have the phenomena of the active neuroses in all parts of the body, whether muscular or visceral. It is a singular fact, that in some visceral diseases we have signs of high exaltation of the nervous functions of the parts, in others not. Why is this the case? I think it must chiefly depend on the mode or degree of excitability of the brain, which is very different in different persons. There is no known organic difference between the gastritis with delirium, and the gastritis of a man in his senses; nor is there any difference between the hepatitis of a man of bilious, and the hepatitis of a man of nervous, temperament: and we have, in order to explain the variety of the symptoms, to refer to some original conformation or mode of excitability of the sentient centre. Whether this difference depends on an original organisation, or on excess or deficiency of parts, or on phenomena similar to those of Isomerism, we know not. As the result of experience, we are forced to admit that these phenomena have no necessary connection with the inflammation of the suffering part, or of the brain; and this proposition applies to the great majority of cases which are called nervous. Experience has also proved the truth of this from the results of treatment—for it has proved that the most successful treatment is that which is by no means calculated to remove inflammation (in its ordinary acceptation), either from the brain or from the suffering parts. The progress and duration of these cases, also, tend to prove the same thing; for, if we were to measure the degree of inflammation by the amount of pain suffered, it would be of an intense character and rapid fatality; and yet we find that, notwithstanding the violence of the symptoms, these diseases may go on for a number of years.

It is quite plain, then, that the pathological condition of a neurosis is not inflammation. Now, one of the most common mistakes in the practice of medicine is the taking these neuroses for cases of local inflammation; and this, I need not tell you, is frequently productive of most distressing consequences. There is one point connected with those violent nervous attacks which leads to a per-

sistence in this error ; and this is, that local antiphlogistic treatment gives temporary relief, although, in the majority of instances, this is of very brief duration, and the pain and other symptoms return, leaving the patient worse than he was before. From the fact of temporary alleviation following depleting means, however, the idea of inflammation gets into the practitioner's mind, and the patient himself is favourably disposed to that plan of treatment from which he has obtained a momentary relief. The consequence is, that a system of general and local depletion is continued, until a period arrives when the nervous excitability gets to an alarming height. Now, is there any circumstance, or class of circumstances, which would lead to the diagnosis of these affections? I feel certain that in many instances this must be a matter of some difficulty. By a careful study of the symptoms, however, you will, in most cases, be able to arrive at the truth. The first thing to which I would direct your attention, in studying the diagnosis of such cases, is, *the extreme violence of the pain*. Now, this intensity of suffering seldom occurs in cases of inflammation ; and it is a curious fact, that the most painful diseases are the non-inflammatory. The agonies of a patient labouring under neuralgia of the liver, or of the left side, or under tic douloureux, are dreadful ; the pain is far more intense than in any case of inflammation : and yet, notwithstanding all this excess and persistence of suffering, we do not see that the duration of life is necessarily curtailed. In the next place, you will observe that these attacks frequently recur, and that, though long continued and violent, they do not affect the patient's life, which would not be the case if they were connected with inflammatory action. If you add to all these circumstances a knowledge of the constitution, temperament, and habits of the patient, you will have still clearer notions. But there are other circumstances besides these to guide you. In the majority of cases, you will find that all the local and general signs of inflammation, with the exception of pain, are absent. A female, labouring under neuralgia of the liver, will have frightful pains in the right hypochondrium, and yet, if you examine her, you find that she has little or no hepatic derangement, no tumour in the region of the liver, no derangement of the digestive system, and (though the disease has lasted for years) no dropsy, and in many cases no appearance of jaundice. She has no fever, thirst, or loss of appetite ; her tongue is clean, her complexion clear, her stools natural, and her pulse soft. All these circumstances tend to show that, however violent the pain may be, it has no connection with inflammation. You will be assisted further in your diagnosis, by finding that the access and cessation of these attacks are equally sudden and unexpected—two characters which do not belong to organic disease. The quick supervention and sudden cessation of these diseases should lead you to suspect that they were not inflammatory. It may happen, in cases of inflammation, that pain may come on suddenly, and as rapidly cease ; but, though it may come and go in a brief space of time, still you will find that lesion of function or structure

remains. Thus, in a case of pleuro-pneumonia, the pain of the side may cease suddenly under treatment; but the stethoscope informs you that the layers of the pleura do not as yet glide freely on each other, and that there is some obstruction still to the free passage of air into the air vesicles. There is also another point. When we come to enquire into the exciting causes of these neuroses, we observe that they are most generally found to depend upon various circumstances connected with improper nutrition and with moral influences. Of these two classes of causes, the latter, though perhaps not the most numerous, are by far the most remarkable. A violent neurosis may be brought on in a single instant by moral causes. A nervous female, in apparent good health, may, from a sudden fright or fit of passion, get an instantaneous attack of neuralgia of the liver, and be thrown into a state of intolerable agony. Lastly, you will be greatly assisted in these cases by a knowledge of their history and *previous treatment*. What you will generally find is, that the patient has gone through a long and harassing course of general and local antiphlogistic treatment. The failure of this treatment will be of great value in guiding you to a correct diagnosis; and you will be further confirmed in this view of the case, by finding that the treatment which has the power of relieving or curing consists in improving the state of the whole system by the use of tonic, and, in many cases, of stimulant remedies. These observations will apply to almost all cases of purely nervous affections.

LECTURE XXXIV.

Principles of treatment of neuralgic affections—Connection with organic disease—Neuralgia of the liver—Treatment—Hemicrania—Treatment—Use of iron, quinine, and opium—Endermic method of using opium—Tic douloureux—Opinions of Sir C. Bell—Remarkable case related by—Inflammation of frontal sinuses—Violent symptoms—Mr. Crampton's treatment—Affections of the fifth and seventh nerves in cases of cerebral disease—Neuralgia of the side—Researches of Lombard and Brande on the effect of nitrate of silver—Injury to the skin.

To-day I purpose to speak of some of the general principles connected with the treatment of neuralgic affections; and here I beg leave to remind you, that we mean by neuralgic affections an exaltation of the nervous sensibility independent, *quoad* its production, of any organic disease *which we can detect* in the nerves, brain, or spinal cord. There is no proposition better proved than this, that the phenomena of nervous affections are not the same as those of inflammatory diseases; every thing tends to prove it, whether we look to the history and symptoms of the case, the results of treatment, or the appearances seen on dissection.

As the nervous system is diffused all over the body, and as there is no part of the system which does not, under certain circum-

stances, exhibit indications of sensibility, it follows that we may have neuralgic pains in any of the component tissues. Still, it is remarkable, that neuralgic affections are much more frequent in some parts than in others; and we find that, of all parts of the nervous system, the superficial nerves are those which are most commonly affected. With respect to the nervous affections of the viscera, we know very little as to their exact seat, but it has been generally observed that the pain is situated in the situations of the plexuses of the great sympathetic.

In entering upon the principles which should regulate the treatment of neuralgic diseases, I have to remark that they are but slightly modified by their situation; in fact, it may be stated generally, that the same principles of treatment apply, no matter where the disease may be situated. But are we to consider this subject as totally apart and having no connection with the occurrence of inflammatory or organic disease? If we did so, we should get but a limited and erroneous view of the matter. I have told you before, that, in long-continued cases of functional disease, organic alterations were very apt to take place. The reverse of this proposition also is true, that organic affections may precede an attack of nervous symptoms; in other words, you may have cases presenting, at first, phenomena amenable to antiphlogistic treatment, and yet a period will arrive when new symptoms occur, and this mode of treatment will be no longer applicable. This is of importance in the practice of medicine, for if, in such a case, you persevere in the use of depleting measures, you will affect nothing towards the removal of distressing symptoms, and may do your patient's constitution much injury. A common example of this is, where a person receives some local injury, as, for instance, a blow on the cheek. This is followed by all the symptoms of inflammation, as pain, swelling, heat, redness, &c. Well, then, you have a case of local inflammation to deal with, and you must treat it as such. But a period may arrive when, the heat, swelling and other symptoms of an inflammatory affection subsiding, the pain alone continues. Here the pain may be purely nervous; and if you were to go on leeching, purging, and depleting your patient, you would not only lose your time, but, in all probability, do mischief. Here, as in many other cases, we have local inflammation followed by an active neurosis. You remember, when speaking of hepatitis, I remarked that many persons were subject to pains in the region of the liver, *independent of any known organic disease*. I also drew your attention to the fact, that after the symptoms of hepatitis are removed, the pain sometimes continues, having no longer any connection with organic disease, and taking on the character of a neurosis. You will see of what importance this is when you reflect on the mischief done to such patients by persevering in bleeding, blistering, and the use of mercury, when the disease is amenable, not to this, but to a plan of treatment calculated to remove the neuralgia of the liver. It is the same thing with respect to the mammæ, injuries of which are frequently followed by severe neuralgic affections. In

the case of the heart, it sometimes happens that, after an attack of pericarditis, the patient will be subject to pain in that region, which may continue for years. Dr. Bright gives a very remarkable case of neuralgia which supervened on the disappearance of a cutaneous affection. All these facts tend to show, *that the mere pre-existence of local inflammation in any individual case does not prove that the pain is not neuralgic*, and hence it is plain that in such a case it might be improper to persevere in the treatment used for local inflammation. This persisting in the taking of a neuralgic pain for the continuance of inflammatory or organic disease is a common error, and often productive of the most frightful consequences. Without a careful consideration of such cases, you will fall constantly into error. Never forget that although neuralgia may be the first and sole affection, yet, that it is often combined with organic disease, which it may precede, accompany, or follow.

One of the most common forms of neuralgia which you will meet with in private practice, is what has been termed *hemicrania*, the chief symptom of which is violent pain in one side of the head and face. The symptoms are exceedingly violent; there is a high degree of exaltation in the sensibility of the surface of the face; the eye is exquisitely sensible to light, and the ear to sound. The patient is very much prostrated, and his spirits depressed, and the slightest cause is sufficient to bring on an attack of pain. In some cases the pain is constant, in some remittent, in other intermittent. The sensibility is deranged only at one side of the head and face, and the pain seldom extends beyond the median line. As far as we know of this affection, it seems to depend on some morbid state, either of the sentient extremities of the fifth nerve, or of that portion of the brain which receives its impressions.

In cases of hemicrania we may have symptoms existing elsewhere, and this leads us to the consideration of the exciting causes. These will be often found to depend on deranged digestion. Here the irritation is reflected through the sympathetic system to the brain and sentient branches of the fifth pair, for there exists between these two nerves a very close and remarkable sympathy. Thus we frequently observe, that *tic douloureux*, as well as hemicrania, are the result of some injury or irritation of those parts to which the ganglionic nerves are distributed. In treating a case of hemicrania, then, you must enquire whether there be any visceral irritation present, and remove it as soon as possible. You must also carefully inspect the teeth and gums, for a carious tooth or a diseased gum will prove the exciting cause of an attack. I have seen many cases of hemicrania where the patient was subjected to a variety of treatment without success, and where complete relief was obtained by the simple process of extracting a carious tooth. It is a very curious fact, that in those instances the pain was referred, not to the diseased tooth, but to the whole surface of the face. Cases of this kind are given in which the pain lasted for many years, resisting every form of treatment, and were afterwards cured by the extraction of a decayed tooth. There is one circumstance

in these cases which is very apt to mislead, and this is, that the pain is often not referred to the tooth; and relief, to a certain extent, is obtained by the use of narcotics and carbonate of iron. This, however, should not lead you to think that the disease has no connection with the state of the tooth and gum; and this fact is illustrative of a most important principle, viz., *that temporary relief by a purely anti-neuralgic treatment does not prove that no organic origin exists.* How often has hysteria depended on local disease, and the practitioner been misled by the temporary relief afforded by antispasmodics. I have seen the most melancholy examples of this, and have more than once been misled myself.

With respect to the remedies most generally employed in the treatment of hemicrania, they are chiefly preparations of arsenic, iron, sulphate of quinine, and opium. Of these, the recently precipitated carbonate of iron appears to be the best; indeed its success is frequently heroic. In proof of this you will find several very interesting cases detailed by Mr. Hutchinson in his excellent work. The best way of giving it is to combine it with an aromatic and a laxative; a small quantity of the pulv. cinnamomi comp. a few grains of rhubarb, and fifteen grains or a scruple of the carbonate of iron, will form a powder which may be given two or three times a day with advantage. It has been asserted, that the carbonate of iron is suited for fixed, and not for intermittent, cases of neuralgia; I have found the contrary to be the fact. I had lately a lady under my care, who, in attempting to remove some furniture, received an injury by striking her cheek against a chest. She was treated for six weeks with purgatives, local bleeding, and mercury; the swelling, heat, and redness of the part went off, but the pain remained, being regularly intermittent, and occasionally very severe. This lady was perfectly cured by a tonic regimen, and the carbonate of iron, in scruple doses, three times a day. The sulphate of quinine has been proposed as being peculiarly adapted for intermittent cases; it will sometimes succeed, but I have known several cases where it completely failed. I grant that the character of intermission would naturally induce a practitioner to have recourse to it, but I have known so many instances of its failure, in purely intermittent neuralgia, that I give a decided preference to the carbonate of iron; I recollect the case of a gentleman who for six weeks had daily attacks of terrible hemicrania. When the attack came on he was obliged to remain perfectly motionless, the tears streaming from the eye of the affected side, the ear was exquisitely sensible to the slightest sounds, and he remained in a state of intolerable suffering for some hours. For the space of six weeks he took quinine *in enormous doses* without any improvement, and was ultimately obliged to give it up as useless. I have seen the same result in a great many cases, and as far as my experience goes, I would not place much reliance on quinine, even where the attack was of a purely intermittent character. I have seen some cases, indeed, where it has done good, and you may try it; but if, after three or four full doses, you find there is no improvement, you may be

almost sure that it will prove useless. When it succeeds, one of the first effects produced by it is to put back the paroxysm for an hour or two, just as occurs when it is successfully given in a case of ague. But I feel certain, that if it is likely to succeed its beneficial effects will be seen in the course of a few days, and to persist for weeks in using it is not only unnecessary but improper. In the very remarkable case to which I have just alluded, the gentleman, after having tried quinine without the slightest improvement for six weeks, was suddenly and completely relieved by a full dose of opium. At night, on retiring to rest, he took a strong opiate, awoke in the morning refreshed and free from pain, and has continued from that time to the present (a period of ten years) without any symptoms of hemicrania. Dr. Mackintosh says that the sedative solution of opium, or the acetate of morphia, are the best remedies for this disease he is acquainted with, and that he has seen many cases where they succeeded, after every thing else had failed. You may also employ in such cases the external use of narcotics with great advantage, of which one of the best is the extract of belladonna. If you prescribe a liniment composed of a drachm of the extract of belladonna, with an ounce of the compound camphor liniment, you will have a powerful remedy, and one which, when applied to the surface of the affected parts, will often produce great relief. I have sometimes used the acetate of morphia in the endermic mode, by putting on a small blister, and leaving it on until vesication was produced, when the raised cuticle was cut away with a pair of fine scissors, and the surface dressed with an ointment composed of a drachm of lard, and from a grain to a grain and a half of the acetate of morphia. I remember two cases of intermittent hemicrania which yielded to this treatment. You will also frequently derive benefit from the internal use of stramonium and belladonna. There are many other remedies used for this purpose, but I shall not detain you any longer on this part of the subject; it will be sufficient to remark that the carbonate of iron, sulphate of quinine, and opium, externally and internally, are the remedies on which the most reliance is to be placed.

We have now to consider one of the most painful affections to which man is subject. This affection has been generally considered under two points of view, either as functional or organic. The functional, as far as we can judge of it, appears to be a pure neurosis; in the organic it is supposed that the disease is connected with an organic affection of some part of the brain; of these the first kind is that most commonly met with in practice.

Tic douloureux is one of the most melancholy and harassing affections to which the human frame is liable; in some instances the poor sufferer, after having lived for years in a state of exquisite misery, is at last worn out by the intensity and persistence of his agonies. Such was the fate of the late celebrated but unfortunate Dr. Pemberton. A great deal of light has been thrown on the nature of this affection by the researches of Sir Charles Bell. He seems to have succeeded in establishing several points connected

with the nature and seat of this affection, one of the most important of which is, that the seat of this disease is in the sentient branches of the fifth pair of nerves, and not, as it has been supposed, in the portio dura. He has shown pretty clearly that the portio dura is the nerve which regulates the muscular motions of the face, producing all those modifications of features which we call expression, and also peculiar motions or changes connected with certain states of respiration; in a word, that it is the expressive and respiratory nerve of the face. It is, according to him, never the seat of tic douloureux, and the practice of dividing it for this complaint is as unscientific as unsuccessful. The division of the portio dura in such cases, not only fails to give relief, but also entails disgrace on the practitioner, and disfigurement and misery on the patient. *Its effect is paralysis of the muscles of one side of the face, and great distortion*, without the slightest relief. Yet it is a melancholy fact that such operations have been performed. Sir C. Bell's researches, however, have put an end to this malpractice, for he has shown that the fifth nerve is that which supplies the face with sensation, and that it is in its branches the disease is situated. We are then, I think, to look upon this disease as a neurosis situated in the expansions of the facial branches of the fifth pair of nerves. Sir C. Bell relates a very remarkable case, in which the patient had suffered from a series of dreadful attacks, the pain coming on in violent paroxysms. From the accounts given by this patient, and from personal observation, he says that one could trace with anatomical precision the course and direction of the branches of the fifth nerve, for, on the recurrence of an attack of pain, he applied his fingers to his face, and made pressure on the foramina, where the different branches of the fifth nerve issue. Having done this, he would press the nerves with all his force, and remain in a fixed posture while the paroxysm continued.

Sir Charles Bell's idea with respect to the cause of this disease, is, that it generally depends on some visceral irritation reflected through the sentient branches of the fifth pair of nerves.

I have told you that this disease is one of the most melancholy affections to which man is subject, it is also one of the most obstinate. A vast number of remedies have been employed or proposed for its treatment, and this affords an illustration of the fact, that the more incurable a disease is, the more extensive is the list of its remedies. A few only are deserving of attention, and these I have already mentioned when speaking of hemicrania, namely, the preparations of arsenic, iron and quinine, and opium. Where these fail after a full trial, Dr. Bright looks upon the case as hopeless. Narcotics in every form and of every description have been employed, both externally and internally, but to all these the same remark applies; many of these remedies will give temporary relief, and the physician will flatter himself on the prospect of a favourable termination, but in a short time he is annoyed at finding that the disease has returned and left the patient as bad as ever. Many a time have I seen a poor sufferer excited by hope on receiving tem-

porary alleviation from the use of arsenic or iron, and sinking into despair when he found that his torturing malady returned, and that the remedies which on the first trial gave relief on the second proved useless. The general principles which should guide you in your treatment are, first, to investigate carefully whether any visceral irritation exists, and remove it as far as possible, taking care at the same time to improve the general state of the patient's health; and the next thing is to allay the sensibility of the nerves of the part, and avoid all exciting causes. In certain cases this disease appears to be connected with an affection of the brain, and this seems to be an explanation of the fact before mentioned, that in some cases, where all specific treatment had completely failed, relief had been obtained by shaving the head and applying ice to the scalp during the paroxysms. I have already mentioned to you a case in which this mode of treatment proved eminently successful. This is a curious fact, and one which, being of practical importance, you should hold in memory.

We have a form of disease consisting of violent paroxysms of pain, apparently nervous, and in which no doubt the branches of the fifth pair of nerves are engaged; it is generally found to depend on a local cause, being connected with some disease of the bones of the face or skull, and bears a close analogy to *tic douloureux*. I have now witnessed several instances of this disease; in some cases it is produced by a carious tooth, in others by disease of the maxillary bones, and I have observed it to occur in one case of abscess of the antrum. The same thing has been observed by Dr. Bright, who gives a case in which the extraction of one of the bicuspids was followed by a gush of matter from the antrum and complete relief of the violent pain. I have also seen cases in which this affection appeared to be the result of disease of the lining membrane of the frontal sinus, of this also Dr. Bright gives an example. The case I witnessed was that of a lady who got a dreadful attack, resembling hemicrania, in consequence of being exposed to cold shortly after leaving a warm climate. She suffered the most violent agonies for some time, until one day she had a discharge of purulent matter from the nostrils, which was almost immediately followed by relief. This has recurred at intervals since that period, the pain ceasing when the discharge comes on, and returning when the discharge goes away. The pain is most intense, and situated in the direction of the frontal sinus, and running down along the side of the face; it is constant, and without any intermissions, returns upon the occurrence of any cause which checks the discharge, and is sometimes so excessive as to render her quite frantic. Whenever an attack comes on she applies a number of leeches over the frontal sinus, then warm fomentations, and this has the effect of bringing on the discharge and giving relief. In a conversation which I had with Mr. Crampton on this case, he stated to me that he had met with two similar ones, and that he had succeeded in accomplishing a perfect cure by inserting a large caustic issue over the top of the head. I accordingly advised my patient to have the

same thing done. She has since that time left the country; but previous to her departure I certainly observed an improvement in her symptoms, and the principle of treatment appears to be perfectly rational.

This leads us to consider some affections of the sentient and motor branches of the fifth pair of nerves, in which the disease is connected with an affection of the brain. A very interesting and important case bearing on this point is given by Sir Charles Bell, which I shall briefly relate. The patient, a lady, had remarked, that for twelve months before the case began to assume a serious character, she felt an unusual sensation in the tip of the tongue, towards the left side, as if it had been burned. This sensation gradually extended over the whole of the left half of the tongue, the left half of the palate, gums, and face, accompanied by an almost total loss of proper sensation in the parts affected. The sensation of heat and uneasiness was increased by the least motion of the face, the application of her hand, and other trifling causes. This case was communicated to Sir C. Bell by Dr. Whiting, under whose care it was. She had paralysis of the buccinator of the affected side, and the morsels of food had to be removed on that side with the finger, so that she was obliged to perform mastication with the opposite jaw alone. The motions of the face, however, were properly performed, showing that the functions of the superficial branches of the portio dura were unimpaired, and the temporal and masseter muscles continued in their natural state. Her general health also was pretty good, and she complained of nothing but the affection of the side of the face, tongue, and palate, and the impossibility of masticating her food with comfort on the left side, in consequence of the state of the buccinator. Some time afterwards, while engaged in eating, she found that a new train of symptoms were in progress; her face became distorted by the retraction of the mouth to one side, the masseter and temporal muscles of the left side ceased to act, the tongue became protruded, with its tip directed to the left side, hearing ceased on the same side, she had some difficulty in performing the motions of the eye, and the eyeball began to waste and diminish. About a month before her death she became quite stupid, and spoke very indistinctly. She died after the disease had continued for two years.

Here was a case, presenting in the first instance symptoms of a nervous affection of the left side of the face, tongue, and palate, unaccompanied at that time by any paralysis of the muscles of the face. About a year afterwards, however, she began to exhibit symptoms of paralysis of that side affecting those muscles which are supplied by the branches of the portio dura and fifth nerve. Expression was now lost, the temporal and masseter muscles ceased to act, the mouth was drawn to one side, and the tongue protruded. In addition to this the sense of hearing on one side was lost, and the globe of the eye began to waste. On dissection, it was found that a tumour, appearing to be a morbid growth from the left crus cerebri, about the size of a pigeon's egg, and containing some fluid,

was situated over the left temporal bone. This production was partly cellular and partly membranous. But the most interesting part of the case was the examination of the state of the nerves. The first and second nerves were undisturbed, and so was the fourth. The third was slightly displaced; but it was on the fifth that the principal impression seemed to have been made, for it was flattened, thin, and wasted, as if from the direct pressure of the tumour. The sixth nerve was uninjured. The seventh was involved and lost in the tumour, from within a quarter of an inch of its origin as far as the meatus auditorius internus. Here is a drawing of the case; here is the fifth nerve flattened and wasted, and here is the seventh involved in the tumour.

Mr. Stanley gives a case very similar to the foregoing, of which I shall give you an abstract. The patient had hemiplegia of the left side, without loss of sensation in the affected arm or leg, but in the left side of the face there was a complete loss of sensation and motion. The loss of sensation and motion in this case would argue that there was an injury of the seventh nerve as well as the fifth. The mucous membrane of the left nostril was red, and there was opacity and disorganisation of the cornea of the left eye, with total loss of hearing on the same side. The patient died some time after the paralytic symptoms were established. On dissection, a tumour was found lying close to the tuber annulare, and compressing the fifth and seventh nerves.

Here was a case in which there was hemiplegia of one side, and complete loss of motion and sensation in the corresponding half of the face, with an erysipelatous redness of the nostril, inflammation of the conjunctiva, and disorganisation of the cornea. It is a curious fact, that in cases where the sentient branches of the fifth nerve, which are distributed to the face, become affected, the eye is frequently disorganised. The cause of this appears to be that the eye, under such circumstances, loses the sensibility of its external surface, which is supplied by the branches of the fifth pair, and is intended to obviate and guard against all causes of irritation, and is consequently left in a state in which it can no longer protect itself from external injuries. In a case of this description which came under the notice of Mr. Crampton, the finger could be rubbed over the eyeball without giving the patient any pain, and there was chronic inflammation of the conjunctiva.

The principles which should guide us in the treatment of neuralgic affections of other parts of the body are the same as those which have been laid down in speaking of the neuralgic affections of the face. You will often meet with affections of this nature in females: they are situated generally in the right or left side, and are frequently, I regret to say, mistaken for cases of local inflammation. I have already dwelt on the disastrous consequences of mistaking a neuralgia of the liver for hepatitis, and showed the mischievous consequences of treating it with purgatives, leeching, blistering, and mercury. There is an analogous affection of the left side, which has frequently been mistaken for disease of the

heart, and treated accordingly. It is most commonly observed in females of a nervous habit. To this affection the same principles will apply as to hepatic neuralgia; by regulating the patient's general health, prescribing a mild nutritious diet, giving up all anti-phlogistic measures, and the judicious employment of tonics and narcotics, you will be able to effect a cure.

It has been lately proposed to use the nitrate of silver in the treatment of cases of this description, from its success in epilepsy. A very interesting memoir on this subject has been recently transmitted to me from Paris by Dr. Lombard, in which he dwells on the utility of the nitrate of silver in several nervous affections. Some persons, but in particular the disciples of the physiological school, think that nitrate of silver relieves cerebro-spinal irritation by creating a new irritation elsewhere; that its efficacy consists in its causing a revulsion of the gastro-intestinal mucous membrane; and that thus we cure an epilepsy by substituting a gastritis. In proof of this they bring forward cases where a chronic gastritis was found to supervene on the removal of an epilepsy by this remedy. This, however, is by no means a fair or logical deduction. The epilepsy might have been preceded and produced by the chronic gastritis, though the symptoms of the latter were not recognised, owing to the existence of other symptoms of a more prominent and striking character. The gastritis might have had a priority of existence, and might have been the cause of the epilepsy; the epilepsy might be cured, and the patient die afterwards with symptoms of chronic gastritis. This shows you how cautious you should be in receiving, on medical subjects, the *post hoc ergo propter hoc* argument. This mode of explanation of the cure of one irritation by the substitution of another, sprung from the denial of all specificism, in disease and its remedies, by the school of Broussais, one of the greatest errors of the "*physiological doctrine.*" The use of mercury in syphilis, of bark in ague, and many other instances have been quoted against it. If in these diseases there be nothing but local irritation, why does not ordinary antiphlogistic treatment always suffice for their removal? Why is it that mercury is the best revulsive in syphilis? The specific character appears under this view, as well as under any other. The term specific may be objectionable as not being precise, but we use it for want of a better, and it rather expresses what the disease is not than what it is.

There is another and a more rational objection to the employment of nitrate of silver; namely, that it has produced a blackening or discoloration of the skin. This, in my opinion, is an objection which will always weigh against the use of the remedy, for there are few who would like to encounter the risk and consequent blame of such an event. It has not been proved that nitrate of silver has cured epilepsy by superinducing gastritis, but it has been proved that it may blacken the skin. Dr. Lombard admits that this may and does occur, but he thinks the frequency of its occurrence much overrated, and states that in the majority of his cases it did not happen at all. He mentions a very interesting fact connected with

this subject. It has been supposed that exposure of the skin to the influence of sunlight during the use of the nitrate of silver is the cause of the blackening. Now Dr. Lombard says that this cannot be the case, for one half of his patients were peasants who worked in the open air, and never took the slightest precaution against exposure to the sun's rays; and yet, among them all, there was no instance of discoloration. He is of opinion, therefore, that the influence of the sun's rays should not be taken into account in a case of blackening of the skin; and this appears to be confirmed by the fact, that in all cases where the nitrate of silver produced discoloration, the patients were inhabitants of towns, and consequently less liable to exposure. This blackening of the skin, though a rare circumstance, will, as long as we are ignorant of the causes which produce it, and the means of controlling them, be a great obstacle to the internal use of nitrate of silver. I have used this remedy in cases of epilepsy and other diseases, and cannot say much for it; in the hands of some of my friends, however, it has been much more successful. Dr. Lombard thinks very highly of its value. In some cases in which he prescribed it a perfect cure followed, in others more or less relief. He gives some cases of facial neuralgia, in which it appears to have produced a cure. He has also prescribed it with success in epilepsy and chorea.

There is one fact, which appears to show that the cause of the blackening of the skin is connected with something else besides the influence of the solar rays, which I had almost forgotten. In a late number of the *Quarterly Journal of the Medical Sciences*, Mr. Brande gives an account of some experiments he made on the bodies of persons who were tinged by the nitrate of silver. He found on examination *that the deep-seated parts were tinged as well as the superficial*, and was able to detect the oxide of silver in the bones, and even in the substance of the viscera, as well as in the skin. If this be the case, we cannot attribute the discoloration to the solar rays, though it generally happens that the face appears to be darker than other parts of the body in persons who have undergone this change of colour. The fact, however, that in Dr. Lombard's cases the peasants escaped while the citizens became tinged, and Mr. Brande's discovery that the deep-seated parts are equally liable to discoloration, furnish a weighty objection to the opinion that the blackening of the cuticle is produced by the decomposing power of the sun's rays.

LECTURE XXXV.

Scrofula; former opinions on—White and red capillaries—Division of the system into red and white tissues—Vascularity of the white tissues—Dr. Graves's views of the lymphatics—Analogies of lymphatics and veins—Meckel, Cruikshank, and Magendie's opinions on—Relations of the circulating and nervous systems—Vitality of serous membranes—Reproductive power of white parts—White blood and white tissues more prevalent in women than men—White tissue more liable to cancer, &c.—Analogy of the white parts with cold-blooded animals—Increased sensibility of white tissues—True nature of the scrofulous diathesis—Reference to arrest of development—Explanation of its phenomena.

In the varied catalogue of morbid affections to which man is liable, there is scarcely one of such paramount importance, such engrossing interest, as scrofula—whether we look to the obscurity of its origin, its insidious progress, the number and variety of the organs which it attacks, or its remarkable intractability and extensive fatality. It is, indeed, a subject of deep concern to every one who is engaged in the pursuit of medical knowledge; and I do therefore entreat your undivided attention while I endeavour to give you some clear ideas as to the meaning of what has been termed the *scrofulous diathesis*, and scrofula itself.

It is now generally admitted, that a great proportion of our improved knowledge on the subject of scrofula, as well as many other diseases, has been the result of those splendid anatomical and pathological investigations which have distinguished modern times. The older authors knew little of pathological or comparative anatomy; and hence it was that scrofula, on which pathology has shed such a broad and searching light, was to them an intangible essence—something which they knew to exist, but could neither portray nor define. If we look to what their opinions were on this subject—opinions which, I regret to state, are not yet sufficiently exploded—we shall find that they are based on the then prevailing doctrines of exclusive humoralism; and that, instead of attempting to reduce the phenomena of scrofula to a fixed and tangible formula, they sought to explain it by referring to certain peccant and noxious humours in the system. But, in order to arrive at accurate notions on this subject, we must begin with the first formation of the human body; we must trace scrofula back to its primal source, and carefully explore its anatomical constitution.

If we examine the capillary circulation in its physiological state, we shall find two kinds of circulating fluids: one distinguished by its red colour, and called *blood*; the other transparent and whitish, or colourless, and termed *lymph*. In fact, we have two kinds of capillaries—one containing fibrin and colouring matter blended with an albuminous fluid, the other circulating only a colourless fluid containing little or no fibrin, and almost identical with serum. This is a fact which is now generally admitted. It has been supposed that the red and white capillaries differ only in point of size; and this is rendered probable by the fact, that, in cases of inflammation, vessels which previously contained only a colourless fluid

become dilated, and are rendered capable of transmitting red blood. This goes very far in support of the doctrine, that the red and white capillaries differ only in respect to size. Let us take a few out of many examples of this kind. The circulation of the serous membranes in their healthy state is entirely white, but in an inflammatory condition we can, with the greatest facility, trace numerous red vessels ramifying through their substance, as you may observe in inflammations of the arachnoid, pleura, peritoneum, and other white tissues. You may see the same also in the case of a mucous membrane—as in that of the conjunctiva where it passes over the transparent cornea. This condition subsides with the disappearance of the inflammatory action.

These investigations, however, as to the cause of the difference between the red and white capillaries, are not of great moment in a pathological point of view; it will be sufficient for our purposes merely to admit this difference, and bear chiefly in mind the relative compositions of the fluids which circulate through them. One of these, as has been already stated, is called *blood*, and contains a quantity of fibrin and colouring matter; the other is termed *lymph*, and is chiefly composed of water and albumen: the former is characterised by the presence of fibrin, a highly animalised product as containing a large proportion of azote; the latter consists of materials of an inferior degree of animalisation, and in which we can scarcely detect the existence of azote. Recollecting this remarkable difference in the nature of the circulating fluids, we find, when we come to examine the solids of the body, that some organs are supplied with red blood, while others receive only white blood, and hence the grand physiological division of the body into *red and white tissues*: the red containing fibrin and colouring matter, and endowed with great vitality; the white containing chiefly albumen, and possessing vital power in a comparatively weak and imperfect degree. To give you an example of this, let us take the muscular fibre, (which may be looked upon as the most highly vitalised of animal products,) and we shall find that its tissue is red, it is supplied by red vessels, and exhibits an acute sensibility to external as well as internal stimuli. On the other hand, we observe that the white tissues—such as cartilage, tendon, and serous membrane—have a white and albuminous fluid circulating through them; that they are of an inferior organisation, and of a lower degree of vitality; and that, in a state of health, they are almost insensible to ordinary stimuli.

In order to prove the close connection which exists between the white tissues and a white circulation, it is necessary that we should admit that these tissues are vascular; and on this point, it must be confessed, there is some difference of opinion among physiologists. Some say that the serous membranes and other white tissues are not supplied with vessels and nerves like other parts of the body, but that they are to be considered as a mere exudation. This is the opinion of Rudolphi. This notion, however, does not appear to be founded on truth; it has been disproved by the investigations

of many eminent physiologists, but by none more than by Dr. Graves in his excellent lecture on the lymphatic system. Let us enquire, briefly, whether it be true that these tissues are supplied with vessels containing white blood. That such is the case appears from the fact of the *sudden development* of red vessels in those tissues when in a state of inflammation. Now, it cannot be supposed that an irritation which has lasted only for a few minutes could be capable of forming new vessels. That vessels already formed might become dilated in a very short space of time, we can easily conceive; but that they should start into existence in the space of a few minutes, is totally incomprehensible. The sudden appearance of these vessels does not by any means prove that they are new creations; it rather tends to show that they must have existed previously to the attack of inflammation which rendered them visible, and that they escaped observation before that occurrence, from their extreme minuteness and from the colourless nature of the fluid they contained. The next thing to be adduced in support of this view of the question is this: that parts unconnected with any red tissue shall themselves become red under the influence of the inflammatory process. Thus, in a case of pleuritis, for instance, we observe that the lymph which has been effused becomes gradually organised, and ultimately converted into the serous membrane, forming those bands of adhesion with which you are all familiar. At certain periods of this process, vessels carrying red blood may be distinctly seen shooting through the lymph, which, you must bear in mind, is surrounded on all sides by the original serous membrane. These vessels are sometimes of great size; I have seen them, in a case of peritonitis, as thick as a crow-quill.

As the organisation advances to completion, we find that these vessels disappear, and a colourless serous membrane is formed. Now, here we have a newly organised tissue, presenting the same characters as serous membrane, and having no connection whatever with any red tissue; and yet it is not less curious than true, that, if this new tissue be attacked with inflammation, it will become distinctly red, and will have red vessels developed in the substance which may be examined and traced with the naked eye. The red vascularity of the synovial membrane, when inflamed, may be also quoted in proof of the same fact; for this, you will recollect, is a white tissue, super-imposed on other white tissues, the cartilage and capsular ligament. In addition to this, we have an argument drawn from the great similarity which exists between the serum of the blood and lymph. Dr. Graves believes that the lymphatics, which are supposed to act merely in carrying back the debris of the system, really enjoy a higher function; that they are to be regarded as the veins of the white tissues, or, in other words, that they are to the white arteries of the white tissues what the veins are to the red arteries of the red tissues. He brings forward many striking analogies between the veins and lymphatics. Both have valves, the effect of which is to direct the contained fluids towards the centre of the circulation; in both the fluid flows with an

equable current toward the heart, and in both its course is directed toward the organ where it is destined to undergo the process of aëration. Dr. Graves quotes Cruikshank and others to show that lymphatics, coming from a white organ or tissue, become capable, in the inflamed state of that organ, of carrying red blood; and this is a strong argument in proof of his opinions. We find, too, that the white blood, like the red, is separable into a watery portion and a crassamentum. If any thing else were necessary to show the close connection between the veins and lymphatics, it is furnished by the fact of their numerous anastomoses—a fact which has been placed beyond all dispute by Meckel, in his magnificent work on the lymphatics, which he has dedicated to his illustrious countryman, Soemmering. Lastly, it has been shown, by Magendie, that the veins and lymphatics discharge a similar function—both being engaged in carrying on the process of absorption. Both, then, have a similarity of structure; in both the contained fluids flow in an equable current toward the heart, and are destined to be submitted to the same process of aëration; both contain a fluid separable into a clot and a watery portion; under the influence of the inflammatory process, the one carries red blood as well as the other; they are intimately connected with each other by innumerable anastomoses, and both are alike engaged in discharging the functions of absorption.

It may be admitted then, I think, that the white tissues are vascular, and that the lymphatics are the veins of the white tissues. Now, pursuing this investigation further, if we examine the different organs and parts of the human body, we find that most of them are composed of a combination of red and white tissues, but that some consist of white tissue alone. Thus, if we look to the constitution of muscular substance, we find that it is composed of muscular fibre which is a red, and cellular membrane which is a white, tissue. In the parenchymatous organs we find the same combination of red and white tissues present. But, on the other hand, we observe that serous and synovial membranes, ligaments, tendons, aponeuroses, and cartilages, have no mixture of red tissue in them, and are composed of white tissue alone. The fact is, that both enter into the composition of most of our organs, but in some the white tissue prevails exclusively. The same conformation is observed in all the vertebrated animals, but particularly in the mammalia and birds. Now, admitting this, it appears that the proportion of red solids is in the ratio of the red fluids, and that of white solids in the ratio of the white fluids. This proposition we can establish by a number of indisputable facts. In the higher classes of the mammalia, the red solids and fluids are found in great abundance in the viscera, as well as in the muscular parts. In birds the quantity of red blood is great, and consequently we find the muscular substance red and firm, the circulation active, and the temperature high. In the amphibious animals the flesh is paler, the quantity of red blood diminishes, and, as the red tissues disappear, the white increase in proportion. Descending in the scale

we come to fishes: here we find that the quantity of red blood is very small, and that the white tissues predominate. When we arrive at the invertebrated animals, or those who have no spinal system, we observe a disappearance of the red fluids and red tissues. I shall here beg leave to read for you an extract from Dr. Graves's lecture. "The view already taken of the intimate connection, in all the different classes of animals, between the development of the white parts and that of the lymphatic system, is easily explained on the supposition of the lymphatics being the veins of the white parts. In invertebrated animals, which have no red blood, it would be more rational to call the vessels conveying the white blood back to the heart lymphatics than veins; for it is more consistent with analogy to suppose, that, in the lower animals, the retained portion of the circulating system corresponds with the former, as in the superior animals the lymphatics are connected with parts which, in their degree of vitality, most resemble the structures of the lower animals.

"In the higher classes of animals there are not only two circulating systems—one of red blood, and the other of white blood—but also a twofold system of nerves, the cerebral and the ganglionic; the latter of which, in invertebrated animals, seems to perform all the nervous functions necessary to their state of existence, while these animals are remarkable for possessing only a simple vascular system.

"We find, therefore, a correspondence between the vascular system of red blood and the nervous system of the brain and spinal marrow. They are most perfect in animals most abounding in highly aerated red blood, and decrease according to the descending scale proportioned to the decrease of red blood, until at last we arrive at the invertebrated animals possessing no red blood, and no brain or spinal marrow."—*Op. Cit. p. 24.*

These facts seem to establish the law, that the proportions of red and white solids are in the ratio of their respective fluids. But let us consider this question in a different point of view. The human fœtus, in the earlier periods of its existence, presents the appearance of a white mass without any red tissue or red vessels—in fact it resembles one of the lower or white-blooded animals; but, as the process of development goes on, the red fluid begins to appear, and the red tissues increase; as it progresses towards perfection, these changes become more manifest, until at length, having arrived at the summit of the zoological scale, the predominance of the red tissues is established, and even the new-born infant exhibits the distinguishing characteristics of that being which holds the first rank in the order of vertebrated animals.

We have seen that, in proportion as the animal rises in the scale of being, the proportion of the red to the white tissues is increased; and that the lowest animals, who possess only a white circulation, enjoy a degree of vitality not far removed from that of the vegetable kingdom. Red blood, then, is the *pabulum vitæ*—the characteristic of superior organisation and vitality—the rich stream by

which the nobler parts of our system are nourished. Applying this to the different states of our bodies in health and disease, we find that the predominance of red blood and red tissues is a proof of health and vigour, while that of white blood and white tissues shows the feeble and unhealthy individual. Before we pursue this further in its application to the investigation of disease, let us refer for a moment to some illustrations. In the Albinos, the white tissues are more abundant and the quantity of red blood smaller; the muscular fibres are lax, and the constitution delicate. Again—compare woman with man. In the former the white tissues are more prevalent than in the latter, the skin is fairer, the vessels carrying red blood smaller, and the muscular system less developed. Women have more white blood and white tissues, and consequently their strength is less, and their constitution more delicate. Again, if we examine the hybernating animals, who pass long periods of time without taking food, we find that, at the termination of their hybernating season, their strength is remarkably reduced, and their tissues much paler than under different circumstances. The same diminution in the quantity of red blood and red tissues, and corresponding increase in the relative quantity of white tissues, occur in the case of persons advanced in life. From some cruel experiments made in France, it appears that, by starving animals, their tissues became blanched in a remarkable degree, and they came to represent animals of an inferior grade, so far as the preponderance of white tissue was concerned. These considerations are highly interesting and important, and remind us of Lord Bacon's aphorism, that *white is the colour of defect*.

We may, then, conclude that the white tissues are less highly animalised than the red, and of a lower degree of vitality; and, arguing *à priori*, we would say that they are less able to resist death, and that in them disease would be slower and more obstinate than in the red tissues. Such, too, is the fact, for we find that the white tissues are most liable to morbid affections of an intractable character, frequently terminating in the total destruction of the diseased parts. Cancer, tubercle, ulceration in the cartilages, ligaments, or cellular substance—all these belong to the affections of white tissues. All these terrible inflictions are to be met with in those tissues which rank low in the scale of vitality; they occur in persons of weak habit and diminished vital energy, and in whom the white tissues predominate over the red; and they are less under the influence of those curative means which are ordinarily employed in the treatment of diseases of the red tissues. Thus we find that many of the means, which are employed with such good effects in dispersing inflammations of red parts, seem to possess very little power in removing the diseases of white parts. In making these observations I have spoken only in reference to chronic disease; but let us turn to the acute diseases. Here we arrive at a very curious fact. If we take, for instance, the serous membranes, we shall find that, although strictly belonging to the class of white tissues, they are, nevertheless, very liable to acute and violent

diseases. It appears strange that parts, possessing only an inferior degree of vitality, should be subject to such frequent and violent attacks of inflammation. Such, however, is the fact. It is very difficult to explain this apparent anomaly; I shall, however, throw out a few considerations which strike me on this point, having premised that you are to look upon them, not as proved, but lying open to future investigation.

If we examine the pathology of serous membranes, we shall find that they are liable to inflammation in proportion to the quantity of red vessels in the organs they cover; or, in other words, the more they are connected with organs enjoying a high degree of vitality, the more they are subject to inflammation. If we take the serous membranes lining the three great cavities of the body, we shall find that this position is, to a certain degree, borne out by the facts. One of the most common cases of inflammation is that of pleurisy; we seldom open a body without meeting with evidences of its existence at some period or other of the life of the individual. Now, we know that the lung is the most vascular organ in the body, and that through it alone the whole of the circulating blood is transmitted. Next in order to the pleura, we have the peritoneum—peritonitis being a much more common affection than arachnitis, for the intestines receive a greater quantity of red blood than the brain. Among the viscera of the three great cavities, the brain is the whitest, and receives the least blood, and we accordingly find that its investing serous membrane is least liable to attacks of acute inflammation. We have also a number of curious facts with respect to the nature of the connection which exists between any inflamed parenchymatous organ, and the serous membrane by which it is covered. Thus, in a case of hepatitis, we find that that portion of serous membrane which invests the liver exhibits traces of inflammation, while the rest of the peritoneum remains in its normal and healthy condition. You will see the layer of the peritoneum covering the liver in a state of inflammation, and will frequently find adhesions between it and the corresponding portion of the parietal layer, but no other sign of extension of inflammation. This fact is strongly in favour of the connection between the inflammations of serous membranes and of the organs they cover. There is another form of partial peritonitis which is exceedingly common—namely, the inflammation of that portion of it which embraces the uterus, and which is so frequently met with in puerperal fever. Now, here we have two considerations to bear in mind. During utero-gestation, there is a remarkable development of the uterus; and, about the period of delivery, it is a vast red organ, having a powerful determination of blood to its substance, as well for its own support as that of the infant. We accordingly find that after delivery peritonitis frequently sets in—its occurrence being favoured, in the first place, by the connection between the peritoneum and a large vascular red organ, and, in the next place, because the determination has not yet ceased, and the blood which went to the uterus (but cannot do so

any longer to the same extent) seems to be directed to the peritoneum. Another consideration bearing on this point is, that, when lymph has been effused in the case of inflammation of serous membranes, the process of organisation and cure goes on the more rapidly, the nearer the inflamed membrane is to a red and vascular organ. If we take a case of pleuritis with effusion of lymph and serum, when is it that we find the process of organisation set up in the effused lymph? It is when the fluid has been removed by absorption, and the two serous layers of the pleura are no longer prevented from coming in contact; for, as long as that portion investing the lung is separated from the costal pleura, either no organisation at all takes place, or, if it does, it is of a slow and imperfect character. But as soon as the effused serum is removed, and the costal pleura come into close apposition with a red and highly vascular organ, the lymph immediately undergoes a process of rapid organisation. The high degree of vascularity of the lung should be reckoned as one among the causes of the great frequency of pleural, as compared with peritoneal, adhesions.

A question may be asked here, connected with the idea that serous membranes possess only a low degree of vitality. If the vitality of serous membranes be of an inferior kind, why is it that their inflammations are so acutely painful? This, I must confess, is a question not easy to be answered. There is no doubt, however, that during the existence of inflammatory affections the vitality of these parts is considerably increased. They are elevated in the scale *for the time*. Their vessels carry red blood, their sensibility becomes exquisite, and they enjoy a reproductive power, as is shown in the case of the formation of new synovial membranes, capsular ligaments, and serous membranes; thus, as Dr. Graves has beautifully remarked, showing an analogy of organisation and function between *the white tissues of the higher*, and the whole constitution of the lower animals, in whom the power of reproducing *parts* is so remarkable. This circumstance seems to be strongly in favour of Broussais' doctrine, that inflammation is nothing more than a plus degree of local vitality. Some persons think that the pain in serous inflammation depends upon the density and unyielding nature of the affected membranes, for you are all aware that one of the effects of inflammation is at first to distend the inflamed organ. This may be true. Again, it is said that we are also to take into account the extensive and constant motions of the serous membranes. As long as respiration goes on in the chest, or digestion in the belly; while the ribs are elevated and depressed, and while the diaphragm descends, or while the peristaltic action of the intestines goes on; there will be motion in the serous membranes which invest their respective viscera. We know, also, that the brain enjoys a certain degree of motion. We must, then, in accounting for the pain of serous inflammation, take into consideration the dense structure and little distensibility of the parts, as well as their constant and uniform motion. There is one fact, however, which, as far as it goes, is of importance, and should not be forgot-

ten when we consider the motions of serous surfaces in reference to pain. Of all the serous membranes, the pericardium is that whose motions are most constant and violent. Yet I have now seen many cases of its inflammation, where quantities of lymph were effused, as detected by the stethoscope and by dissection, yet in which no pain whatever existed.

It appears to me that we can add something to these views in the way of explanation. When lecturing on hepatitis, I drew your attention to the fact, that if we compare the inflammations of the different solid viscera with respect to pain, we find that there is always less pain when the inflammation affects the central parts of an organ than when it is superficial. In deep-seated pneumonia there is scarcely any pain, in pleuro-pneumonia the pain is often acute. In arachnitis the pain is violent, in deep-seated disease of the brain there is frequently no pain at all. Inflammation of the central parts of the liver is generally a painless affection; but, when it approaches the surface, it is always accompanied by more or less suffering. Now, if we consider the serous membrane of the abdomen, for instance, to be an organ of low vitality and inferior sensibility, we must seek for some other explanation of the pain which attends superficial inflammations of viscera. The following idea may be of some assistance towards an explanation. Let us take, for example, a case of inflammation occurring in one of the glandular viscera. You are aware that Müller has reduced the structure of all glands to one formula, that is to say,—a gland, in its simple state, consists of a cavity, shut at one end and open at the other, and the difference between the various glands does not depend on any departure from this rule, but on a difference in the mode of aggregation and arrangement of these little structures. You will have a notion of the structure of glands by comparing them to a bunch of grapes, the grapes representing the glandular part, and the stalks their excretory ducts. Now you are aware that in most glandular organs the secreting portion is placed towards the circumference, and the ducts accumulate towards the centre; and, as the glandular portion always possesses a higher degree of vitality than the ducts, we can understand why the superficial parts of a glandular organ may be endowed with a higher sensibility than the deep-seated or central, and, consequently, that inflammation affecting the superficial parts of glandular viscera will be attended with more pain than when seated towards the centre, and affecting the excretory portion of the organ.

We are now, gentlemen, prepared to enter on the consideration of scrofula, on which much error still prevails, notwithstanding all that has been said and written on the subject. One great cause of the confused notions respecting scrofula arises from the circumstance, that some persons have understood the term as expressing mere local disease, as, for instance, of the glands of the neck, and having no reference to any peculiar constitutional diathesis; while others use it with reference to a peculiar condition of the system, which is called the scrofulous diathesis; and a third class apply

the term *scrofulous* to a number of very opposite diseases, which have no character in common but chronicity and incurability.

By considering the subject physiologically, we escape those errors and acquire more accurate ideas as to its real nature; and, as far as I can see, we cannot help adopting the opinion of Broussais,—that scrofula implies nothing specific; but, simply, that there is an undue preponderance of the white over the red tissues, and that, in such persons, there is of course a greater liability to *diseases of the lymphatic system*. Where there is an undue preponderance of the white over the red tissues, there we have the scrofulous constitution, and the liability to its accompanying diseases. Observe, there is nothing specific in this; an individual, originally free from scrofula, may afterwards be subject to it, and it may, under certain circumstances, occur in all constitutions.

The characteristics of what has been termed the strumous habit are known since the time of Galen. The skin is white, the complexion delicate and transparent, the hair fair in general, but sometimes dark; the features delicate, the upper lip thick, the alæ of the nose large, the head fully developed, the chest rather narrow, the joints large, a great tendency to sanguineous congestions, internal and external, which are very little under the influence of antiphlogistic means; the intellectual faculties early developed and of a higher order, great refinement, and delicacy of taste. In such persons there is generally a considerable preponderance of the white tissues, and they are much disposed to scrofulous disease, which is nothing more than a chronic irritation of the white parts and of the organs immediately connected with them.

We may look on the scrofulous diathesis as a condition of the human body which is to a certain degree imperfect, and which is to be attributed to arrest of development. There is a period of fetal life, in which the whole mass of the body consists of white tissues. According as the individual progresses towards maturity, the red tissues become more abundant; and when he arrives at maturity of maturation the proportion between the tissues becomes completely altered, the red being now more abundant than the white. But if this process should happen to be arrested, either shortly after birth or during life, we have then an individual of a lower degree of vitality, and approximating to the class of white-blooded animals. That we may reduce the scrofulous diathesis to arrest of development seems to be borne out by other considerations. We find in persons of a strumous diathesis proofs of arrest of development in various parts, so that whether we consider the question as to the development of the whole or of particular parts of the body, the same conclusion obtains. Scrofulous children have large heads, and it has been long known that they are exceedingly subject to hydrocephalus. The great size of the head in this instance is reducible to the principle of arrest of development; and here we have some explanation of the fact of the activity of the intellectual powers in scrofulous persons. Again, scrofulous children have large bellies; and here we have another proof of the arrest of development. In the fœtus

the belly is larger in proportion than it is in the adult; and if the individual grows up with this predominance, it is a proof of arrest of development. The liver in the fœtus is, as you all know, very large. Now it is a fact that many persons of a scrofulous habit grow up with this fœtal condition of the liver; and, accordingly, we find this organ enlarged, not as the result of disease, but because an equal and proportionate increase of other parts has not gone on; and here we have another fact confirming the principle of arrest of development. Scrofulous children are observed to have small limbs and contracted chests. Here, too, we again meet with the fœtal condition. In the fœtus the chest is small and contracted, and the extremities are puny and ill-developed. How beautifully this tallies with the state of the lung at that period of life, when there is very little employment for the thorax, and when the active functions of the lung have not as yet been called into operation. This, too, informs us, why it is that such children are so liable to affections of the lungs. We find that scrofulous persons are of a feeble frame, and have weak and flabby muscles; and, in accordance with this, we find on examination that the muscular system to a certain degree represents the condition of fœtal life, that the blood is albuminous, and its proportion of fibrin small. We observe that scrofulous children are subject to rickets, and that the proportion of phosphate of lime in their bones is small. Now this is precisely the condition of the bones in the fœtus. Thus, whether we look to the whole or to particular parts of the body, we find that scrofula is reducible to arrest of development, and that there is not in it any virus, any thing particular or specific, as has been erroneously imagined. To these considerations it might be added, that nothing is more common than to see those monstrosities, distinctly referable to *local* arrest of development, occurring in the scrofulous subject; and the statistics of monstrosity show that in this respect the female sex predominates over the male.

There has been much disputation as to the question whether scrofula be hereditary or not. You can easily solve this question, by reflecting on what you have already heard. No doubt it is often so; one or both parents may be scrofulous, and it is as natural that a child born of such parents should inherit their scrofulous constitution as that it should resemble them in features. But, on the other hand, we sometimes find that scrofulous parents beget healthy children. This appears to be an anomaly, but it may be explained by the circumstances of the child having a good healthy nurse, living in pure air, and having comfortable warm clothing, all circumstances calculated to develop the red tissues, and of course strengthen the system. Thus a scrofulous taint may be completely worn out in a few generations. It sometimes, on the other hand, happens that healthy parents may have children of a strumous habit. This, however, is the rarest case, but can be explained by reference to causes which would disturb the balance of development, and a person of an originally sound constitution may, under certain circumstances, become scrofulous. Children may be badly fed and

have insufficient clothing; they may be kept confined, and deprived of free air, light, and exercise; they may have an early irritation of the digestive system, from bad and unwholesome food, and in this way may acquire the scrofulous character without any hereditary disposition. Thus we come to the fact, which has been so frequently noticed, that scrofula will often pass over a generation, and that the grandfather and grandson may labour under it, while the son escapes its infliction.

It may be objected to this view of the question, that we frequently observe persons of dark hair and robust constitutions falling victims to scrofula. This is certainly true, but it is not on that account an argument calculated to militate against the doctrines which have been propounded on the subject. There are many causes capable of producing this increase in the white tissues, this peculiar state of the constitution, which we call the scrofulous diathesis. Excessive mental or bodily exertion, all the depressing passions, injuries of innervation of various kinds, deprivation of light, air, and exercise, early and continued gastro-intestinal irritation, persistent and exhausting diseases,—all these have a tendency to impair the functions of nutrition, and to destroy the balance between the different tissues of the body. If we look to those animals in which tubercles are found, we see that they are often those which have been brought from a hot to a cold climate, and kept in a state of confinement for a long time, and hence it is that we so often find fatal tubercular disease in animals which are kept for the purposes of exhibition. The same liability to tubercle is observed in several of our domestic animals kept in unhealthy situations, and deprived of air, light, and exercise. The truth is, that a combination of such causes will generate scrofula in constitutions which were originally sound and good; but it will occur at an earlier period, and with much more certainty when these causes are brought to act on persons in whom there is an original or congenital predominance of the white tissues. This fact is so well known as to require no further illustration; it is now generally admitted, and you will have many opportunities of verifying it in practice.

Allow me in concluding this subject, to direct your attention to the following considerations. If we were to connect the phenomena of scrofula with a predominance of the white tissues, it would follow that woman would be more liable to the disease than man. Louis, in stating the relative liability of the sexes to consumption, makes the proportion of males to females as 70 to 92. Cancer, which is also a disease of the white tissues, is, you all know, much more frequent in women than men.

Scrofula, then, to use the term as expressing local disease, seems to have in it nothing of a specific character, or reducible to the supposition of a virus existing in the system; it seems to be a slow irritation of the lymphatic system, occurring in persons who have a predominance of white fluids and white tissues. It would appear, also, that this predominance of white tissues may be either con-

genital, or it may be acquired, and that, in this case, it is superinduced by various causes, all having a direct tendency to diminish the proportion of red tissues, and lower the vitality of the system. This, which appears to me to be the true meaning of what has been termed the scrofulous diathesis, you will find to be borne out by a multitude of facts, and you will have numerous opportunities in practice of proving the value and importance of this view of the question. In this way we get rid of that mode of treatment which was based on the supposed specific nature of scrofula, and of which the object was the removal of an imaginary virus, and we are guided to the knowledge of a more philosophical and more successful line of practice. We arrive thus at the few and simple, but grand and efficient principles, in the treatment of scrofula; we see that scrofula is in its nature closely connected with the predominance of white and the diminution of red tissues, and we accordingly find that our curative means must embrace every thing calculated to invigorate the system, and add to the existing sum of vitality.

LECTURE XXXVI.

Fever—General considerations on—Erroneous modes of investigation—Importance of the labours of French pathologists—Complication of fever with local disease—Primary and secondary fevers—Relation of, to local changes—Tendency to spontaneous termination—Principles of treatment—Errors of Brown and Broussais—Researches of MM. Gaspard and Magendie—Their pathological conclusions—Importance of the knowledge of secondary lesions—Effect in preventing crisis—Treatment—Humoralism and solidism.

It may be safely asserted, that, in the whole range of medical science, there is no subject on which so much has been written and so little known as fever. You will find, in the writings attributed to Hippocrates, a series of observations on the rise, progress and termination of febrile affections, which it must be acknowledged are characterised by singular beauty and truth; and I think I may venture to say, that such is their extreme accuracy, such the comprehensiveness, acumen, and power of the master mind that made them, that scarcely a single one has been overturned by the researches of modern times. From the period of Hippocrates almost down to the present day, the contributions to this department of medicine, though numerous and varied, were of very inconsiderable importance; they effected little towards the improvement of our knowledge of fever, and many of them were calculated rather to puzzle and mislead, than to throw light upon what was difficult and obscure. In place of studying the phenomena and effects of fever, instead of applying themselves to what was tangible and useful, the minds of medical men were occupied in tedious but profitless attempts to discover the proximate cause of fever, and it

was to this subject that the labours of some of the greatest men in medicine were exclusively directed for a series of years. The consequence of this was, that our knowledge made no real progress, and as little was known about fever in the time of Cullen and Brown as in that of Hippocrates. We had innumerable discussions as to its cause and nature, we had a vast quantity of learned writing and ingenious speculation, but they produced nothing available for practicable purposes, nothing tangible or real. The investigators failed, because they reversed the Baconian method of arriving at the truth; they first built up a theory, and then thought to make the phenomena of nature square with it; they forgot that, to be truly philosophical, we must first collect, compare, and arrange facts; and, when we have done this, we may deduce from them a theory, cautiously, and with a strict regard to truth. They did not pursue this course, and the consequence is that they added nothing to the sum of our available knowledge. Their disciples knew nothing more than was known to Hippocrates; in fact, they knew less, for their notions on the subject of fever had reached them through an erroneous and distorted medium. The followers of Cullen viewed it through the theories of Cullen, the Brownists through those of Brown; both alike forgot nature, and both were consequently inferior to Hippocrates in true knowledge. They attempted to discover the proximate cause of fever, and they failed, as men generally do, when they attempt to investigate first causes. We know very little, indeed nothing, of the nature of first causes; they are, and will in all probability remain for ever, beyond the range of human intellect. It may be argued, that Cullen and Brown did not seek to ascertain the first cause, but only the proximate cause of fever; but this is only a play upon words, both are shrouded in the same obscurity, and in both the same difficulty attends our investigations. Even suppose we say with Cullen, that fever is a spasm of the extreme vessels; or with Brown, that it is asthenia of the whole system, what do we learn by this, or of what use is our knowledge? Have we more defined and accurate notions of fever?—Certainly not. They failed, as all men do who occupy themselves in the fruitless labour of searching after first causes. There is but one First Cause, and even of Him we know nothing accurate, but what He has vouchsafed to reveal.

Modern pathologists have pursued a course very different from this, and the consequence has been that they have arrived at the most splendid results. Instead of attempting to investigate proximate causes, they have studied the *phenomena and effects of fever*, they have examined dead bodies, they have accurately appreciated the series of pathological changes they present, and endeavoured to connect those changes with the symptoms. In this great work the French pathologists took a prominent part; indeed, I think it may be asserted, without fear of contradiction, that a vast proportion of our improved knowledge on the subject of fever is due to the French. It has been, I regret to say, too much the fashion to decry the labours of the French pathologists; but I believe this has

been chiefly done by persons who would gladly possess the knowledge they affect to despise. The French pathologists have pursued with respect to fever the same method they have so successfully employed in the investigation of other diseases; and though their researches have not thrown any important light on its proximate cause, they have taught us a vast deal as to its phenomena and complications, they have established a great number of valuable facts, and unfolded a series of beautiful truths; and, I need not say, that it is in the appreciation of these facts that a proper knowledge of fever consists.

In the first place, they have strongly drawn the attention of the medical world to this great truth, which should be engraven on your minds—that *mere fever, without local disease, is of very rare occurrence.* Here was a new and extraordinary light thrown upon the misty doctrines of the older pathologists. With them fever was a nonentity, something they endeavoured to describe but could not, something apart from, and totally unconnected with, organic change. The result of this mode of viewing the question was a variety of crude hypotheses and fanciful speculations. Bear this always in mind—mere fever, unaccompanied by local disease, is very rarely met with. Recollect, too, that it has been established beyond the possibility of doubt, *that fever, complicated with local disease, is the rule, and its non-complication the exception.* We have further learned from modern pathology, *that every system and every organ in the body may be, and frequently is, diseased during the course of fever, and that, in the vast majority of cases, death is the result of one or many local inflammations.* We further learn, that the character and symptoms of fever are infinitely varied, and that the cause of this variation mainly depends on the seat, the number, and the nature of the local affections. It is to these that we are to attribute the principal modifications in the character and phenomena of fever, and it is by these that its course and termination are mainly influenced.

Dr. Fordyce, in his work on fever, attempts to give a definition of the disease, and as I feel convinced that it is, if not a definition, at least one of the best descriptions of fever, I shall give it as nearly as possible in his own words. The style of this description is quaint but expressive. "Fever," says he, "is a disease which affects the whole system; it affects the head, trunk, and extremities; it affects the circulation, absorption, and the nervous system; it affects the skin, fibres, muscles, and membranes; it affects the body, and it affects the mind; it is, therefore, a disease of the whole system in the fullest sense of the term. *It does not, however,*" says he, "*affect the various parts of the system uniformly and equally, but, on the contrary, sometimes one part is more affected than another.*" This last observation is totally at variance with the idea that fever is a mere morbid condition of the whole system without reference to local lesions, for he expressly states that it does not affect the whole system uniformly and equally. This excellent view of fever seems to be borne out completely by modern pathology, and

particularly the last part, where he says, that in cases of fever one part is more affected than another. We have, for instance, cerebral fevers, nervous, bilious, gastric, and catarrhal fevers, by which, it is to be observed, we do not mean to imply that there is nothing more than simple disease of the brain, or nerves, or liver, or bowels, or respiratory system, but that in each of these fevers, disease predominates in some particular part. So that when we speak of these fevers, we speak of such a disease as Fordyce has described, in which one part of the body is affected more than the rest. In many of the schools you will still meet with Cullen's division into synocha, synochus, and typhus, a division by which we gain nothing at all, these terms being but mere words and no more. Will any one define what is meant by synocha, or synochus? Will any one say what is typhus? Will any one say that a particular class and character of symptoms and morbid changes apply to any of these affections? It would be quite impossible. What we generally find is, that in the different cases of what have been termed synocha, synochus, and typhus, though they may present the symptoms belonging to each separately, yet in these same cases, at some period or other, the symptoms pass into one another so as to confound the original distinction. We have synocha to-day, synochus to-morrow, and then typhus; or we may have typhoid symptoms at first, and inflammatory ones afterwards, and so on. We find, too, that *similar causes will produce in different individuals essentially different forms of fever*, and hence it is that we cannot find any distinction of fevers on their exciting causes. Nothing is more common than to see in two patients the same lesion producing, in one the synocha, in the other the typhus of Cullen. Thus, whether we look to the progress, symptoms, or exciting causes of fever, we find that this division has no foundation in nature, and is purely scholastic. Synocha, synochus, and typhus, are but mere names without meaning, terms which belong to the dogmatism of theory, and not to the expression of truth, and yet it is dreadful to think of the numerous lives which have been sacrificed at the shrine of this dogmatism.

Fevers may be divided into two classes. We have, in the first place, fevers which we may call primary or essential, in which we find (as far as human investigation can go) affection of the whole system, of the fluids as well as of the solids. This general state of the whole system seems in such fevers to have the initiative, constituting the first step in the process of disease; but it is also true, that, in almost every instance of essential fever, local disease springs up at some period or other of its course. We have, then, in these fevers this primary state of the system, the cause and nature of which are unknown, and we have this followed by various secondary lesions, affecting different parts of the body, and presenting characters by which we can arrive at a knowledge of their nature, more or less.

In the second class of fevers, we place all those in which the first affection is local, and the fever secondary. Observe the dis-

inction between this and the former class. In the first kind, or primary, the local disease is consequent on the fever, in the secondary, fever is the result of local disease. Let us take an example of each. A person in health is exposed to the contagion of typhus; he becomes languid and weak, has troubled sleep, bad digestion, and low spirits; after some time, what is called fever sets in, and during the course of this, various local diseases may supervene. In the other case, a person, also in health, from exposure to cold, or from some local injury, gets an attack of inflammation of the lungs, or some other local lesion, and, as a consequence of that lesion, has symptomatic fever.

Now the relation which the fever bears to the local symptoms in each of these cases is different. In the first case, the fever is primary, and the local affections secondary; and it may happen that, although the local diseases may be modified or removed, the fever will still continue; but in the second, the fever always vanishes on the removal of the local disease. We have to enter on the consideration of the first of these to-day, and to examine that morbid state of the whole system in which local disease supervenes at some period of the fever; in other words, where the lesions of particular parts or organs are symptomatic of the fever.

I have already mentioned, that one of the great truths at which modern pathologists have arrived is, that local disease commonly occurs during the course of fevers. It has also been established that, in the great majority of cases, the cause of death is one or more local inflammations. The experience of every candid pathologist is in favour of this doctrine. Patients seem to die of fever, but the fact is that some die by the brain, some by the lungs, others by the digestive system, that is to say, during the course of fever they get disease of various organs, some of the brain, some of the lung, and a vast number of the digestive system, often sufficient to destroy life if there was no fever at all. It is an undeniable fact, that, in the great majority of cases, there is local disease of some part or other of the body, and that a vast proportion of fever patients are carried off by local inflammation. How plain then is the deduction from these facts, that the man who neglects the viscera in fever is practising with his eyes shut.

So much for the first great fact of the complication of fever with local disease, and its important bearing on practical medicine. But there is another general consideration with respect to these primary fevers, they have a tendency to terminate spontaneously; of the cause of this spontaneous termination we are still in ignorance. One of the most simple and familiar examples of this is the paroxysm of an intermittent. A patient, who is at present apparently in good health, will in the space of an hour or less be attacked with severe rigours, followed by all the symptoms of fever, a flushed countenance, hot skin, quick pulse, and high coloured urine, and in some time afterwards a copious perspiration breaks out, which is attended with complete relief to the symptoms, and the patient gets well again. From this time until the period of the next attack

he continues to all appearance in health. Now, if we consider each of these paroxysms as an attack of fever, we see in them an evident disposition to terminate spontaneously. The same thing occurs in the case of the exanthemata. Scarlatina, measles, and small-pox, have a regular course, which generally terminates at stated periods; they also exhibit a succession of stages characterised by corresponding symptoms. We observe the same disposition to terminate spontaneously in most continued fevers, and it has been further remarked, that this spontaneous termination generally occurs on particular days. We have then two great leading facts in the history of all primary fevers, first, that they are most commonly complicated with local disease, and, in the next place, that they have a great tendency to terminate spontaneously and on particular days.

Now, gentlemen, you will please to observe, that a knowledge of these two very important facts furnishes us with two great indications—one, to discover and remove, or modify the local inflammations; the other, to support the patient's strength, so that he may not become exhausted during the progress of the disease, and thus lose his chance of this spontaneous favourable termination. These two indications, though apparently incompatible, are not so in reality. You will of course understand that the extent to which we pursue one or other of these indications, must necessarily vary according to circumstances. The rapidity, violence, and particular seat of the local inflammation, the duration of the attack, the age, sex, and constitution of the individual, all these are circumstances which must be taken into account in adopting any plan of treatment, whether calculated to remove local disease, or support the patient's strength.

The similarity between the different individual cases of fever, is too faintly shadowed out to amount to any thing like identity; in fact, there are no two cases of fever perfectly alike. You might as well expect to find two human beings exactly alike as to find two cases of fever perfectly similar. The causes of this remarkable variation are reducible to the extent, variety, seat, and complication of local disease, and to the peculiarities of the patient's constitution. These two classes of circumstances produce infinite varieties in the appearance and character of fevers.

The followers of Brown saw nothing in fever but debility, and their practice was to support the strength, and give stimulants from the commencement, ignorant of the fact, that neglected local inflammation will produce and keep up debility. The followers of M. Broussais, on the other hand, think that fever is sympathetic, that it depends on local inflammation, and that it must be subdued by depletion. Truth lies between. We must do both, we must combat the local inflammations by antiphlogistic means, and we must support the patient's strength by a well regulated regimen. These two indications are by no means incompatible, but their application must vary according to circumstances. If it be true, then, that local disease is very common in fever, and a frequent cause of

death, it is plain that to practise without a knowledge of the state of the viscera, would be acting like the physician mentioned by D'Alembert. He compares him to a blind man armed with a club, who comes to interfere between nature and disease. If he strikes the disease, he kills the disease; if he strikes nature, he kills nature.

A discussion has arisen in modern times, as to whether we should look upon all fevers as sympathetic. This is one of the leading doctrines of the school of M. Broussais. He declares that all fevers are sympathetic, that there is no such thing as an essential fever, or, in other words, that there never exists that peculiar morbid state of the *whole system* to which we apply the term fever; that in all cases fever is the result of local lesions, and that on the removal of these lesions its cure will depend. To this conclusion the school of M. Broussais was compelled to come in consequence of their exclusive solidism. They endeavour to reduce all the phenomena of life, whether in a state of health or disease, to the mutual action and influence of the viscera and solid parts on each other. They are solidists, in the strict sense of the word, and can have no conception of fever as existing independently of some primary local lesion. But it seems that the leading points of this doctrine have not been able to stand the test of an impartial examination, and may at present be looked upon as disproved. You will see at once the importance of this, when you consider that if it be true that all fevers are sympathetic, the practice must necessarily consist in the discovery and removal of local lesions, and no more. But I said that these doctrines are now disproved, and the following arguments may be laid before you in proof of this statement.

In the first place, let us enquire whether any cause acting on the whole economy is capable of producing local disease. Mark, the object of our enquiry is to ascertain whether any cause operating on the whole economy is capable of producing local disease. Now, I believe it is quite certain that such is the fact, and that we may have, first, a morbid condition of the whole system, and, consequent on this, various local lesions. Several continental pathologists, but in particular MM. Gaspard and Magendie, have shown, by repeated experiments, that we can produce all the phenomena of typhus in the lower animals, by introducing putrid substances into the system. These gentlemen injected putrid substances into the veins of animals, and applied them to the surfaces of wounds, and, in every case where these experiments were performed, they observed that the animals became ill, had languor, loss of appetite, thirst, prostration—in fact, all the symptoms of bad typhus; *and, in case of death, that they exhibited, on dissection, local lesions corresponding with those we meet with in the human subject in fever.* Now, observe, these animals were, previous to the experiment, in a state of perfect health; they are, then, subjected to the operation of a cause which is found to produce a morbid state of the whole system; they die, and on dissection inflammation and ulceration of the mucous membrane of the digestive tube, and other lesions, are dis-

covered in almost every instance. It would be quite absurd to say here, that the ulceration of the bowels was the cause of the morbid symptoms, for the animals were previously healthy. We can come to no conclusion, then, but that the introduction of putrid matter produced that morbid state of the whole system which is termed *fever*, and that the local inflammation was the result of this state. It is the same thing with respect to the exanthemata. A child is exposed to the contagion of small-pox; for some time nothing particular is observed; he then gets ill and feverish, and this is followed by an eruption of variolous pustules. Here we have a local disease consequent upon a circumstance affecting the whole system, and in this, as in the former examples, the local lesion is secondary. We might as well argue that the pustules were the cause of the symptoms in one case, as to say that the ulceration of the intestines was the cause of the other. Every one, I think, will admit that the pustular eruption in a case of small-pox is secondary, and not the cause of the symptoms; and the same argument will apply to the secondary affections of typhus. If it be true, as appears by M. Magendie's cases, that fever follows the introduction of putrid substances into the body, and that this morbid state of the system produces inflammation of the intestinal mucous membrane; if, too, we admit that in small-pox the pustules are secondary, and consequent on a morbid state of the whole system originating in contagion, the same argument will hold good in all cases of local inflammation, (whether of the liver, lungs, brain, or any other organ,) which may arise during the progress of fever. These facts are adduced in support of the first part of the argument—that local lesions may be and are consequent on that morbid state of the whole system to which the name of fever is applied.

The next thing to be observed with reference to this question is, that if it be true that typhus is merely symptomatic of local disease, it would then follow *that there should be as constant a relation between the symptoms during life and the morbid changes seen on dissection*, as there is between the fever of pneumonia and the changes presented by the lung. But this is not at all the case, for we find that there is no constancy, no uniformity, either in the seat or extent of the local disease. Two patients will exhibit symptoms of typhus not differing in any material point, and yet, on dissection, you will find little or no traces of disease in the intestinal canal of one; in the other, you will find in the same parts a vast amount of disease. Two others will also present symptoms very similar; in the one you will find the lung healthy and normal, in the other, you will find it extensively disorganised. Would it not be absurd to assert here, that the fever was symptomatic of the local lesion, seeing that there is no constant relation between the symptoms and the morbid changes, either as to situation or extent? Again; it is a fact, that you may have several patients presenting different symptoms, and yet, when you come to examine their bodies, you find the same morbid changes in all. One may exhibit all the phenomena of typhus; in another, this condition is but slightly marked;

in a third, it is absent; and yet, on dissection, you find a similarity of local lesion. Lastly, it may be argued that if typhus were symptomatic of any particular local lesion, we should be able to cure it by removing that local lesion. This, however, is not always the case; that it sometimes does occur I am willing to admit, and this is therefore to be considered as the weakest of my arguments. But, on the other hand, if it be admitted that the local lesions are accidental and secondary, we can easily understand why their removal should not necessarily cause the removal of the fever. Such are the arguments on which I ground my objections to the doctrine—that all fevers are merely symptomatic of local disease; and in these views I think I am borne out by the opinions of the soundest modern pathologists.

But though we admit that local inflammations are secondary, and bear the same relation to typhus as the eruption of small-pox to the morbid state that precedes it, still they are not the less important; and it is by a careful study of them that we arrive at a key to correct and successful treatment. They are of great importance from being exceedingly common in fever; in fact, so common that their occurrence is the rule, their absence the exception. They are also, in the majority of cases, the cause of death, and this they bring about in two different ways. First, directly, as in a case of simple inflammation. A patient in fever, who gets an attack of violent enteritis, may die of it as well as if he got an attack of primary enteritis; or he may die of pneumonia coming on during the course of his fever as well as the man who dies of pneumonia from cold. Thus we see that the secondary inflammations may produce death directly. They may also produce it indirectly, by preventing the efforts of nature towards a favourable termination. You recollect I told you, that in fevers there is a strong tendency to terminate spontaneously and on particular days. Now we find that this disposition is greatly impeded by the presence of local inflammation, so that local inflammation may operate to the destruction of life in two ways; either directly, by its intensity and extent, or indirectly, by preventing a critical termination.

This leads us to look still deeper into the matter. We find that these local or secondary affections may also produce a train of sympathetic phenomena of a very remarkable character. There is no reason why enteritis coming on during the course of a fever may not react on the economy as well as the enteritis from cold, which we know generally produces symptomatic fever. In the case of two patients, one, for instance, meets with some lesion of the intestinal mucous membrane, and, as a consequence, gets enteritis and sympathetic fever; another gets enteritis during the course of a typhus; in the one case, the local lesion plainly reacts on the system, in the other this is less apparent, but there is no reason to suppose that it does not produce some effect on the system in one case as well as in the another. The law appears to be this, that in almost all cases of fever there is a combination of the essential and the sympathetic fevers, the essential the result of the first cause, and the sympathetic the result of the local lesions which arise

during its course. Indeed, nothing seems to be better established than that local disease reacts on the system and prevents a critical termination. You will get a very good idea of this by considering the paroxysms of an intermittent. What are the periods in which an intermittent is most liable to terminate favourably? The earlier ones. What are the periods in which a favourable termination is least likely to happen? The later ones. Now what are the periods in which there is little or no accompanying organic lesion? The first or earlier. What are those in which there is more or less of organic change? The later, in which we generally find, on making a careful examination, that disease of some organ, or organs, has taken place, and is presenting an obstacle to a favourable termination. It is the same thing with respect to fever.

In the treatment of fever, it is a most important rule to investigate the condition of the viscera, and remove, if possible, any existing local inflammation. By this we accomplish a double purpose; we prevent the direct danger of death from the violence of local disease, and we obviate the inconveniences arising from sympathetic irritation. We give nature fair play, we reduce the case to a state of the greatest simplicity, we prevent the liability to new local affections, and we thus effect a great deal towards a favourable termination. It is an interesting and singular fact, and one dwelt on by the school of Broussais, that in many cases of fever the removal of the local inflammations is speedily followed by a subsidence of the fever. It is chiefly from this fact that they argue in favour of the opinion, that all fevers are symptomatic of local disease. This argument, however, as I have already proved, is more specious than solid. The true reason is, that by removing local disease we remove the barrier which opposes the salutary operations of nature. Every attempt at a favourable termination is impeded by the co-existence of local disease, and the more intense and extensive this is the greater is the obstruction. You are not by any means to conclude that a fever is symptomatic, because it disappears on the removal of local disease; the true explanation is, that by subduing the local inflammation you have removed a focus of irritation, and given scope to the preservative powers of nature.

These I believe most firmly to be the true principles which should guide us in considering the subject of fever. They have been obtained by careful and accurate deduction, and are based on a numerous series of well conducted experiments. Weigh the matter calmly, and I think you will be disposed to agree with me, that fever in its origin implies no tangible condition of the system, and that we know it only as consisting of a group of phenomena, varying as to their cause, seat, effect, and duration. The humorists erred by fixing its seat in the fluids, the solidists by limiting its locality to the solids. We recognise no distinction between the fluids and solids, so far as fever is concerned; they all form parts of the great whole; one cannot act without the other, but their mutual reaction is extensive and various. From these considera-

tions we deduce the important rule, that there is no mode of treatment universally applicable, and the man who treats fever with wine and stimulants only, or he who contents himself with purgatives and diaphoretics, or he who limits his practice to leeches and the lancet, that man knows nothing of fever. Though his hair be gray and his authority high, he is but a child in knowledge, and his reputation an error. On a level with the child so far as a correct appreciation of the great truths of medicine is concerned, he is very different in other respects; his powers of doing mischief are greater, he is far more dangerous. Oh! that men would stoop to learn, or at least cease to destroy!

LECTURE XXXVII.

Intermittent fever—Definition and character of—Phenomena of the paroxysm—Cold stage—Internal congestions—Pathology of—Hot stage—Ague not a simple fever—Affections of various viscera—Theory of Broussais—Effects of bark, quinine, &c.—Modus operandi of.

To-day we commence the consideration of intermittent fever. One of the most prominent characters of this affection is expressed by its name; we have all the phenomena of fever making their appearance at certain periods, and then disappearing, leaving an interval in which the constitution seems to be in the normal state, and continues so until the supervention of a second attack. It has been also termed a primitive or essential fever, in which there is no original local disease, and where the fever, in the beginning, is not symptomatic of any local lesion. We may define intermittent fever as a primitive, or essential fever, composed of many paroxysms which recur at certain periods, and in the intervals between which we have a state of apyrexia, or freedom from fever. This definition, though applying to the great majority of cases, is still to a certain degree imperfect, for we meet with examples of this disease in which the periods of attack are by no means regular or certain, and the state of apyrexia between the paroxysms not well marked. Thus, in two cases of intermittent, we observe that in one the patient appears, during the interval, to be completely free from fever, while in the other we find that the febrile symptoms continue to a certain extent after the subsidence of the paroxysm. As a general definition, however, the foregoing is tolerably good, and will be quite sufficient for practical purposes.

Another remark to be made on this subject is, that an intermittent is not necessarily an essential fever. We may have it from lesions of various kinds. You are all familiar with that form which attends bad cases of stricture and retention of urine, and

which has been called urinary fever. Here we have fever of an intermittent character, not essential, but depending upon a local lesion. We have many other instances of a similar kind, and I could easily multiply examples.

I shall not take up your time by entering into a description of the various divisions of intermittent fever; it is a species of knowledge unconnected with any point of great practical importance, nor does an acquaintance with the nature of the disease, so far as frequency of paroxysm is concerned, shed any useful light on its treatment. The same principles of treatment are applicable to quotidian, tertian, quartan, and every other variety of intermittent. Besides, we know nothing whatever of the nature of an intermittent. We are here as much in the dark as we are in the case of continued fever. We are still in complete ignorance as to the cause of the periodicity which is so remarkable a feature of this as well as of other diseases. It is enough for us, in the present state of medical science, to know that such things exist, and, leaving the researches after the cause of the disease and its periodicity to future investigations, let us study the effects of the disease, and direct our attention to things within our reach.

Let us, as far as we can, examine what takes place during the paroxysm of an intermittent. I think it my duty, as a lecturer on practical medicine, to direct your attention to this point rather than to the history of intermittents; this is, I grant, not devoid of interest, but it is a subject on which you will find ample information in the various systematic treatises on medicine. My intention is to endeavour to point out the true principles of treatment; I shall, therefore, enter no farther into the history of intermittents than what is connected with diagnosis. This will certainly diminish the interest of a lecture on intermittent fever, but this cannot be helped; it would, in a limited course of lectures like this, be quite out of my power to lay before you the mass of curious and instructive matter connected with the history of intermittents. The paroxysm of an intermittent fever has been divided into three stages—the cold, the hot, and the sweating; but it simplifies the matter very much, to consider it as divisible into two stages, the sweating being the result of the hot stage. With respect to the cold stage, I shall endeavour to establish three great propositions. In the first place, it appears (and this is highly important) that, in the majority of cases (I do not say in all, for there is no general rule in medicine), there is, during the cold stage, a perceptible lesion of one or more internal organs, and that there is congestion of many, if not of all, the viscera of the three great cavities. To use a modern phrase, we have, during the cold stage of an ague, a state of hyperæmia of the internal and anæmia of the external parts, or, in other words, the balance of the circulation is lost, the blood forsakes the surface and accumulates in deep-seated organs. The next proposition is, that this hyperæmia, from frequency of repetition, or excessive violence, may be accompanied by, or productive of, an inflammatory condition of these organs. Lastly, that

organs thus altered may in themselves become sources of irritation, react on the system, and powerfully tend to keep up disease. You see, then, that, in considering the phenomena of intermittent fever, we must follow the same road as in continued fever, and regulate our enquiries more in relation to the effects than the cause of the disease.

The first of these propositions—that during the cold stage of an ague there is a congested state of almost all the viscera—is proved in every way that a pathological proposition can be proved. It is confirmed by an examination of the symptoms, by the results of treatment, and by the appearances seen on dissection. In this country, we very seldom have an opportunity of examining the bodies of patients who have died in the cold stage, for the intermittents of this country are trifling in comparison with those which are observed in warm climates. But the fact is fully borne out by an examination of those who have died in this country under such circumstances, as well as by the more numerous examples occurring in countries in warmer latitudes. Let us take the different parts of the system during the cold stage, and see how far the symptoms point out an accumulation of blood. First, let us review the nervous system. There is a feeling of tension and fulness about the head, the patient complains more or less of headache, the sensibility is diminished, and there is frequently stupor and coma, and in violent cases there may be convulsions. All these circumstances are indicative of congestion of the brain; and accordingly we find, where we have an opportunity of making an examination, that the venous system of the brain is in a state of engorgement. In some cases, the carotids and their branches have been observed full of dark-coloured blood, the congestion of the lungs having interfered with the process of aëration. In the intermittents of warm climates still more remarkable effects have been witnessed—enormous congestion of the vessels of the brain, and frequently effusions of blood into its substance; so that the symptoms during life, and the appearances seen after death, tend to confirm the fact of congestion, so far as the brain is concerned. If we turn to the pulmonary system, we find that nothing is of more common occurrence, during the cold stage of an ague, than lividity of face, anxiety, cough, and hurried breathing; and when we come to examine the chest, we find more or less dulness of sound on percussion, and the other physical signs of congestion of the lung. This is further confirmed by dissection; the lung is congested, and of a dark red colour; it will often sink in water, and presents a condition closely bordering on hepatisation. If you examine the heart, you will find that its action is oppressed, the pulse is small and irregular, and the right ventricle, with the vessels attached to it, are found engorged. Proceeding to the abdominal cavity, we find the same indications of congestion. There is a sense of pain and fulness in different parts of it; the patient has vomiting, often diarrhœa, and a copious discharge of urine; all this shows a violent determination of blood. You have often heard of the tume-

faction of the spleen which accompanies the cold stage of an intermittent. Now, so rapidly does this occur, and so extensive is the engorgement, in many instances, that shortly after an attack we can readily feel and trace it distinctly. On dissection, we meet with abundant proofs of congestion; we find the liver highly engorged, the intestinal mucous membrane very vascular, the mesenteric and portal veins filled with blood, the kidneys congested, and the spleen enlarged. Cases of rupture of the spleen, from excessive congestion during the cold stage of ague, and of hepatic apoplexy from the same cause, are described by Bailly. These facts are sufficient to prove the truth of the first proposition—that, during the cold stage of an intermittent, most of the internal organs are in a state of congestion. We are led to suspect this from the symptoms, and our conjectures are confirmed by dissection. Indeed, the most superficial observer must be struck with the remarkable retreat of blood from the superficial parts of the body. The skin is pale and shriveled, the bulk of the limbs diminished, the countenance collapsed, the whole surface cold, and superficial vascular tumours are observed to lose their vascularity and become reduced in size. All these circumstances lead us to the supposition of internal congestion, and this is corroborated by the results of dissection, which show the various internal organs in a state of hyperæmia, and shows the retreat of the blood from the surface, and its accumulation in deep-seated parts.

The next proposition is equally important—that the frequent repetition, or excessive amount of these internal congestions, may, and does, give rise to an inflammatory condition of the affected organs; in other words, that the hyperæmic condition of certain viscera, during the cold stage of ague, and their subsequent organic lesions, stand in the relation of cause and effect. We find that the effect produced by the cold stage on the viscera is twofold; we may either have congestion independent of any organic change, or we may have inflammation. In some cases, particularly in those which occur in warm countries, the congestion is followed by violent inflammation; in others, as in the cases of this country, we have chronic inflammation produced. But whether we meet with one or the other, whether the inflammation be acute or chronic, we find that, as soon as the viscera become affected in this way, a state of constitution is brought on in which the power of the usual remedies is diminished, and their use frequently prejudicial. New local inflammations are set up in various organs, and *these, which were in the beginning only the effect of the disease, become, by reacting on the system, the cause of its continuance.* In warm climates, where the congestion is excessive, there is nothing more common than to see fatal pneumonia, or violent gastritis, or rupture of the spleen, or cerebritis, supervening on a bad attack of ague. Strictly speaking, the production of inflammation in organs seems to belong more to the hot than the cold fit, and this we can easily understand. In the cold stage, the viscera are in a state of intense congestion; violent reaction then comes on with the hot fits; and

when we have in any organ a stasis of blood, and violent action of its vessels, occurring during the existence of this stasis, it is not surprising that inflammation should be the result.

In the hot fit, all the phenomena, which we have been just now examining, are reversed. Every thing indicates that the energy of the circulation is about being restored, and that there is a powerful determination to the surface. The pulse gradually rises in strength, the rigours gradually disappear; the skin, which was cold and shriveled, becomes hot and tense; the face, which was blanched and collapsed, assumes a full and flushed appearance; the cough and hurried respiration are relieved; the vomiting, diarrhoea, and discharge of urine, cease; and the stupor is removed. The vessels on the surface of the body become more and more distinct; and in those who have large superficial veins, those vessels (though there was no appearance of them during the cold fit) stand out in bold relief like so many thick cords. I have alluded before to the greater severity of the cold fit of the intermittents of warm countries; the hot fit exhibits a corresponding degree of intensity. The cold fit is generally accompanied by violent vomiting and purging, spasms and convulsions; the hot stage is attended with fits resembling apoplexy, and is frequently succeeded by intense pneumonia, cerebritis, and other forms of visceral inflammation. Such occurrences are rarely seen in the fevers of this country.

The great principle to be borne in mind with respect to intermittent fever is, that during the cold stage the viscera are in a state of congestion, and consequently fitted for the reception of disease. When the hot fit comes on, this state of congestion generally disappears; but if it should continue, we shall have a chance of inflammatory action being set up in one or more viscera, and in this way we may have a number of points of irritation in the system, complicating the original affection, and tending to retard the operations of nature and art towards a favourable termination. You can easily understand, that if the lung, which has been in a highly congested state during the cold stage, does not, during the succeeding hot stage, throw off the load of blood completely, it will be less able to accomplish this at the next attack, and so on, until at length the process of inflammatory alteration is firmly established. The same observation applies to the other viscera. In some cases, just as in continued fever, we have the brain chiefly engaged, in others the lung, in others the digestive system; and, in the intense agues of tropical climates, we often have the three great cavities simultaneously attacked. You will observe here, that the production of visceral disease depends on two circumstances—first, on the intensity of the congestion, and, secondly, on a frequent repetition of the attack where the symptoms are less violent. In this country, the latter seems to be the principal cause.

From these considerations, we come to the important law that, after some time, we are not warranted in looking upon ague as a simple fever, but as a fever of a compound nature, involving affections of many important viscera, to which we must attend carefully,

if we seek to practise with safety and success. It is singular, that the majority of medical men appear to look upon the effects of an intermittent as being very circumscribed. You have all heard of enlargement of the spleen. Now, I have known some practitioners who appeared to think that this was the whole pathology of intermittent fever; in fact, that there was a very close connection between enlargement of the spleen and ague. This, I need not tell you, is a very imperfect view of the question; the spleen suffers like other viscera, but there is no separate relation between its enlargement and the production of intermittent fever. The reason why attention has been chiefly directed to it is, because its lesions are generally more manifest than those of other viscera. It is composed of a loose, spongy, erectile tissue; it receives in its natural condition a great quantity of blood; it becomes rapidly and extensively enlarged during the cold fit; and hence it becomes a very prominent and remarkable sign of the disease. But I believe that in all cases where the spleen is found to be enlarged you will also be able to detect disease of the liver or lung. All the viscera are more or less liable to suffer under similar circumstances; there is, in a word, no single acute or chronic disease which may not be the result of intermittent fever. In this country, it generally gives rise to chronic disease; in warm climates, acute visceral inflammation is more commonly the result. Here we arrive at another very important consideration, namely, that it may happen that the phenomena of an intermittent shall cease, and yet the chronic disease produced by the violence or persistence of the original malady will continue. This is exceedingly common. We frequently meet with chronic disease of the heart, lungs, or brain, with dysentery, diarrhœa, peritonitis, affections of the kidney, and chronic inflammations of the liver and the spleen, brought on by intermittent fever, in this country. These local affections are the result of violent congestion, and the continued irritation which accompanies the early paroxysms of the disease; and though the symptoms of ague may subside, the morbid irritation, which has been set up in the constitution, may proceed to such an extent that death may be the result of a complication of visceral affections thus produced; and this I believe to be the history of many chronic cases of ague. Chronic affections of the lung, liver, brain, and digestive system, are, then, the chief things to be dreaded or guarded against in a case of intermittent fever; for after all that has been said about the enlargement of the spleen, it seems to me to be the least important of the visceral lesions which follow ague. It appears in almost every instance (at least as far as we can see of it in this country) to exercise but very little influence over the economy. In warm climates, indeed, it is sometimes so much enlarged as to produce serious inconvenience by its pressure and bulk. Thus, in South America, it has been observed, that in bad cases of ague the spleen has become so enormously increased in size as to fill nearly the whole abdominal cavity, producing great derangement of the digestive organs, and actually hernia. Dropsy, jaundice, chronic hepa-

titis, diarrhœa of an intractable character, various nervous affections, amentia, an atrophied state of the system, phthisis, hectic, typhus, all these constitute part of the morbid affections which follow in the train of ague as it appears in this country, and all will be found connected with various chronic visceral lesions which have been the immediate results of the original disease.

When called to treat a case of ague which has been going on for some time, you will generally meet with one of two things; you may find that the viscera have, or that they have not, suffered much from the effects of congestion. Now, when organic changes of viscera take place, we have the remarkable circumstance of their active tendency to keep up the original malady. Here you will frequently see practitioners prescribing bark, and if you watch its operation, you will sometimes find that it does more harm than good. But if, on the other hand, attention be directed to the local lesion, neglecting the intermittent for some time, you will find that the removal of the local disease brings back the intermittent, more or less, to its original state of simplicity, and renders it amenable to the specific. Observe the importance of this. The same rule holds in intermittent as in continued fever; you must practise with an eye to the state of the viscera, and recollecting that the disease, which results from one as well as the other, may be, in its turn, cause and effect. Here I shall take an opportunity of making a few observations with respect to a theory of intermittent fever which has been put forward by such high authority that we cannot pass it over. I allude to the theory of M. Broussais, in which he endeavours to show that intermittent and continued fevers are reducible to the same form, namely, an irritation of the digestive system. To this view of the question a great many facts are opposed, all tending to prove that disease of the digestive system (which is so common in intermittent as well as continued fever) is to be looked upon more as an effect than as a cause. It is absurd to say that intermittent fever is merely an intermittent gastro-enteritis, when dissection shows that we have not only disease of the digestive tube, but also of the heart, lungs, and brain. The fact of coexisting visceral inflammations was passed over by M. Broussais, and certainly it must be acknowledged that this is a very simple mode of getting rid of a strong objection. His doctrine was, that fevers, intermittent as well as continued, are only examples of the effects of irritation of the digestive system, the continued fever being significant of severe and extensive disease, the intermittent of an affection of a milder character. He also maintains that the rigours are produced by, and proportionate to, the internal irritation. Now, admitting, for argument's sake, the two first propositions, what do we find to be the fact? That in continued fever, where the irritation is greatest, the rigours are comparatively trifling, while in the intermittent, where the irritation is less violent in degree, the rigours are remarkably intense. Here we see intense rigours with slight disease, and trifling rigours with

intense disease, two facts strongly militating against the proposition of M. Broussais.

Another argument may be urged against these doctrines: I allude to the effect of bark. This is a point which very much puzzled the physiological school, and they have accordingly exerted all their ingenuity to explain it away. At first, I believe, they were strongly inclined to deny altogether the specific powers of this remedy; they were subsequently, however, compelled to subscribe to the fact of its efficacy, which was too notorious to be denied, and they had then to explain how bark could cure a gastro-enteritis. Hard as it was to explain how bark could be instrumental in removing gastrointestinal inflammation, it was a matter of unavoidable necessity to attempt something like an explanation, in order to maintain the integrity of the physiological doctrine. They therefore set about the task, and endeavoured to show that bark cures the ague, or its cause, gastro-enteritis, by substituting one irritation for another. The whole gist of their arguments is founded on this point:—quinine, they say, is a stimulant; ague is the irritation produced by gastro-enteritis. Now, it is a fact that stimulants will frequently remove existing irritations; thus, we frequently observe, that blennorrhagia, chronic ophthalmia, and diarrhœa, are cured by stimulants. This argument, however, is more specious than solid. There are certainly cases of irritations of mucous surfaces which may be removed by stimulants, but these are cases of *chronic and not of acute disease*. No one would dream of employing stimulants in a case of acute febrile diarrhœa, few would think of applying irritants to an acutely inflamed and painful conjunctiva; it is only when the character of the inflammation alters, and the affection acquires more or less chronicity, that these remedies prove at once serviceable and safe. Now this is not the case in ague. If it depends upon an acute gastro-enteritis, we are, according to this line of argument, to conclude, in the teeth of their other doctrines, that the best mode of curing an acute attack of gastro-enteritis is to stimulate the inflamed mucous surface. Here, you see, we arrive at the argument *ad absurdum*. Another observation on this subject:—In a case of intermittent fever, supposing it to depend on gastrointestinal irritation, it is a matter of indifference whether you give the bark during the paroxysms, or during the intermissions. Now, it is a matter of experience, that bark will be far more effectual when given during the intervals of apyrexia. Again, if bark cured by producing a new irritation, we ought to see the symptoms of that irritation succeeding the administration of the remedy.

Thus, whether we look to the symptoms, the appearances seen on dissection, or the results of treatment, and the efficacy of bark, we must conclude that the morbid state of the digestive system stands in the relation of effect, and not of cause. As far as we can judge of ague, it appears to be some profound alteration of innervation, some affection of the whole system, the nature of which we cannot understand, but the effects of which we perceive in the

various derangements of internal organs, by which it is attended. Intermittent fever is not intermittent gastro-enteritis, because dissection reveals various other important lesions, and because it is cured by bark, which has nothing in it calculated to remove acute inflammation. We know that if a patient labouring under ague has acute inflammation of the stomach, bark, so far from curing, will do him a great deal of harm. Bark cannot cure by exciting irritation, because, if it did, it would increase the supposed gastro-enteritis, and we should have more violent symptoms during the intermissions, the time when it is always given, than during the paroxysms. Lastly, there is no analogy between the effect of stimulants on acute and chronic inflammations. What the *modus operandi* of bark is, we cannot explain. Many things, connected with the phenomena of life in health and disease, are, and probably will for ever remain, concealed from human ken. We daily witness the effects of stimulants, but we cannot explain their mode of action. This, however, is no opprobrium to medicine. This is the right way of viewing the subject; part of it is capable of admitting an explanation, the rest constitutes a portion of the inscrutable arcana of nature. It fortunately happens, however, that, for all practical purposes, the knowledge of these occult portions of medical science is as yet comparatively unimportant.

LECTURE XXXVIII.

Intermittent fever—Symptoms—Occasional irregularity of the paroxysms—Convulsive motions of the fetus in a pregnant woman during ague—Exciting causes of ague—Treatment—Complication with other diseases—Importance of careful investigation—Visceral lesion, how far contra-indicating the use of bark—Bark almost a specific in ague—Large doses of quinine in ague—Rapidly of its operations in some cases—Fowler's solution of arsenic—Prussian blue—Its advantages.

Let us proceed with the subject of intermittent fever. I gave you, at our last meeting, an analysis of the phenomena which accompany the paroxysms of this disease, and endeavoured to show that intermittent fever was not reducible to any form of fever symptomatic of local inflammation, but that it was, as far as we could see, an original affection of the system, connected in all probability with some profound lesion of innervation. I shall now proceed to a brief review of some of the more prominent symptoms.

The first approach of a paroxysm of ague is, generally speaking, pointed out by a degree of languor and debility more or less marked. The patient, without any apparent cause, becomes weak and restless, throws himself on a bed or couch, feels chilly, and has a sensation as if a stream of cold water was running down his back; his features are considerably shrunk, and his skin pale and corrugated. In some cases the surface of the body is cold, and I believe

in most cases the patient has a strong sensation of cold, even though the skin may continue warm during the rigour. By degrees the chilly sensation which is first felt along the back extends to all parts of the body; the patient begins to shiver; has horripilation and chattering of teeth; and as the symptoms increase, the rigours are so violent as to shake the bed on which he lies. The sensation of cold now becomes intense; he calls for warm drinks, and begs of his attendants to heap more bedclothes on him. He is anxious, restless, and uncomfortable, and if questioned answers briefly and with reluctance. His pulse is small, weak, and irregular; the action of the heart oppressed, and respiration more or less laboured. The general sensibility is impaired; in some cases there is delirium, in others a state of stupor amounting to apoplexy. Headache is most commonly present, and there is often pain in the loins and sides. The situation of the patient is indeed truly pitiable; he lies beneath a heap of bedclothes, with his teeth chattering, his whole frame agitated by the shiverings, an icy sensation running through his whole frame, and which neither warm drinks nor accumulated covering can relieve. Add to this the headache, the delirium, the stupor, the nausea, and diarrhoea, and you have the picture of suffering complete. This extraordinary condition may last for some time. It is generally terminated by a copious attack of bilious vomiting, when the rigours begin to decline, and are succeeded by alternate flushings of heat. The rigours now cease; the flushings become more frequent and protracted; the patient begins to feel the return of heat; the skin becomes gradually hot, and this increases until the heat gets far above the natural standard, being in some cases as high as 110° . In a few cases, particularly in warm climates, the temperature during the hot fit has been known to exceed this. Accordingly, as vascularity returns to the surface, the features lose their contracted appearance and the skin its paleness; the face becomes full and flushed, the limbs regain their ordinary dimensions, and the skin becomes smooth and almost tense. The nausea and vomiting abate, the patient is hot and thirsty, and has scanty high-coloured urine. The pains in the loins and back still continue, the headache is generally increased in intensity, and the tendency to delirium greater. The pulse is strong, quick, and bounding, and all the symptoms of high inflammatory fever are present. After some time, a tingling sensation is felt all over the body, accompanied by a degree of moisture, which soon increases to a copious and universal perspiration. With this all the unpleasant symptoms subside; the patient seems to luxuriate in his relief; thirst, headache, fever, vanish altogether; he declares that he is quite well, gets up, and continues in the enjoyment of apparent good health until the next attack.

The length of time which the whole paroxysm may last will vary from one to twenty-four hours. I have seen the three stages pass through their course and subside in the space of an hour; on the other hand, I have known them last for a day—this, however, is very rare.

I told you in my last lecture, that in this country we seldom observe those violent symptoms which characterise the agues of warm climates. In the bad cases of those countries a train of symptoms occur marked by extraordinary intensity; the patient has violent convulsions and tonic spasms, a wild staring eye, high delirium, excessive vomiting and purging, terrific rigours during the cold fit, and when the hot stage comes on, he frequently has fits resembling epilepsy or apoplexy, and inflammation of the brain, or lungs, or digestive system. Sometimes two or all the great splanchnic cavities are simultaneously affected.

It is generally believed, that in the different types of intermittent fever the paroxysms come on at particular periods:—thus we hear of the paroxysm of quotidian occurring in the morning, of tertian at mid-day, and of quartan in the afternoon. In Dr. Good's Study of Medicine, a work remarkable for its learning, you will find all these circumstances detailed with an elaborate minuteness, and you will see that the author has considered them of importance sufficient to make a generic difference between the various forms of intermittent. Many of his observations, however, on this subject, betray a want of accurate observation. The truth is, that in every form of intermittent we may have the access of fever at any hour in the day. In the Meath Hospital we have found that the rule before laid down does not hold good; for we observed that the paroxysms of both quotidian and tertian occurred at all hours of the day and night. Andral, in his Clinique Médicale, gives twenty-five cases of ague, of which eleven that were cases of quotidian came on between four and eleven in the afternoon. Out of seven cases of quartan, one came on in the morning, the rest in the afternoon. These facts show that the rule is by no means so general as has been imagined, and that it is not of sufficient importance to ground upon it any generic difference. There are also many varieties in the complications of intermittent described besides those which are witnessed in this country, but these I am inclined to think as being of very little value, for as yet we have no therapeutic indication founded on the divisions of intermittent fevers *quoad* their type. However, as some of you may be hereafter placed in such circumstances as to find this species of knowledge a matter of importance, I shall make a few observations on the point.

During an epidemic of intermittent fever, you will meet with some cases in which there will be an extraordinary regularity in the access, duration, and intermission of the paroxysms, while in other cases you will find them irregular as to the time of invasion, the phenomena of the different stages, and the period of subsidence, as well as the interval of apyrexia. This is what is called irregular ague, and seems to be the same as that which Frank has described under the title of *febres intermittentes larvatae*. These are cases in which, to use an expression common in this country, the ague has gone astray. In some instances we meet with a very peculiar type of intermittent, the double quotidian, or that form in which we have two attacks in the course of a day similar in their

phenomena and character. This is a very rare form of the disease; I have never observed it but on one occasion in the Meath Hospital. There is also another kind which has been called double tertian. In this variety, you have a paroxysm every day, but the coincidence between the nature of the paroxysms and the period of invasion is to be observed only every second day, just as if there were two tertians, each attacking on alternate days. You have a paroxysm every day, one to-day at twelve o'clock, which is of a mild character, another to-morrow at the same hour which will be severe; on the third day the paroxysm of the same character as on the first, and in this way you have, as it were, two fevers having their access on alternate days. There are some other varieties of intermittent fever, but they are so exceedingly rare that I will not take up your time in describing them.

The duration of an intermittent fever is subject to considerable variety, but, in general, they are tedious if not interfered with, and apt to assume a more or less obstinate character. Agues which occur in spring and summer subside much sooner, *cæteris paribus*, than those which occur in autumn. Of all forms of the disease, quartan appears to be the most obstinate; there are many cases of it on record which have lasted for years, indeed, it seems to be well established, that it may continue for a great length of time. In one very remarkable case it lasted for forty years, and during that time the patient had his paroxysms every third day. Schenck gives the details of a case in which the patient had quartan from his birth, where, in fact, the disease seemed to be congenital. There is another remarkable one bearing on the same point. A female, labouring under an attack of tertian ague during her pregnancy, observed that on the alternate days, when she was well, the fœtus appeared to be affected in a very singular manner. Violent convulsive motions of a certain character occurred at fixed periods, lasted for a certain length of time, and then regularly subsided. It would appear, that in this instance the fœtus was afflicted with the same disease as the mother, nor is there any thing improbable in the supposition. We have well established cases of the existence of ague from birth, and there is no reason why the nervous system of a child should not be modified in this way, as well as that of the mother. We know, too, that the fœtus in utero is liable to many diseases, even of a chronic character, and it has been proved that they may have chronic inflammation of the mucous membrane of the digestive tube, abscess, tubercles, small-pox, and many other affections.

I shall say but little with respect to the exciting causes of ague. A common opinion, and one which is taken up by too many medical men, is, that ague is generally produced by unwholesome emanations from marshes, or what has been termed malaria, or marsh miasma. It cannot be denied, that in many cases we observe a coincidence between the existence of marsh miasmata and endemic ague; but it is to be investigated how far we are to believe in the existence of any separate and distinct miasm. There are numerous

facts to show that other causes, the nature of which is much more easily understood, are capable of producing all the varieties of intermittent fever. Cold and moisture are fully capable of producing ague, independently of any miasm whatever. In Sir G. Baker's and Sir Gilbert Blane's writings you will see that in several instances no ague was found to exist in the marshy districts, while in hot and dry situations it was endemic. My friend Dr. Dundas, who resided for some time at Bahia, mentioned to me some very remarkable facts connected with this circumstance. The town of Bahia is built in a low marshy valley, which is separated from the sea-shore by a range of high hills. The streets are narrow and filthy, and, being exposed to the direct influence of a burning sun, it has every thing in it calculated to produce unhealthy emanations. Yet it is a curious fact, that in the town ague is almost unknown, while, on the neighbouring highlands which separate the valley from the Atlantic, and where the soil is dry, and the air pure, it is endemic. When Dr. Dundas went to reside there, these hills were pointed out to him by the natives, and he was told that there was the ague country. This appeared so strange to him, that he did not believe it until some time afterwards, when he had an opportunity of correcting his error. A merchant who lived in the town with his family, all in good health, was attracted by the beauty of the hills, and wished to purchase a country house which had every advantage of situation to recommend it. Before he removed his family thither, however, he was anxious to have Dr. Dundas's opinion as to the salubrity of the place. On arriving there Dr. Dundas found it in every thing calculated apparently to promote health, a fine, dry, luxuriant soil, a situation commanding a range of beautiful scenery, an airy house, and a plentiful supply of excellent water. Under these circumstances, he thought he might safely recommend the place, and the gentleman removed thither accordingly. In the course of ten days the following occurrences took place. The master and one of the servants were attacked with violent intermittent fever; and an old female, of a low weakly habit, got symptoms of typhus; two other servants were attacked with dysentery, and an European gentleman who had joined the family a few days after their arrival got intense pneumonia. In fact, almost every individual of the party was laid up with some disease or other. Of this variety of disease, occurring in cases where the exposure has been the same, we have many analogous examples. You will find in the writing of Dr. James Johnson, how an apparently similar case will produce different diseases in different individuals. It may be observed, however, that although these diseases differ in their seat, they do not differ in their nature, being all referable to the same pathological condition, namely—irritation of organs.

I shall now proceed to consider the treatment of intermittent fever. Suppose you are called to treat a case of ague, and have ascertained its history, type, and period of access, &c., what are you to do? Here, gentlemen, let me entreat of you to lay this down for yourselves as a rule never to be departed from, that before you

prescribe the slightest medicine you first make an accurate and perfect survey of the state of the viscera. I do not mean to say that you may not be able in a great many cases to arrive at a proper and successful mode of treatment without taking this trouble; but, if you succeed in many, you will be certainly disappointed in some, if you neglect the state of the viscera; whereas, by adopting this rule you will always be on the safe side. Depend upon it, that if medical men were to do this more generally than they do at present, it would render their practice more certain, and tend to raise the character of medicine. The great error of medical practice is the absurd habit of prescribing for names and not diseases.

Well, then, the first thing you do is to ascertain precisely the nature of the disease, and see whether it be an original and essential intermittent, or one of a secondary nature and depending upon local lesion. You are aware that we have many cases of continued fever, which are the results of local lesion, and we may in like manner have the symptomatic in intermittent as well as in continued fever. Now the whole nicety of treatment turns on this. If the case be one of the essential kind we know the remedy which will answer, if not in all, at least in the majority of cases. If it be symptomatic, your treatment must be directed to the removal of the local lesions. It would be wrong to begin the treatment of a case of urinary intermittent by giving bark; it would be equally absurd to pursue the same course in any other intermittent depending on local irritation. It would be only exasperating the disease, at the same time that its cause is allowed to continue. If the patient has come from a situation where ague frequently prevails, there is a chance that it is a case of essential, and not of symptomatic, disease. If ague be raging as an epidemic at the time, it is very probable that any one particular case is of the same description. But you are not for these reasons to conclude that it is a case of essential intermittent; if you did so, without investigating its precise nature, your practice would be empirical, and probably unsuccessful. I recollect on one occasion, after the subsidence of an epidemic fever, we had a vast number of cases of ague in this city. Previously to this period the few cases we had were generally imported, the patients being in almost every instance poor Irish labourers, who had been engaged during the autumnal season in agricultural labour through the fenny districts of England. These poor fellows frequently exhibited symptoms of ague on their arrival in Dublin, and of such the majority of our patients consisted; but, after the epidemic of 1826, we had a vast number of cases in and about Dublin. At this time we frequently had from a dozen to twenty ague patients in the Meath Hospital together.

During the early part of this period, a patient came in with all the symptoms of well-marked tertian. He had a regular cold, hot, and sweating stage about noon on every second day, and a distinct interval of apyrexia. In this case I made no examination, but, taking it as one of a simple essential character, I put the patient at once on the use of quinine. On the day of the next access, I was

surprised to find that the paroxysm was much more severe than the one which preceded it. On the next day I observed that the type had changed, and that it was now a double tertian. This led me to suspect that something was going wrong. On turning down the bedclothes to make an examination, I was struck with the enormous size of one of his legs, and found that he was labouring under phlebitis, with œdema. I immediately ordered his bark to be discontinued, applied leeches along the limb, and had him put on a strict antiphlogistic treatment. In the course of a few days the swelling of the limb was removed, and the intermittent disappeared. I mention this case to put you on your guard. In entering on the treatment of a case of intermittent, you must investigate several circumstances; first, the character of the paroxysms, and whether they are of a mild or severe description, and how long they last; next, the duration of the whole disease; thirdly, and this is a point of importance, whether the intermissions be complete or not; and, lastly, the nature and extent of the complications. By a careful investigation of these circumstances, you will be able to arrive at the precise character of the disease, and can judge of the probability of the existence or non-existence of internal lesion. In my lecture of Saturday, you will recollect I stated that the liability to internal disease was in proportion to the duration of the ague, and the violence of its paroxysms. If the disease be recent, and the paroxysms not violent, there is nothing, perhaps, to contra-indicate the use of bark. By carefully examining the intermissions, you will arrive at some very important conclusions. You will find them either perfect or imperfect. Now, if they are imperfect, if there is not a state of complete apyrexia between the paroxysms, it points out one of two things, either that the disease was from its commencement more of a remittent than of an intermittent character, and consequently less under the influence of bark, or that the febrile condition is kept up by some local internal disease. If you find on examination that in the early period there was complete apyrexia, but that this is no longer the case at the time you see the patient, the great probability is, that it is some organic visceral affection which keeps up the fever during the intermissions. You now proceed to examine the state of the viscera, and first the nervous system. Observe whether there is any headache, lesion of intellect, or stupor; whether there is any affection of the muscular system, any derangement of sensibility, any throbbing of the vessels of the head and neck, or any signs of inflammation of the brain. Go then to the chest, and carefully investigate its actual condition. Here you will derive great advantages from the use of the stethoscope and percussion. In all cases of any considerable duration, never omit making this examination, for in ague, as in continued fever, visceral affections are remarkable for their latency. A patient may have the lung in a very dangerous state, and yet during the periods of apyrexia will not present any remarkable symptom. I have seen cases where the lung was actually solidified from repeated congestion, and yet the cough was but trifling, and the distress of breathing slight

during the intermissions ; but when you applied the stethoscope to the posterior and lateral portions of one or both sides of the chest, they were found to be completely impervious. You then proceed to examine the belly. See whether it is flat, or prominent and swelled ; whether it is tender on pressure, and where the tenderness is greatest ; inspect your patient's tongue, and ask him has he thirst, nausea, or diarrhœa ; observe whether the solid viscera of the abdomen are enlarged or not, and carefully note the condition of the liver and spleen. It is necessary to make this examination in every suspicious case, and you can do it easily and rapidly. In the majority of cases which have lasted for any considerable time, you will hardly fail to detect some internal lesion.

Here we arrive at a very important question. Does the existence of any amount of visceral lesion necessarily contra-indicate the use of bark? The answer to this question is—the mere existence of visceral lesion does not contra-indicate the use of bark, if the lesion be slight and recent, and does not keep up febrile irritation after the cessation of the paroxysms. If the lesion has not arrived at that state in which it becomes reciprocally cause and effect, it does not present any obstacle to the employment of bark. A slight degree of bronchitis may be present, but you should not on that account suspend the use of the specific ; a trifling enlargement of the spleen may exist, but this should not alter your practice. In these cases the lesions are trifling, and do not react on the system, and in all such cases bark may be given with a reasonable prospect of success.

I shall commence the treatment of ague by at once recommending to you its great specific. If there is any thing in medicine which deserves the name of a specific, it is bark in ague. I do not mean to say that this is to be received in the full sense of the term, for there is no such thing as a complete specific. There are many cases of symptomatic ague made worse by bark, and the same thing is to be said of cases of essential ague, where the local lesions stand in the light of cause and effect. The more the case is free from visceral disease, the more simple and essential in its character it is, the more certain will be the action of quinine. Many persons commence the treatment of a case of ague by unloading the bowels. To this practice there is no objection, and it has many advantages. It is not however absolutely necessary, and may sometimes be dispensed with ; but if you do it gently, avoiding strong irritating purgatives, it is an excellent mode of preliminary treatment. You may give the bark in large doses at considerable intervals, or you may give it in small and frequently repeated doses. There is some difference of opinion as to the comparative value of these two modes. Our experience with respect to this point is, that you will cure an intermittent more certainly, easily, and with less loss of quinine, by giving it in large doses at considerable intervals. Thus, if you treat a case by giving ten grains of sulphate of quinine in a single dose once a day, you will cure your patient sooner than if you gave the same quantity in a day in divided doses.

You will find it stated in books, that the interval between the paroxysms is the best time for giving bark. Of this there can be no doubt; not because the powers of bark are greater at one time than another, or that they are impaired during the paroxysms, but it is difficult to give a patient in that state any kind of medicine, for the stomach is generally irritable, and will not bear it. But it is a fact that you can give it at any period, and it is remarkable how rapidly its febrifuge properties will sometimes act. There is no doubt that its effect may be, to a certain extent, produced in the course of an hour. I have seen cases with bad paroxysms to-day, and on the next day have observed that a dose of quinine, given during the paroxysm, has produced a sensible effect.

It is a matter of little consequence what way the quinine may be administered. You may give it in pills, in solution, or in the form of enema. The latter seems to be the most convenient mode of giving it to children who will not swallow the pills or the solution. If you take four ounces of water, four grains of sulphate of quinine, a few drops of dilute sulphuric acid, and a few drops of laudanum or liquor muriatis morphiæ, you have an excellent febrifuge injection, and you can vary the proportions of quinine and opium according to the age and condition of the patient. In employing this injection, you should recollect that it is given to be retained; the bulk should therefore be small, and it should be of a medium temperature, neither hot nor cold. If you find on the following day that the paroxysm, instead of coming on at twelve, is delayed until two o'clock, it is a sign that the bark is acting favourably; if, on the next accession, the paroxysm is still further protracted, you may be sure the patient will soon get well. In some cases the disease has been cut short by giving a single large dose; but as far as I can judge, it appears to me that the best way is to remove it gradually. It is a curious fact, that when the ague has been arrested at once, new local diseases have supervened. In all cases, therefore, where the symptoms have rapidly and suddenly disappeared, you should be on the look-out for new symptoms.

Of all remedies for ague, quinine appears to be the best. I have made a number of comparative trials of sulphate of quinine, Fowler's solution, and the per-ferro-cyanuret of iron. All these remedies were found to have a good effect; among them, however, quinine holds the first rank, next in value is Fowler's solution, and then Prussian blue. I may remark here, that the introduction of Prussian blue is a great improvement in medicine. It may be given with safety, it is a remedy of no ordinary power, and from its cheapness is particularly adapted for dispensaries and institutions where the funds are limited. We have given it in doses of from a scruple to half a dram three times a day, and though it is inferior to bark, still it is a remedy from which much advantage is derived.

At our next meeting I shall speak of the endermic mode of using quinine, and lay before you some curious researches on this subject made in France; and these, with a few additional observations, will conclude the treatment of intermittent fever.

LECTURE XXXIX.

Use of quinine—Disease not a simple increase or decrease of vitality—Bark a specific in ague—To be given in the period of apyrexia—Large doses at considerable intervals—Arsenic followed by dyspepsia—Mercury, its effects in ague—Treatment during the paroxysms—Dover's powder, heat, laudanum, carbonate of ammonia—Pressure on large arteries to arrest the cold stage—Used in a case of hydrophobia with temporary relief—Gastric-intermittent—Endermic mode of using quinine—Bleeding in the cold stage—Generally with safety and advantage—Supervention of other diseases.

At our last meeting I spoke of the use of quinine in intermittent fever. With respect to its *modus operandi* I may observe, that we know as little of it as we do of that of other remedial agents. The truth is, that this part of medical knowledge is still involved in great obscurity, and that we know almost nothing of the mode of action of various medicines. But that different remedies have different actions, and that there are such things as specifics, seems to be established, and this fact of the existence of remedies, which may be termed specific, is a strong argument against the formula which has been proposed for all diseases by the school of Broussais, *viz. that they are reducible to a plus or minus degree of local vitality*. If this were the case, the peculiar applicability of the properties of bark and mercury, for the cure of certain affections, could not exist. The fact of particular remedies possessing a powerful influence over certain diseases shows, that there is something in those diseases more than a mere increase or decrease of vitality, and that in such instances there is some modification in the phenomena of vitality, which only admits of being modified, or removed, by the action of certain remedies. One of the most remarkable instances of the adaptation of peculiar remedies to peculiar diseases, is the use of bark in pure ague, and I believe I may safely say, that, in pure simple intermittent, bark may be justly called a specific.

In speaking of the use of bark in intermittents, I showed that it might be given either during the paroxysms or the intermissions, but that it was much better to give it during the period of apyrexia, on account of the greater facility of exhibition, and because there was less chance of its being rejected by the stomach. Having alluded before to the different modes of using bark, I shall not take up your time with this part of the subject. Happily for medicine, quinine has completely superseded the use of bark; but where you cannot get quinine, you may employ the bark itself, either in the form of powder, electuary, decoction, or tincture.

With respect to the question whether it is better to prescribe large doses of quinine at considerable intervals, or to give it in small and repeated doses, I have stated, that as far as our experience in the Meath Hospital went, it was decidedly in favour of the practice of giving a large quantity at once. The best way appears to be, to order the patient to take ten grains of quinine at a single dose, about two or three hours before the paroxysm comes on. In Italy,

where ague of a severe character is frequently met with, the quantity given at a time is much larger, fifteen and twenty grains being often taken at a single dose by an adult. In some cases as much as one hundred and eight grains have been taken before a cure has been accomplished. You can easily understand why such large quantities are prescribed, when you recollect that in Italy and other warm countries, where pernicious ague prevails, it is of the utmost importance to introduce the remedy as soon as possible into the system, for if you are not able to cut short the disease soon after the first or second paroxysm, the patient may fall a victim to its violence. Professor Speranza, in his excellent medical reports, defends this practice, and observes, that it is absurd to maintain that the doses of medicines should be the same in all countries; and when we consider the different intensities of particular diseases in different countries, we must allow that there should be some variation in the dose.

I spoke of arsenic as one of those remedies which exert a considerable influence over intermittent fever. This medicine, which was imported from the East, was first strongly recommended to the profession by Dr. Fowler, who has given a formula for employing it, and there is no doubt that it possesses febrifuge properties. It is stated, that, in consequence of the establishment of a smelting house for copper, in a particular district of Cornwall, ague, which frequently appeared in that situation, has entirely ceased. You can understand this by recollecting, that in the smelting of copper ore, arsenic fumes are often copiously extricated.

Sir Gilbert Blane states, that in the ague which prevailed among our troops at Walcheren arsenic was used with good effects in cases where bark would not be borne. In these cases it was generally prescribed in combination with opium and aromatics. Under certain circumstances, also, it may be employed with greater advantage than bark; but as a remedy it is decidedly inferior to bark, and there is one objection which applies to it, but not to bark, namely, that in several cases where arsenic had been employed in the cure of this and other diseases, the patients, after their recovery, fell into a state of bad health, became weak, emaciated, and presented remarkable derangement of the digestive organs. Some of these persons sunk in the course of a few years, others continued to drag on a miserable existence. In several instances, where large quantities of arsenic had been taken for the cure of skin diseases, I have observed that the patients never recovered from its effects, and there can be no doubt that it is a substance highly inimical to the system, and that its long continued use is productive of mischievous results. We should, therefore, decline its employment whenever we can dispense with it, and though we may not be able to procure Peruvian bark, or sulphate of quinine, we should recollect that there are many other astringent barks possessed of febrifuge properties, and which may be employed with safety and advantage.

It has been proposed to cure intermittent fever with mercury, and this requires a few observations. In cases where mercury has been

used instead of bark, it has been observed that the disease has been checked for some time, but that as soon as salivation disappeared the disease returned. It produces a suspension of the paroxysm, but does not eradicate the complaint. It is related, that in two ships-of-war, whose stock of bark happened to be exhausted at a time when ague prevailed extensively among the crews, the medical officers had recourse to mercury, and it was found that as soon as salivation was established, the paroxysms of ague disappeared. But three-fourths of the patients thus treated relapsed when the salivation disappeared, and this happened even in cases where mercury had been used three or four times for the same affection. Mercury is certainly of great utility in removing some of the diseases consequent on intermittent, but it does not appear to possess any specific power over the disease. The fact of the subsidence of the paroxysms of an ague on the establishment of mercurial action has many analogies, for we all know that there is a vast number of diseases which will subside as soon as the mercurial action is set up in the system.

I shall now draw your attention to some other circumstances connected with the treatment of intermittent fever, and first with respect to the management of the paroxysms. In the cold stage, there is scarcely any thing necessary but to keep the patient well covered, and give him mild warm drinks. In very bad cases, the use of the warm foot-bath, the application of warm bricks, or stupes, to the feet, and small quantities of warm wine, or carbonate of ammonia, will be required; but I would advise you to be sparing in the use of internal stimulants, unless the cold fits are extremely severe. We may venture to say (and this applies to cholera as well as to the cold stage of ague) that the extent and danger of re-action and its accompanying diseases in the hot stage is often proportioned to the quantity of stimulants used during the cold stage. There is one remedy, however, to which this remark does not apply, namely, opium—this may be given with advantage at the commencement of the paroxysm. A dose of laudanum will diminish the duration and violence of the cold fit, without increasing the hot, and if combined with some preparation of ipecacuanha, its effect will be still better. What we generally give in the Meath Hospital is, from ten grains to a scruple of Dover's powder. Dr. Fordyce mentions a case of quartan ague which was cured in a few days, after having resisted the use of bark for two years. Two hours before the paroxysm came on, he gave the patient a full opiate draught combined with a small quantity of carbonate of ammonia. The effect of this was to increase the quantity of perspiration during the sweating stage, and produce a change in the character of the paroxysms. The disease then came under the influence of bark, and the patient was rapidly and permanently cured. This case is of great importance, as it shows that in some instances where the disease resists the use of bark, it may be rendered amenable to it by first making an impression on the system with opium, and then trying the specific.

A curious circumstance in the treatment of the cold stage of ague is the effect of pressure on the extremities. If you apply strong pressure to the extremities, particularly in the vicinity of their nerves, it is a fact that the rigours, even though they may be remarkably violent, will often cease. This I have seen very often. I remember when I was a pupil at the old Meath Hospital, a little boy who was labouring under intermittent, used to send for me as soon as the rigours came on, and beg of me to "hold him," as he said, while the shivering lasted. My practice in this case was to compress the anterior and internal parts of the thigh and opposite arm with all my force, and I have observed that in almost every case where pressure was employed, the hot fit came on much earlier than usual. The same thing was observed by Dr. Kelly in 1794. He advises the use of the tourniquet for this purpose, and states that, in some cases, the hot fit came on in three minutes after its application. It is difficult to explain how the tourniquet acts in arresting the cold fit; at present, I believe, we must confess our ignorance as to its mode of operation. It, however, goes to prove that ague is a nervous disease, for if it were an intermittent gastro-enteritis, as the physiological school teach, how could it be possible to remove it by pressure on the extremities? Besides, we have analogous instances of the same effects being produced by pressure in other diseases of the nervous kind. We have them, for example, in hysteria and epilepsy. In a very remarkable case of hydrophobia, which was, some years since, under the care of my father, pressure on the extremities was attended with very singular results. The patient, a female child, was brought into the Meath Hospital, with symptoms of well-marked hydrophobia, which had come on some time after she had been bitten in the foot by a rabid dog. The tourniquet was, by my father's direction, applied over the middle of the thigh of the bitten extremity, and he found that as soon as it was sufficiently tightened, the spasms ceased, and the hydrophobic symptoms disappeared, *for the patient could drink freely, and without inconvenience.* During the course of the night the paroxysms came on again; on making an examination, it was found that the tourniquet had become loose, and had slipped downwards; on being again tightened the paroxysms ceased. After thirty-six hours the tourniquet was removed, and the paroxysms again returned with all their original violence. During the course of a clinical visit, the patient was asked if she had taken any drink; she replied she had not, and *dared not make the attempt unless the tourniquet was applied.* This was immediately done, and she drank freely. In this case my father proposed amputation, but nothing was done, and the little patient died, with all the terrific symptoms of the disease. It is a curious fact, however, that, in this case, the paroxysms were arrested, and the progress of the disease clearly suspended, by the use of the tourniquet.

There is a form of intermittent, common in warm climates, and not unfrequent in this country, to which the name of gastric-intermittent has been applied by Frank, and which is generally met

with in the autumnal season. This disease is known by the presence of symptoms of gastro-enteric irritation combined with the phenomena of ague. The symptoms of gastro-enteric disease may be recognised, not only during the paroxysms, but also during the intermissions. The patient complains of thirst, nausea, and loss of appetite; there is a desire for cold drinks, the tongue is loaded, and the epigastrium tender on pressure. The skin is slightly yellow, the eyes have a semi-jaundiced appearance, and the urine is turbid and high coloured. In the treatment of this affection, Frank recommends us to commence with an emetic. I cannot speak from experience of the utility of this practice; he, however, recommends it very strongly. The plan which I would advise you to pursue is the following: you must here hold in view, that there is a combination of intermittent fever with local disease of the gastro-intestinal mucous surface, and that the latter demands your attention as much as the former. In some cases the gastric-irritation is so considerable, that if you were to proceed at once to the use of bark, you would in all probability do mischief. After having directed such measures as will abate the violence of the paroxysms, I generally begin with the application of leeches to the belly, and the employment of all the ordinary means to relieve gastro-intestinal irritation. By this mode of treatment, you will find that the gastric character of the disease will disappear in a great degree, and then you can have recourse to bark with safety. In a few cases I have observed, that even without the use of bark, all the symptoms of ague have been removed by treatment directed to the gastro-intestinal disease. If they do not, you can easily give quinine; and where the irritation has been subdued, it will act both safely and expeditiously. Allow me to repeat the circumstances under which you recognise the necessity of leeching and other measures to remove the affection of the digestive tube. These circumstances are drawn chiefly from the state of the patient during the intermissions, for in this kind of ague we seldom have complete apyrexia. Thirst, nausea, foul tongue, epigastric tenderness and fulness, a semi-jaundiced state of the skin and eyes, and turbid high coloured urine: these are the symptoms which mark the co-existence of gastric disease with ague; and, under these circumstances, I would advise you never to omit leeching, the use of purgative injections, mild mucilaginous drinks, and every other means calculated to remove gastro-enteric irritation. When you have accomplished this, you may then employ quinine with safety.

There is one point connected with the treatment of this form of intermittent which you should be acquainted with. It occasionally happens, that the symptoms of gastro-enteritis will disappear, but the tongue still continues foul. It is a common opinion in this country, that the tongue is a correct index of the state of the bowels; and the inference in such cases would be (according to this opinion) that in these circumstances it would be improper to prescribe bark. If nothing remains, however, but foulness of tongue, it should not prevent you from giving bark, for the state of the

tongue here is connected with the morbid condition of the whole system, and not with disease of the digestive tube.

I shall now proceed to make some observations on the endermic mode of using quinine. A number of curious and interesting researches have been made on this subject not long since in France, and it is necessary that you should be acquainted with them. In some cases bark or its preparations cannot be taken internally; the stomach will frequently reject the remedy; and in cases of inflammation of the digestive tube its employment would be dangerous. It has been stated, however, by those who have used it in the endermic mode, that even in cases of ague complicated with gastro-enteritis, they have been able to accomplish a great deal of good, and that quinine may be always employed in this way with safety. The first author who wrote on this subject was M. Martin, who published a memoir on the endermic mode of using quinine, in the *Archives G n rales*. In this memoir he states, that the application of quinine to a blistered surface, even where the quantity employed was very small, never fails in arresting the symptoms of ague. You are to observe, however, that quinine cannot be employed in this way in its simple state; if this were done, it would excite a high degree of irritation, and the proper effect of the remedy would not be obtained. One of the most severe and troublesome forms of ulceration I have witnessed for a long time, occurred in a case where quinine in its simple state had been applied to a blistered surface. Over the whole extent of skin to which it had been applied, numerous ulcerations took place, which resisted for the space of six weeks every attempt to heal them. But it is a singular fact, that when quinine, reduced to a fine powder, is incorporated with a certain quantity of cerate, no such effect is produced. The advantages of this mode of using quinine are, first, that it can be safely employed in cases where its internal use is dangerous, as in the case of gastric intermittent; secondly, it is stated that a smaller quantity will be required than if taken internally; thirdly, that it will act more rapidly; and lastly, that it can be employed with advantage in cases where the patients are averse to taking medicines internally. These are the grounds on which M. Martin claims a superiority for the endermic mode of using quinine. He further remarks, that it cannot be supposed to act as a counter-irritant when used in this manner, for any local irritation produced by it would exert an unfavourable influence over its effects, and render them less certain. All this may be easily understood by recollecting what the phenomena of absorption are, and how they are promoted or obstructed. Where the application of quinine has caused a considerable degree of irritation, a quantity of gelatinous fluid will always be thrown out, and this must obviously prevent the entrance of the remedy into the absorbent vessels. In proof of this M. Martin gives some cases. A robust man, aged thirty-five, had been labouring under an attack of quartan ague, for which he had taken bark internally four times shortly before he came to consult M. Martin. When seen during the hot fit of the next paroxysm,

his skin was intensely hot; he had cough, mucous expectoration, and rapid laborious breathing; considerable excitement of circulation, and a red dry tongue. Sixteen ounces of blood were taken from his arm on account of the pulmonary symptoms. In three days after, the paroxysm returned as usual, and on the next morning a blister was applied, and the raw surface dressed with six grains of sulphate of quinine mixed with a quantity of cerate. From this period he had no return of the ague, and his general health improved. In this case no irritation attended the application of quinine. He gives another remarkable case. A woman, aged twenty-three, who had oppression of breathing, pain in the chest, and cough, was attacked with quartan ague in the month of September, but recovered under treatment. The ague again returned in the following December, accompanied by hard quick pulse, difficulty of breathing, and cough. In this case also venesection was performed. The patient had then four grains of quinine applied to a blistered surface, and the paroxysms never returned. The blistered surface in this instance was somewhat red, but the irritation disappeared rapidly. In both these cases you will observe, that bleeding was performed before quinine was employed, and, so far as they go, they cannot be looked upon as unexceptionable examples of the cure of intermittent fever by the endermic mode, as from the researches of Dr. Mackintosh, we learn that many cases of the disease will recover after bleeding in the cold stage alone.

M. Lembert, another writer on the same subject, states, that in ten minutes after the application of quinine to a blistered surface on one of the extremities, a sensation of gentle warmth diffused itself along the limb towards the trunk, and so on over the whole body. The hot fit, says he, also comes on much sooner than usual, and the violence of the whole paroxysm is diminished. The sensation of warmth is coincident, according to his account, with a diminution of the symptoms and an increase of muscular power, and he thinks that the reduction of heat during the paroxysms is a necessary consequence of this mode of using bark. There is another case given, which exemplifies some of his statements. A stout young man, who had got ague during the expedition to Spain, presented himself with the usual symptoms of the disease, accompanied by foul tongue and a semi-jaundiced colour of the skin. For the space of ten days, he took from eight to ten grains of sulphate of quinine daily, but without any improvement. The quinine was then applied to a blistered surface. His paroxysm came on after the application about the usual time, but its duration was half an hour shorter than before. The next attack was also shorter, and the fifth was prevented altogether by applying ten grains of quinine at once. He was then considered to be cured, but about fifteen days afterwards, in consequence of an error in diet, he got another attack. The endermic mode was again tried, but without success; the internal use of quinine was then resorted to, and was followed by a perfect cure.

These facts do not show that the external mode of using quinine has any great advantage over the internal. Where it would be improper to prescribe it internally, it is certainly a valuable mode, but it remains to be proved whether employing it in this way be preferable to giving it by the mouth or in the form of enema.

I shall now make a few observations on the last subject connected with the treatment of intermittent fever, and this is the proposition of bleeding in the cold stage, put forward so ably by my distinguished friend, Dr. Mackintosh, of Edinburgh. In the writings of almost all the older authors it was laid down, as an undoubted fact, that the cold stage of ague was essentially a stage of debility; they saw that the cold stage of intermittent fever was attended with considerable prostration of the powers of life; they regarded it as one of pure debility, and they completely overlooked its effects on the viscera. From an accurate observation of the phenomena of the cold fit, from perceiving that the signs of a retreat of blood from the surface to internal parts were evident, from ascertaining by dissection that the viscera were overloaded with blood, and from recollecting that in chronic cases of ague visceral disease was of constant occurrence, and often the cause of death—from all these circumstances, Dr. Mackintosh was led to conclude that the old idea respecting the cold fit was a mistaken one, and that much good might be done by bleeding in the cold stage, so as to relieve the congestion of internal organs. This idea he entertained for some time without venturing to put it in practice, so strong were the existing prejudices against any kind of depletion in what was looked upon as a distinct example of debility; he ultimately, however, determined to make trial of it, and, with the self-devotion which characterises true science, first put it in practice on himself. On being attacked with intermittent fever, he boldly rejected the venerated dogmas of the schools, and had himself bled in the cold fit. The operation was performed; and, so far from any of the dreaded results occurring, it was followed by great relief. Since that time, the remedy has been extensively tried, and he has given an interesting and detailed account of it in his admirable work on the practice of physic.

An incorrect view of Dr. Mackintosh's opinions on the treatment of intermittent fever has been taken by some persons. It has been said that he neglects and disregards bark, and that he considers bleeding in the cold stage as the only remedy required for the cure of ague. This is not true. All that he proposes to establish is, that there is congestion of the viscera during the cold stage, and that although the phenomena of debility are present, blood may be taken away safely and with the greatest possible relief in many instances. Of course all those persons who are attached to the practice of studying and prescribing for names rather than actual diseases, are still opposed to Dr. Mackintosh's treatment, for nothing shocks them so much as that which has a tendency to overturn and expose their favourite dogmas. For this reason they looked upon the new opinions as heterodox, and the practice as a danger-

ous innovation. Notwithstanding all this, no sensible practitioner can entertain a doubt that Dr. Mackintosh has conferred a most valuable boon on medicine, first, by demonstrating the real nature of the cold stage of ague, and, in the next place, by showing that bleeding may be performed in it with safety and advantage.

It is a question, however, which remains to be discussed, whether we can have recourse with safety and success to this mode of practice as the ordinary treatment of intermittent in this country. My own impression is, that, in this country, the practice is unnecessary in the great majority of cases, and that you can remove all the simple cases of ague with certainty and safety, by means of the specific alone. I told you in a former lecture, that I had endeavoured to render my experience on this subject as extensive as possible, and that during the epidemic ague of 1827, I had made trial of Dr. Mackintosh's practice on not less than one hundred patients. Now the general conclusions which I was induced to draw from an accurate examination and comparison of these cases, were, that in the vast majority, bleeding in the cold stage of an intermittent may be performed with safety; that there is very little or no danger of the patient dying of debility, as was formerly apprehended; and that, in many cases, this treatment was found to diminish the violence and ameliorate the character of the paroxysms, and, in some, had the effect of completely arresting the disease. Dr. Mackintosh has witnessed several cases in which bleeding in the cold stage has cut short the disease at once, and of this I am fully convinced, but I believe it to be a rare occurrence. I have certainly seen it in a few cases. In one instance, where the ague had resisted the ordinary treatment for a considerable time, the patient was cured at once by bleeding freely in the cold fit. As far as my recollection goes, the number of cases in which this singularly favourable termination occurred, did not exceed three or four.

But, you will ask me, did I observe any bad effects to result from this practice? I have seen some cases in which the cold and others in which the hot fit were increased in violence after bleeding in the cold stage. I have seen several cases in which the paroxysms were brought more closely together, and the period of their occurrence anticipated. Now this is a very unfavourable occurrence, for the nearer an intermittent approaches to remittent fever, the more unmanageable it is, and the less is it under the influence of bark. This observation has been further confirmed by my friend Mr. Gill, who went into the fenny parts of Lincolnshire, during the prevalence of an epidemic ague, to put the practice to the test. In a communication which he made to me on this subject, he states that he felt quite certain that he had it in his power to convert many cases of intermittent into continued fever by bleeding in the cold stage.

We observed also in the Meath Hospital some other effects, which, however, do not particularly apply to bleeding in the cold stage, as they have been observed to follow the employment of other measures,—I allude to the supervention of new diseases. I have

seen cases of intense pneumonia, of inflammation of the brain, and of gastritis, occurring immediately after the disease had been cut short by bleeding. This, however, is no argument against the practice of Dr. Mackintosh, for the same thing might happen where the paroxysms had been arrested by bark. I have hazarded a conjecture in one of my clinical lectures, that if bleeding in the cold stage were useful, it would be chiefly in the intense agues of warm climates. Where life is threatened by the violence of the congestion, or by the excess of inflammatory action in warm countries, in such circumstances I think bleeding may be looked upon as a valuable remedy in intermittent fever. Since that period, Dr. Mackintosh has received a number of communications from the East Indies, and from these it appears that the practice has been attended with great success.

I may remark, in conclusion, that the foregoing observations apply to almost all the cases which came under our care in the Meath Hospital, for in almost every instance we were obliged to have recourse to bark after the use of the lancet, and we could not say that the patients required less quinine than those who had not been bled. However, in all cases of intense congestion, but particularly in the pernicious ague of warm climates, I think this practice of great utility, and I am almost inclined to think that it may be employed with advantage in cases where bark, after a sufficient trial, does not exert any influence over the disease. Under all circumstances, the medical profession is deeply indebted to Dr. Mackintosh, for being the first to demonstrate the real nature of the cold stage of ague, and that the visceral congestion may be relieved by bleeding; and it is to be remarked, that my chief trials of the treatment were made on the cases of *an epidemic fever*, a circumstance which of course diminishes the value of my results, as arguments against the treatment of sporadic or ordinary cases.

LECTURE XL.

Continued fever—Varieties of fever infinite—Typhus fever—Symptoms of typhus—Petechiæ, sign of typhoid character—State of the tongue various—Progress of the disease—Typhus produced by injection of putrid substances into the veins—Hemorrhage from the intestines, &c.—Opinions on fever—Prognosis—Phenomena arising from each system—Jaundice an unfavourable sign.

Before I enter on the consideration of exanthematous diseases, it will be necessary to examine the subject of ordinary continued fever. My reason for this is, that the phenomena of continued fever bear a closer relation to those of intermittent than they do to those of the exanthematous; and as I have introduced the subject of fever by lectures which apply more obviously to the pathology of continued than to that of exanthematous fevers, I think it better to conclude this part before we proceed to the rest.

I do not know of any subject so difficult to lecture on as fever. It is, in the first place, next to impossible to give any description of it which will apply to all, or even to the majority of cases. The varieties of fever are infinite. It varies according to situation and climate, to the peculiar character of the epidemic, the constitution and habits of the individual, and the numberless complications of local disease. One of the best modes of examining fever is to take a brief but general sketch of the usual symptoms of what is called typhus, and, proceeding thence analytically, to review fevers of a nervous type, or those in which the symptoms are chiefly referable to disease of the brain and spinal cord, together with their respective nervous connections; next, to examine those fevers in which there is a predominance of diseased action in the respiratory system; and, lastly, to ascertain the character of those in which the digestive tube seems to be principally engaged.

You will recollect a principle which I have more than once impressed upon your attention, that we are to look on these fevers as general affections of the system, complicated, in the vast majority of instances, with local disease; and that I stated that there was the greatest possible variety in the number, seat and extent of these local affections. In some cases, they make their appearance almost at the commencement of the fever; in others, they supervene at a more advanced period. In some instances, they are confined to a single system, or even a single organ; in others, the morbid action extends to all the splanchnic cavities.

The symptoms of typhus may be divided into two classes—the precursory or latent, and the actual or manifest. The precursory are, more or less prostration of strength, a feeling of malaise without decided illness, an unusual paleness, a sensation of coldness and perhaps some shivering, a sense of weight about the head, indifference to objects of business or pleasure, troubled sleep and frightful dreams, some nausea, foulness of tongue, and loss of appetite. These symptoms may continue from a few hours to three or four days. In other cases, you will see the phenomena of typhus setting in at once, and with an awful rapidity. This occurs chiefly in cases where a severe epidemic is prevalent, or where the miasm of fever exerts its influence over bodies of men closely cooped up together, as we see exemplified in camp and jail fever.

Typhus fever frequently sets in with a state of the system closely resembling inflammation. The skin is hot and dry, the pulse full and bounding; and this often leads the practitioner to think that he has to deal with ordinary inflammatory fever, and he bleeds with the expectation of cutting it short; but it too commonly happens that we are very seldom indeed able to effect this desirable purpose by blood-letting. You may succeed in modifying the symptoms, but you cannot arrest the disease. It is, in fact, a question, how far general bleeding is admissible in the great proportion of cases of typhus. I shall examine this point more particularly when we come to the treatment of fever. If you take blood from the system generally in this apparently inflammatory condition, you

may find, in some instances, that the clot will be of a red colour, and crusted at top with a slight coat of buff; in other cases, you will perceive that the quantity of serum is trifling, the clot of a dark red colour, easily broken up, and without any buff.

Whether bleeding has been performed or not, the pulse begins to alter about the third or fourth day, and this simulated appearance of inflammation goes away; then comes a train of nervous symptoms—*anxiety, restlessness, delirium, tinnitus aurium*, with an augmentation of the febrile symptoms towards evening. In the morning the patient feels better; and this is almost always the case, for we seldom meet with a case of typhus without morning remissions. In some cases the symptoms are aggravated on alternate days, so as to bear some resemblance to double tertian. Sometimes the exhalant vessels of the skin are in a state of great activity, and the patients sweat profusely almost from the commencement of the disease. These cases we have found in the Meath Hospital to be extremely tedious. In the greater number of cases the skin remains dry, and this often continues through the whole disease up to its termination. With respect to the appearance of *petechiæ*, they may occur at an early period, or not until towards the close of the fever. The appearance of *petechiæ* has one particular value, as being a symptom most commonly connected with fevers of an essential type, and rarely with those of a simple inflammatory character, or where the fever is symptomatic of local disease. Thus, we very seldom meet with *petechiæ* in the fevers which accompany *cerebritis*, or *pneumonia*, or *gastro-entéritis*. On the other hand, where the inflammation of these organs is secondary, and supervening on typhus, *petechiæ* are exceedingly common.

As the disease advances, we observe other symptoms. The patient has more or less stupor, sighs frequently, answers briefly and impatiently when questioned; his eyes are red, glazed, and filled with the lachrymal secretion; his nostrils obstructed with adhesive mucus, which he repeatedly attempts to remove; he is sometimes quite deaf, without any apparent affection of the ear; his lips are dry and chapped, and his teeth and gums crusted with a dark sordes. If you ask him to put out his tongue, he moves it slightly forwards, being unable to thrust it out fully, from its dry state and the injury of its muscular functions. Its upper surface is generally of a dark brown colour, or even black; its mucous covering chapped and inclined to bleed, and feeling quite dry and relaxed; but there is an infinite variety in the state of the tongue in typhus fever. Not unfrequently it is moist; and I have seen cases in which it was nearly natural throughout the whole course of the disease. In many cases it is red, dry, tremulous, and pointed; and in such cases we have generally observed symptoms of *gastro-intestinal irritation*. But it is a well-established fact, that we may have fever with *gastro-intestinal disease*, and yet without any characteristic appearance of the tongue. In typhus, the tongue is not a correct index of the state of the intestinal canal; and if you were to take it as the grounds of your diagnosis, you would often fall

into error. You should, therefore, as I have before taught you, study the appearance of the tongue in fever more as indicative of the general condition of the patient than of the state of the gastrointestinal membrane. You may have extensive disease with a natural tongue, and you may have a morbid state of the tongue without any appreciable intestinal lesion. You will often see in the advanced stage of fever a red, dry, and chapped tongue become pale, moist, and smooth, under the use of wine, carbonate of ammonia, and other stimulants; and yet, in such cases, if you were to judge by the tongue alone, you would say that there was inflammation of the intestines, and that the employment of stimulants was dangerous; and, indeed, if it was ordinary inflammation, we know that it would be exacerbated by stimulants.

In some cases of typhus there is thirst, and a desire sometimes for hot, sometimes for cold, drinks; in other instances, the patient drinks very little, and never complains of thirst. At one time wine is relished, at another time it is not. The skin varies as to its condition, being sometimes bedewed with profuse clammy perspirations of a well-known odour; but in the greater number of cases it is dry and harsh. Sometimes there is diarrhœa, tympanitis, and tenderness of the belly; occasionally it is soft and flaccid, and pressure is borne without inconvenience. The state of the urine, which engrossed so much attention in former times, cannot be looked upon as a diagnostic in typhus. In bad cases, it is, as well as the stools, passed involuntarily. This at first occurs during the patient's sleep, but as the disease advances it is passed unconsciously. It is not uncommon, on the other hand, to meet with retention of urine.

As the symptoms advance, the patient lies entirely on his back, quite indifferent to every thing, with his lower extremities separated, and the trunk constantly sliding down in the bed from debility. The eye is dull, without meaning, and of an ecchymosed appearance. During his brief and unrefreshing slumbers, the eyelids are only half closed, and the eyeball is turned upwards. The delirium, feebleness of pulse, and prostration of strength, increase. A quantity of liquid fœces is discharged involuntarily, the belly is tympanitic, and hiccup comes on, which generally proves a very distressing symptom. At this period, also, we frequently have hemorrhagic discharges from various parts of the body—the nose, stomach, bowels, and urinary organs. If the patient holds out for any considerable length of time, bed-sores begin to form on the back and loins, which are frequently surrounded by an erysipelatous border, and have a great tendency to run into mortification. The patient gradually gets weaker, his extremities become cold, his pulse thready, irregular, and indistinct; the hiccup increases, and there is sometimes regurgitation. He now begins to sink rapidly, his eye is glazed, his jaw fixed, his face Hippocratic, his stupor increases, and he dies with tracheal rattle.

The foregoing is a tolerably correct account of the symptoms of ordinary typhus, but of course it cannot be looked upon as complete, for the symptoms of this disease are remarkable for their

great variety. With respect to the prognosis, it is also found to vary. Some patients will have almost all the symptoms above-mentioned, and yet may recover; but, generally speaking, the prognosis under such circumstances is unfavourable. The worst symptoms are those which point out lesions of the nervous system, and a morbid condition of the fluids. All the phenomena of nervous derangement, delirium, coma, subsultus tendinum, and spasms, are to be looked upon as unfavourable; and the same may be said of bad petechiæ, hemorrhages from the intestines and urinary organs. In cases presenting these symptoms, the prognosis is always of the worst kind. The most malignant form of typhus is that which arises among bodies of men closely crowded together—as we see in the camp, jail, and hospital fevers. Here the disease appears to be the result of an excessive and highly-concentrated dose of the poison of typhus. Where a number of persons are confined together in close apartments, you can easily conceive that the poison of the disease will act with tenfold severity, and it is in such cases that we often observe the phenomena of local inflammation to be most extensive and violent. I have before alluded to the experiments of MM. Gaspar and Magendie, and mentioned the fact of fever, of a distinctly typhoid character, being produced by the injection of putrid substances into the veins of animals. Now, in those cases, it was observed that the greater and the more concentrated was the dose of putrid poison, the more closely did the consequent fever resemble bad typhus, and the more violent and extensive were the local inflammations. Now, if these local inflammations are in proportion to the quantity and concentration of the dose of poison, you can easily conceive why local disease should assume such threatening appearances in the jail and camp fevers. This excess in quantity and concentration in the virulence of the typhus poison, I look upon as the true cause of the malignant nature of these fevers. And if we take the reverse of these cases—if we look to the remarkable effects which result from separating the sick from those who are in health—we have an additional light thrown on this subject. In cases of camp fever, it has been repeatedly observed, that, when the camp was broken up, and the sick separated into different parties, the fever totally disappeared, although the patients happened to be exposed to bad weather and the jolting of carriages.

Before we proceed to analyse the symptoms of typhus as they appear in the nervous, vascular, respiratory, and digestive systems, it will be necessary to make a few observations on petechiæ. On this subject there are two opinions: one is, that they are analogous to the exanthematous eruptions, and, as it were, an attempt of nature to relieve the internal disease by external revulsion. Another opinion is, that they proceed from a weakened state of the cutaneous capillaries, accompanied by a dissolved condition of the blood, and that they are in fact a kind of passive ecchymosis. I am inclined to think that we may adopt both opinions. There can be no doubt that petechiæ are sometimes of an exanthematous cha-

racter. We see them at an early period of the disease, when there is but little debility, and where there are no other signs of a morbid condition of the fluids; and we know, too, that they are sometimes productive of relief, and that they form elevations, and desquamate like the exanthematous eruptions during the progress of the disease. All this proves that petechiæ are sometimes to be looked upon as bearing a close analogy to the exanthemata; on the other hand, we may find them without any of these characters, and presenting a livid or black colour. Quarin divides such into three classes—the red, the livid, and the black. The black are of the worst character, the livid are less dangerous, and the red are the most favourable, though he does not say that they are entirely devoid of danger. It is the red petechiæ, however, that are to be considered as having a resemblance to exanthematous eruptions. It sometimes happens that they are of very considerable size, simulating the appearance of purpura hemorrhagica. In the advanced stage of fever, large petechiæ of a livid or black colour are dangerous, and point out a morbid alteration in the fluids. On the other hand, the earlier they appear, and the redder and smaller they are, the better; but when they occur at an advanced period of the disease, are of a dark colour, and accompanied by hemorrhages from the bowels or urinary organs, they may be looked on as indicating an unfavourable prognosis. Huxham looks upon them as sometimes critical; I suppose he alludes to the first or exanthematous description. Pringle is of a different opinion. Our experience in the Meath Hospital does not enable us to make any other remark on this subject, than that we have seen them followed by some relief, but no exact or distinct crisis. The worst form in which they can appear is that of vibices, or large dark spots like purpura hemorrhagica; and in this form, as Huxham observes, they afford grounds for making a most unfavourable prognosis.

It is hardly necessary for me to state that persons differ exceedingly in their power of bearing up against an attack of fever. It is a fact, almost universally known, that one man may be favourably circumstanced for resisting the disease, while another may be a bad and unfavourable subject. Any thing that has a tendency to weaken and depress—as excessive apprehension, exhausting labour of body or mind, the debility brought on by mercury, bad food and foul air, grief, care, and other depressing moral causes—all these are circumstances which, generally speaking, render fever exceedingly dangerous. Inhabitants of towns are worse off in this respect than those who live in the country: this is a fact which has been well established.

Let us now enumerate briefly the symptoms of typhus, arranging them in separate groups, according as they appear in the nervous, respiratory, or digestive systems. First, with respect to the nerves. We observe in typhus a remarkable prostration of strength, and it has this peculiarity in it, *that it cannot be referred to a local cause.* This is what has been termed direct prostration, in contradistinction to that which occurs as a consequence of various local inflamma-

tions. It would not be correct to look upon it in the same light as you would the prostration which attends simple primary inflammation of the digestive system, or peritonitis; for we know that bleeding will remove this, and restore the patient's strength. But the prostration of typhus cannot be removed in this way, for it exists generally without any perceptible local cause to account for it. We have, in the next place, agitation, anxiety, and loss of sleep—a remarkable and very unfavourable symptom; so unfavourable is it that, when present, though all the other symptoms may be trifling and free from danger, the chances are often against the patient; whereas, if he sleeps well, the case will very seldom go on badly, even though there may be other circumstances in it of a very threatening character. We next meet with the different varieties of delirium, furious excitement of the brain, or low and quiet muttering; then the symptoms connected with the muscular system—as floccitatio, subsultus tendinum, pains and spasms in various parts of the body, tremours, and paralysis of the sphincters. Now, with respect to the circulation, I told you that, in the commencement of the disease, we frequently observe an excitement of the heart and arterial system simulating the phenomena of active inflammation. In the advanced stage this disappears, and we have a weak and jerking action of the heart, with a feeble irregular pulse, which can be compressed by a slight touch of the finger. In some cases, where the pulse is naturally irregular, the effect of fever is to render it regular: this is a curious circumstance, and you should not neglect enquiring into it. Another remarkable symptom is, that you may have a very obvious disproportion between the action of the heart and the pulse; this is most commonly a sign of disease of some internal organ. With respect to the state of the blood itself, I do not intend to make any particular observations. We know almost nothing on this very interesting subject. All we can say is, that the blood is altered in its condition; but what the nature of that alteration is, we know not. Notwithstanding all that has been written on the subject, there has been as yet no valuable indication, no practical improvement, founded on the state of the blood in fever. There can be no doubt, however, that the condition of the blood (if known) would furnish a most important link in the chain of phenomena, and would be productive of a more certain and successful treatment. Still it is only a link, and the treatment of fever will still continue to partake of a more or less empirical character, as long as we remain ignorant of those profound lesions of innervation, with which it appears to be so intimately connected.

When we turn to the respiratory system, we find typhus presenting here also a very numerous and important group of symptoms. In some cases we have hurried breathing, with cough, and signs of organic lesion of the lung; in other instances the breathing is accelerated, but we are not able to detect any appreciable lesion. Here we have a purely nervous dyspnoea; in the former case we have it co-existing with disease of the lung, which can be detected by the stethoscope. Hurried respiration is an unfavourable symptom

in fever. It points out either an organic affection of the lung, or a sympathetic excitement. Hurried respiration, without pulmonary disease, is an anomalous symptom, and all such are to be looked on with suspicion. We have sometimes the symptoms of a general affection of the respiratory system—cough, dyspnoea, lividity of countenance, and bloody expectoration. There is nothing more common in typhus than catarrh, or congestion of the lungs, or pneumonia. A great many fever patients die of extensive but latent and neglected bronchitis. The patients may go on for days and weeks without presenting any thing capable of arresting the attention, or exciting the suspicions, of the mere symptomatologist; they will die with tracheal rattle, and, on dissection, the whole bronchial mucous membrane will be found in a state of intense inflammation.

When we come to the digestive system, we find numerous morbid alterations. The lips are dry and chapped; the gums red, dry, and covered with an adhesive mucus; the teeth crusted with a peculiar sordes; the tongue in some cases natural, in others appearing as if it had been dried and then glazed over; in some covered with a dirty white mucus towards the centre, and clean towards the edges and tip; sometimes its mucous papillæ are enlarged, red, and prominent; while, in other cases, it is of a dark brown or black colour, its upper surface chapped and inclined to bleed, and its muscular functions impaired, so that the patient is unable to put it out when desired. Then we have thirst, nausea, vomiting, and hiccup. In some cases the desire is for cold drinks, in others for hot, and occasionally there is no thirst; and the patient drinks only when he is requested, and then swallows merely what is sufficient to moisten the mouth and fauces. In some cases the fluid ejected from the stomach is of a bilious character; in others it is dark, or like coffee-grounds. The evacuations from the bowels present a very remarkable variety. They may be thin and liquid, or solid and figured; they may be clay-coloured, brown, or black; and they may be passed at considerable intervals of time, or they may be frequent and harassing. Generally speaking, they are liquid, of an unhealthy colour and consistence, and have a peculiar and very offensive odour. Obstinate constipation may be met with, but this is very rare, and in the great majority of typhus fevers there is rather a tendency to diarrhoea. You will have eructations, borborygmi, and tympanitis. You may also have jaundice, and this is generally to be considered as a bad symptom in fever.

In order to get an accurate idea of these symptoms, you must proceed analytically—you must pass in review the cerebral, respiratory, circulating, and digestive systems—and see how far the symptoms can be connected with actual disease of the three great cavities.

LECTURE XLI.

Nervous symptoms in typhus—Uncertainty of development—Opinions of Dr. Clutterbuck—Unfrequency of lesions of the brain in typhus—Occurrence of all nervous symptoms, independent of any appreciable symptoms of the brain—Nature and treatment of headache in fever—Delirium, researches of Louis on—Its treatment in early and advanced stages of fever—Pathological state of the brain in delirium—Use of wine and opium—Dr. Greaves's remarks on—Nature of adynamia—Principles of treatment of the local inflammations in fever—Errors of the school of Broussais on this point—Use of stimulants at certain stages.

At my last lecture I enumerated the symptoms of typhus, which are referable to lesions of the nervous system. The principle of these are the occurrence of headache, the different varieties of delirium, coma, spasms, subsultus tendinum, and various lesions of sensibility. Now, you are all, I believe, aware that the followers of Clutterbuck place the seat of fever in the brain, or, in other words, they look upon fever as being symptomatic of inflammation of the brain.

This doctrine has acquired a considerable degree of interest, from the circumstance of being supported and put forward by such high authority, and is therefore entitled to our notice. It must, however, be admitted, that it does not rest on a very secure basis, for it has been contradicted by the results of the recent investigations with respect to the pathology of fever. These investigations prove, that lesions of the brain are, in their nature, analogous to those of other parts, and can no more explain the phenomena of typhus than similar affections of the lungs, or digestive system. It has been shown, in the first place, that lesions of the brain (which are supposed by Clutterbuck to be the cause of fever) are by no means constant in their appearance; that there is the most remarkable variety in their seat and extent; and that the appearances, on dissection, do not correspond with the symptoms observed during life. If we were to consider fever as symptomatic of disease of the brain, we should always expect to meet with lesions of the cerebral substance under such circumstances, just as we find proportional lesions of the lung in the fever of pneumonia, or of the digestive system in that of gastro-enteritis. Even in cases where these lesions of the brain are found to exist, we do not find that they are in proportion to the symptoms, for we may have severe delirium and headache in typhus with slight cerebral lesion, and, on the other hand, we may have extensive organic disease of the brain with a fever, in which the nervous symptoms are but little marked. Here, then, we have a character absent which should necessarily exist, if typhus was symptomatic of disease of the brain. Thirdly, the symptoms, referable to mere irritation of the nervous system, independent of typhus, have not the initiative, for we frequently observe the symptoms of typhus to precede those which belong to irritation of the brain. A patient in typhus will go on for days without exhibiting any symptoms of cerebral irritation, and then, perhaps, towards the termination of his illness, will get symptoms

of disease of the brain. But they are not the first link in the chain of morbid phenomena. Again, in cases presenting a similarity, nay, almost an identity, of symptoms, *you will meet with the greatest possible variety in the nature, seat and extent of the organic alterations.* It has been farther shown that, with the same amount of nervous symptoms, with symptoms perfectly alike in their nature, intensity, and duration, we shall find one class of patients presenting remarkable lesions, and another class with none at all. Lastly, we meet with the same state of the brain in cases where no typhoid symptoms existed, as in those where all the characters of typhus were distinct and prominent.

Now, if we admit the foregoing facts, and I think they cannot be denied, we must reject the theory of Dr. Clutterbuck. The lesions of the brain are by no means constant in typhus; the symptoms observed during life do not correspond with the phenomena of dissection; we may have extensive organic changes with modified symptoms of typhus, and severe typhus with slight organic change; and we may have, in patients presenting similar symptoms, great organic changes or none at all. The conclusion which has been come to by the most eminent pathologists of the present day on this subject is, *that, in fevers of an essential type, there is no single nervous symptom which may not, and does not, occur independently of any appreciable lesion of the brain, nerves, and spinal cord; that the state of the nervous centres, after death, cannot explain the symptoms during life, and that the nervous symptoms, in particular, cannot be referred, in many instances, to any known organic alteration of the brain.*

This is a conclusion which may appear humiliating, and you may think that it depreciates medicine by representing it as obscure, perplexed, and deficient. This, however, is not the proper way of considering the matter; you are only to enquire whether it be just and well founded or not, and then to steer as well as you can. We cannot say any thing certain with respect to the nature of these and various other symptoms unconnected with any appreciable lesion of the nervous system; we merely observe them as facts, and find that they square with experience. Experience has shown, long since, that these symptoms frequently subside under treatment which could not be employed if they depended on inflammation. How could we explain the disappearance of delirium, under the use of opium and wine, or the removal of spasms and subsultus tendinum by stimulants, if they depended on active inflammation of the brain or its membranes? This is another fact which tallies exactly with these already mentioned, and showing that the nervous symptoms of typhus are frequently unconnected with inflammation of the brain.

One general observation may be made with respect to the nervous symptoms of fever, that the greater their number and intensity are, the more unfavourable is the prognosis to be. I may here mention, that the worst description of nervous symptoms are persistent sleeplessness, constant headache, violent delirium, subsultus tendinum,

and coma. All these point out profound lesions of innervation, and are met with in the worst cases of typhus. They do not point out the existence of extensive *organic* derangement of the brain, or that you should treat the case by means adapted to deplete the head, but that the fever is of a severe character, and that the shock which the system has received is likely to endanger life. Headache is one of the most constant symptoms of fever. It is sometimes partial, sometimes general; it may be more severe at one time than at another, or in one part of the head than the rest, and it may be dull, acute, or pulsative. It may or may not be accompanied by other symptoms, such as heat of the head, throbbing of the arteries, and the different varieties of delirium. In cases of fever where there is sympathetic headache, without any other sign of cerebral irritation, we are not at all warranted in looking on the case as one of disease of the brain. With other symptoms it may, and, consequently, under these circumstances should, command our attention, but alone it is to be looked upon as one of the general symptoms, or as symptomatic of some of those secondary affections which occur in the course of fever. When it is symptomatic, it is most generally connected with irritation of the digestive tube. There is nothing more common than headache in gastric fever, and you will constantly see this headache disappear after the application of leeches to the epigastrium. This I have witnessed, not in one, but in many instances.

Now, with respect to the treatment of the headache of fever, I may observe, that it must be more or less modified by circumstances. In mild and simple cases, where the pain of the head is not very severe, you may be content with cold applications, which you will commonly find to answer very well, particularly where the headache is accompanied by heat of the scalp, and throbbing of the arteries. You will also find the warm foot-bath of service, and you may, if necessary, increase its efficacy by the addition of mustard. Where the headache resists this treatment, you must have recourse to leeching the temples and behind the ears, shaving the head, and applying cold or iced lotions. Now it might be urged here, that if the best mode of relieving headache is found to consist in the application of leeches and cold lotions to the scalp, are we not justified in concluding that the symptoms, in these instances, are to be looked upon as depending on inflammation? I have stated to you, when lecturing on diseases of the brain, that headache was not to be considered as connected in all, or even many cases, with arachnitis; and besides, it does not follow that the symptoms must depend on inflammation, simply because they have been relieved by antiphlogistic treatment. What does the occurrence of pain in the head prove? Nothing more than this, that there is an excited condition, a deranged sensibility of the part affected. This derangement of sensibility may be accompanied or not by organic disease; but whether it is or not, experience has shown that it may be relieved by the use of local antiphlogistic means. I think you may lay this down as a rule, with respect to

the pain of the head in fever, that where you meet with headache or delirium, and find them connected with other symptoms, indicating a decided affection of the digestive system, you should seek for the cause of the headache in the sympathies which attend irritation of the gastro-intestinal surface, and direct your plan of treatment accordingly. If a patient, labouring under gastric symptoms, as red tongue, epigastric tenderness, swelled belly, and diarrhœa, complains of pain in the head, you may be pretty sure that the headache proceeds from sympathy, and that it will be most effectually relieved by leeching, and other means calculated to relieve gastric irritation. There are two reasons for this: of all the secondary irritations of fever those of the digestive system are the most common; and they are, in the majority of cases, productive of more or less functional derangement of the brain, with which the gastro-intestinal mucous membrane is linked in the closest sympathy. I have seen cases of fever where the patient complained of bad headache, and where the symptoms were combated unsuccessfully from day to day by the application of leeches to the head, and yet I have seen complete relief obtained by putting a few leeches to the epigastrium. If, therefore, you find, in cases like this, pain on pressure, tympanitis, diarrhœa, thirst, and other signs of irritation of the digestive tube, you will direct your attention to the belly, and see whether the headache may not be removed by means adapted to subdue gastric irritation. What I wish to impress on you is this, that secondary gastric disease will produce headache, and that this may be removed by applying leeches to the belly.

We now come to speak of a very important symptom in fever—delirium. I believe I may here assert that the same pathological observations apply to this symptom as to headache, and that, taken alone, it cannot by any means be looked on as a decided indication of actual organic or inflammatory disease of the brain. Before I enter on the subject of delirium, I wish to make a few remarks on the occurrence of hysterical symptoms in fever. In some persons, you will find that, at the commencement of fever, fits, like those of hysteria, will come on, and this may happen even in the case of robust men. One of the worst cases of fever I ever saw, was ushered in by these symptoms. The patient had violent fits of laughing, crying, and other symptoms of an hysterical character, and these were succeeded by profound typhus. Indeed, the complication of hysteria may be looked upon as favourable in all cases except fever; but the occurrence of hysterical symptoms, at an early period of fever, affords grounds for making a bad prognosis.

In fever, we meet with two kinds of delirium—the violent, and the low muttering delirium. The high and violent delirium is a disagreeable symptom, from the unmanageable state of the patient, and the difficulty of doing any thing for him. It is most commonly observed in the commencement of the disease, whereas the low muttering kind is generally observed towards the termination. This is the most usual way, but sometimes the reverse takes place; and when this occurs, it is to be looked on as dangerous, on this

principle, that any departure from the ordinary course of things is unfavourable, and gives cause for a bad prognosis in fever. Thus the occurrence of low muttering delirium in the commencement, or of the violent kind towards the termination, of the disease, is always alarming. Low delirium is most frequently observed towards the close of fever, and we find it always accompanied by other symptoms, indicating a low and prostrated condition of the system, as subsultus tendinum, picking of the bed-clothes, involuntary stools and urine, hiccup, and cold extremities.

Let us consider how far we are to consider this symptom as connected with actual disease of the brain. Louis, who has devoted a considerable share of his attention to this point, has examined carefully the state of the brain in twelve fever patients who had little or no delirium, and in twelve others, who died with violent delirium, and the following are the results of his experience. In the first table he places those who had no delirium; and here we find, that in four cases there was some redness of the cortical substance of the brain; in one, inflammation of the optic thalamus; in one, slight softening of the brain; and in the remaining six, the cerebral substance was quite healthy and normal. Now, in the cases where violent delirium was present, the following circumstances were observed. In five cases the morbid appearances were slight, there being some redness and vascularity of the cortical substance, but no more; in one, there was slight ramollissement; in one, livid injection of the brain and its membranes; in the remaining five, the substance of the brain was of a natural colour and consistence, and appeared to be quite healthy. Now, in these two tables you will observe that the difference in the morbid appearances is extremely slight, and that the presence or absence of delirium seems to have very little connection with the perceptible state of the brain. In those cases where there was no delirium, we find four patients presenting redness and vascularity of the cortical substance; in those which were attended with violent delirium, we find five patients similarly circumstanced. In the cases without delirium, there was one with inflammation of the optic thalamus, and another with ramollissement; in those where violent delirium prevailed, there was one with ramollissement, and another with dark venous congestion of the brain and its membranes. In the first table, where delirium was absent, we find six cases in which the brain was, to all appearance, healthy and normal; in the second table we find five cases similarly circumstanced. Recollect, too, that these observations were made by one of the most accurate pathologists in the world; a man who had no theory to support, who merely records what he has seen, and whose statements are therefore entitled to the most complete credence. With respect to induration of the brain, M. Louis remarks that he has met with it in two instances of fever; in one, the patient was delirious; in the other, the mental powers were unaffected. In the *Clinique Médicale* of M. Andral, we find that he has observed induration of the brain to be connected with stupor and coma. Thus we see that in

one case this state of the brain is found in connection with delirium, in another with stupor, and in a third with no appreciable lesion of the intellectual faculties. The conclusion, then, which we must come to is, that inflammation of the brain does not necessarily imply delirium, nor does delirium imply any certain disease of the brain.

In the foregoing tables, we have a healthy state of the brain with delirium, and a morbid state without it. Observe the practical deduction to be drawn from this. You might suppose that you could draw no conclusion from this, which would not have a tendency to embarrass your practice, and depreciate the value of medical science. This, however, is not the fact. The conclusion is an obvious and a just one, that, in the treatment of a case of delirium, *we are not to expect that we shall be always able to remove this symptom by treatment calculated to relieve inflammation*, because it may occur with or without disease of the brain. Whenever you meet with a case of delirium, you should carefully investigate its nature and origin, as far as you possibly can, and if it be probable that lesion of the brain is going on, you should direct your treatment to the head; but if it be merely sympathetic, you must look for its source elsewhere, and regulate your treatment accordingly. You cannot always relieve delirium by antiphlogistic means, directed to the head. This is an undoubted fact, and squares with what has been stated with respect to headache, for we find that delirium is frequently removed by measures which would be quite inadmissible if it depended on actual inflammation of the brain. In all cases of febrile delirium, where it is accompanied by pain of the head, heat of the scalp, throbbing of the arteries, and other signs of determination of blood to the brain, you should not neglect the employment of local depleting means. For this purpose, you may use with great advantage the cold affusion, as recommended and practised by Dr. Abercrombie. I have seen patients who were for nights without sleep, and in whom the delirium was so uncontrollable that the strait waistcoat was necessary, and I have observed that those persons slept soundly and tranquilly after the cold affusion. In cases of high delirium with strong reaction, the use of blisters has been recommended. This practice appears to me to be questionable and hazardous, and I feel convinced that they very often do mischief. A much better practice is that of applying sinapisms to the feet. The strait waistcoat is a measure very frequently adopted in the treatment of delirium, but it is one which you should be cautious in employing. It is too generally resorted to from apprehension on the part of the attendants, and is continued through mismanagement. In Dr. Cheyne's valuable report on the epidemic fever of Ireland, he remarks, that some patients have perished from exhaustion in struggling with their attendants, or in endeavouring to free themselves from the restraint of a strait waistcoat. If a patient gets out of bed and walks about the room, there is not the slightest risk of his receiving any injury, provided he be properly covered. When he endeavours to get out of bed, the

nurse should calm him if possible, and, if he perseveres, use her authority over him, but never resort to violence. She should even permit him to put on his clothes, sit on the side of the bed, or walk about the room, and wait until the violence of the delirium abates. This generally occurs in a short time, and the poor patient becomes quite amenable. Such are Dr. Cheyne's views. I believe, firmly, that the strait waistcoat is too often abused. Coercion should be used only in extreme cases, and where there is danger of life; and it should be dispensed with on the first opportunity.

In the advanced stage of fever, even where delirium is present, I believe we may often give wine with advantage. I have seen the tongue, which was dry and black, or red, become pale and natural under the use of wine, opium, and other stimulants. This tallies exactly with what I mentioned *as to the tongue being rather an index of the state of the whole system, than of any particular condition of the intestinal canal.* With respect to the use of wine, Dr. Graves remarks, that it is a common opinion that wine should not be allowed while the patient's eyes are red and suffused. He is of a different opinion, and thinks it may be often given with safety and advantage even in this state. "It should be borne in mind," says Dr. Graves (and this is important), "that want of sleep will make the eyes red and suffused; and this being very common in fever, it cannot be brought forward as an argument against the use of wine. Neither does hot skin contra-indicate the use of wine, particularly when there is at the same time a tendency to coldness of the extremities." Now, to come to the subject of delirium, so far as this is connected with giving wine and stimulants, or withholding them, Dr. Graves thinks that when the delirium is violent, wine should not be given. I would modify this assertion, and say, that it should not be given in the delirium which comes out at an early period of fever; but in the advanced stage I have seen it do good. The patient may be irritable, restless, and delirious, and yet in a state which demands the use of wine and opium. You will find patients in the advanced stage of fever talking incoherently, and incapable of giving direct answers to your questions. This state of mind is frequently accompanied by an anxiety about their illness, and want of sleep. Now, to procure sleep, and remove the delirium, you will have recourse to the use of wine and narcotics. "Headache," says Dr. Graves, "is, at any period of fever, a bad circumstance; for it does not allow the patient any sleep. Now, we must, in the first place, endeavour to remove this symptom by depleting the head, and by purgatives. These measures frequently succeed, but sometimes they fail, and we cannot pursue this line of treatment any farther. (This is the case to which I have already alluded.) In this case," says he, "we must prescribe an opiate to procure sleep, and from this sleep the patient often awakes much improved. Before you employ the opiate, you may apply a blister to the head." Much benefit may be derived from the use of the common starch injection with tincture of opium. Thirty or forty drops of laudanum, in two or three ounces of the mucilage

of starch, is a very good way of employing opium to procure sleep in fever. The general rule with respect to the use of opium is this: *Where you have nervous symptoms remaining after treatment calculated to remove inflammation, or where they have occurred unaccompanied by any signs of inflammation of the brain*—in these cases I feel no doubt that opium may be employed with safety and success; in fact, that it is the best means you can adopt. Observe, all that I now state is founded on facts, and independent of any theory. You will observe, too, that it squares with the pathology of fever given in a former lecture—namely, that we are to look upon it as a general affection of the whole system; and that the presence of any one single symptom does not necessarily imply the existence of local inflammatory disease. There is one practical remark with respect to local inflammation which I have already made, but I shall here beg leave to repeat, as it is connected with the point in question. When we compare the three great cavities with respect to the comparative frequency of inflammatory disease observed in them, we find that the account stands as follows:—local inflammatory disease is most frequent in the belly, next in the chest, and lastly in the head. We much more frequently meet with organic affections of the digestive system in fever, than of the pulmonary, and of the pulmonary than of the nervous.

In the advanced stage of fever, we have often to combat with a very distressing symptom—namely, coma. The patient lies in a state of lethargic stupor, and cannot be roused, or if he exhibits any signs of consciousness, it is only for a moment, and then he relapses again into the same stupid state. This is a bad symptom, but not a fatal one, if it be properly managed. What seems to answer best is, keeping the head cool, and using counter-irritation. These two measures may appear inconsistent with each other, but they are not really so. Some of the worst cases of febrile coma, which I have seen recover, were those in which blisters were applied to the back of the head, or vertex, or nape of the neck, the rest of the head being kept cool. In addition to these measures, you may employ counter-irritation to the extremities with advantage. You will see, in the works of Lallemand and Mackintosh, some very remarkable cases of coma, where the patient was, as it were, snatched from the jaws of death by pouring boiling water on the extremities, at the same time that the head was kept cool. In the advanced stage of fever, where the skin and extremities are cool, the existence of coma does not contra-indicate the use of wine. You may also employ musk in doses of ten or fifteen grains every third hour, either alone, or in combination with camphor, or carbonate of ammonia. These, with blistering the head and keeping up counter-irritation in the lower extremities, are the principal curative means used in the treatment of coma. I shall conclude this lecture with some observations on adynamic fever. Whenever you meet with this remarkable prostration of strength, it will be necessary for you to enquire carefully into its nature. You must here bear in mind two propositions which should never be forgotten—first,

that the adynamia may be secondary, and produced by visceral lesions, for the removal of which antiphlogistics may be necessary; and, secondly, that it may exist as a consequence of the morbid state of the whole system, and independent of any decided local inflammation. The first of these is called false, the second true, adynamia. One of the most important questions in fever is connected with this point, and I beg your particular attention to it. It was long taught, and is to a certain extent still believed, that fever is a disease of debility; and hence all the phenomena indicative of this debility were treated by stimulants; and it is melancholy to reflect that, even at the present day, hundreds of persons are sacrificed at the shrine of this dogmatism. Now, when you recollect that fever most commonly presents a group of local inflammatory affections, and that the excess, or extent, or seat of a local inflammation will produce debility, you will very readily perceive that there are two causes for the adynamia of fever—one the reaction of local disease on the constitution, the other depending on a peculiar morbid state of the whole system; and it is in striking a balance between these two causes that the judgment of the physician consists, and it is on the accuracy of his judgment that his success will decidedly hinge. In all cases where the prostration is unconnected with inflammation, or, even if inflammation should exist, where antiphlogistics can be no longer employed, you must have recourse to the cautious but decided use of stimulants. In some cases, particularly where the prostration is not excessive, you may, at the same time that you are prescribing stimulants, employ local antiphlogistics and counter-irritants with great advantage. When I say this, I do not wish you to believe that in treating the inflammatory affections which arise during the course of fever, you are to proceed with the same boldness as in original simple inflammation. I have before marked the distinction between them. In the secondary local inflammations of fever, where profound lesion of innervation and a morbid state of the fluids exist, you should always modify the activity of your antiphlogistic measures; but, on the other hand, where the local inflammations are simple, and the constitution strong, your measures must be prompt and energetic. There is one great rule which is applicable to most fevers:—In the early stages, antiphlogistics are required; in the advanced, stimulants and tonics. This rule applies not only to fevers, but also to most cases of local diseases. In the early stage of fever, you may always employ the antiphlogistic treatment so far as regimen, local depletion, and the prohibition of all stimulants are concerned; in the latter periods, where the patient's strength is reduced from depletion, extent, or duration of disease, and want of nutriment, your object must be to support the strength, and even to give stimulants. This is the great point which has been overlooked by the school of Broussais. They looked upon fever as depending on local inflammation, and concluded that stimulants were inadmissible *at any stage of the disease*; and their practice was accordingly to continue the use of antiphlogistics throughout the whole case.

By thus neglecting to support the system, they permitted exhausted nature to sink, ignorant of the fact, that while you are taking measures to remove local inflammation, you may at the same time give nutriment to support strength, *and that at certain periods of all inflammatory affections, stimulants become antiphlogistics.* The same thing occurs in local inflammation of a simple and original character, as in fever. In healthy inflammation we cannot pursue the antiphlogistic plan all through; after a certain period it loses its effects on the local disease, and it will run the patient down if persevered in; a cure is to be obtained only by the use of stimulants and tonics. So it is with respect to fever, in the advanced stage of which stimulants become antiphlogistics. The great rule, then, with respect to the treatment of adynamia is this:—First ascertain if there is any existing local inflammation, and if it be of sufficient extent and intensity to produce prostration, adopt means to modify or remove it, as far as your patient's strength will admit; in the next place, if you find no local inflammation to account for it, or if it occurs at an *advanced stage*, when the powers of life are sinking, give wine and stimulants to support nature in the struggle which she is making with disease. In the treatment of the secondary local inflammations which arise during the course of the fever, we should always endeavour to moderate the symptoms as far as we can, and gain time. If we can succeed in prolonging life, we give our patient every chance for recovery, and allow the powers of nature time and opportunity to act. Hence it is that one of the most important objects of the physician, in the treatment of the advanced stage of fever, is to prevent his patient from becoming too much debilitated. Even though local inflammations should be present, yet, if the patient be sinking, and the fever of considerable duration, he knows that his only resource lies in judicious stimulation, which I fully believe not only acts beneficially on the general fever, but also on the local disease, whatever it may be. In all probability, the original affections have changed their character, and the irritated capillaries have passed from a sthenic to an asthenic state; in other words, to a state where antiphlogosis will be, to them, insufficient, and, to the disease, dangerous, but where stimulation is their cure, and also what nature demands. The great point is to know the exact time when we should change our practice. This period occurs in all fevers, but the time of its supervention is infinitely various; and it is in the knowledge of this, as well as in striking a proper balance between the dangers to be apprehended from local inflammation on the one hand, and constitutional debility on the other, that the great nicety of treatment consists. It would be impossible to teach this in a lecture; the only way in which it can be learned is by exact and long-continued clinical observation.

LECTURE XLII.

Opium in fever—Dr. Latham's opinion on—Symptoms for the exhibition of opium—Affection of the sensorium in fever—Adynamia, consequence of fever—Direct adynamia—Indirect adynamia—Treatment of—Stimulants in fever—Dr. Grant's notions of fever—Symptoms of typhus—Catarrh of fever—Opinions of Andral, Louis, and Laennec—Bronchitis with fever—Increase of râles on decrease of disease—Affection of the gastro-intestinal mucous surface—Symptoms of pneumonia and bronchitis—Pneumonia of fever—Symptomatic affections of the respiratory system in fever—Sympathy between the left lung and stomach—Phthisis, consequent of fever.

I drew your attention, at our last meeting, to the employment of opium in the treatment of some of the nervous symptoms of fever. Before I leave this subject, I wish to lay before you a brief abstract of Dr. Latham's opinions on this point. I believe I may say, that one of the first persons who proposed and administered opium in fever was the celebrated Sydenham. His opinion of it was, that, after purgation, it is generally productive of good effects. At the present day, there is considerable difference of opinion with respect to giving opium in fever, but there can be no doubt that it may be given with propriety in many cases, and that it has a powerful effect in combating some of the worst symptoms of typhus. Dr. Latham observes—"In some cases of fever the disorder of the sensorium keeps pace with that of other parts, and thus the disease proceeds until it is somewhat advanced, when the harmony between its symptoms is disturbed; those of the brain outrun the rest; new indications of treatment arise, and if the patient can be saved at all it must be by opium. Thus the heart and arteries may be full of activity, and every symptom in proportion to it. Remedies are accordingly addressed to the vascular system, and succeed for every purpose they are intended to fulfil, *except one*. By venesection, or topical bleeding, the general symptoms are relieved, and each organ loses its peculiar distress, but still *the delirium continues*. With an improved state of the circulation and of other organs, this condition of the sensorium remains, and to this subsultus tendinum is liable to be added, and the unrestrained passage of the different evacuations."

"In such a case as this," says Dr. Latham, "I have seen a single dose of opium, dexterously administered, change the whole complexion of the disorder in a single night, and place the patient at once in a state of safety. Again, I have seen the sensorial affections incident to fever, which require opium for their cure, manifest themselves in another form. There has been high vascular action from the first, and large depletion has been required to subdue it, and guard particular organs, and especially the brain, from injury. Under such treatment all has gone on successfully, and the patient has reached the point of convalescence, when suddenly (the tongue, pulse, and all other circumstances continuing favourable) some strangeness of manner has arisen, and then the wildest delirium, and then the unrestrained passage of the evacuations. I have known a transition from such a state of convalescence to such a

state of peril take place in a few hours; and I have known the patient again brought back to a state of convalescence in twenty-four hours, by a moderate dose of opium."

Three circumstances call for the employment of opium in fever: first, where there is persistent watchfulness; secondly, where an actual inflammatory condition of the brain existed, and has been subdued by proper antiphlogistic treatment, but delirium and other nervous symptoms still remain; and, lastly, where an excited state of innervation of the brain exists, without heat of scalp or remarkable throbbing of the arteries of the head.

There is another remark of Dr. Latham, which appears to me to be so important that I shall offer no apology for laying it before you in full. After observing that the affections of the brain in fever are capable of much illustration, from a knowledge of the habits and state of health of individuals before they become the subject of fever, he goes on to say—"And here I would make one general remark, that, by knowing what a man is, and how he lives habitually, the physician often arrives at a much better judgment, and a better treatment of his diseases. It is trying a man's diseases by his health, and a most valuable test it is.

"In healthy and vigorous bodies, there is a certain balance and regularity of function which, even when disease befalls them, is seldom lost; but their morbid action is still harmonious and proportional. In them, diseases are often severe, but they are generally simple; they often require the most active remedies, but they are generally easy of cure. On the other hand, the weak and valetudinary—who, at the best, are full of jars and incongruities—are obnoxious to the strangest forms of disease, hard to understand, and hard to treat. Now, fever, when it happens to a perfectly vigorous and healthy man, is never characterised by any such peculiar affections of the sensorium as have been mentioned. These are incident, according to my observations, to those only whose habits and mode of living have been calculated to do an abiding injury to the nervous system, and who have been long actually suffering from such an injury. Every class of society has furnished me with instances of this form of fever, and every instance has confirmed the truth of the remark.

"Among the higher and educated classes, there is in this age and country a wonderful striving for all the objects of wealth, and honour, and power. We need only think upon the strife of politics, the hazards of mercantile gambling, and the wear and tear of hard professional toil, to see how many there must be who, from the common business of life, have derived, both to their minds and bodies, new feelings and impulses, and new susceptibilities of disease. These susceptibilities chiefly belong to the brain and nervous system, and they are apt to come forth in frightful activity when such men become the subjects of fever. The trouble of the brain gets the mastery (as it were) of the disorder of every other part.

"The poor and mean, among mankind have the mind overwrought, and the nervous system exhausted, by real calamities,

just as the high and educated by their more refined; and thus they often claim an unenviable approximation to them in the character of their diseases.

"To these moral causes," says Dr. Latham, "I will add one physical cause which is of most extensive influence—the habitual indulgence in spirituous liquors. Individuals who have done a permanent harm to the nervous system by the abuse of spirits, do never, when they become the subjects of fever, suffer delirium of the ordinary kind, in which the brain is excited nearly in the same proportion as the blood-vessels, and which, by remedies addressed to the blood-vessels, is uniformly controlled; but they suffer a delirium in which the brain is actuated disproportionately to, and (perhaps) independently of, the blood-vessels; and, if curable, to be cured by opium. This I venture to state almost absolutely and without exception."

I can only state here, that the foregoing eloquent and philosophical observations are in perfect accordance with what I have observed of fever in Dublin. In all cases where the patient has been exposed to the depressing effects which high intellectual or moral excitement, or the abuse of spirituous liquors, produces on the nervous system, you will have, during the course of fever, more or less disorder of the sensorium. Here, but particularly if the delirium exists independent of any affection of the blood-vessels, opium is our sheet-anchor. With respect to the quantity to be administered, there are some cases in which it will be necessary to feel your way, and indeed I believe that in all cases this is the best and safest practice. In place of giving a large dose, as in delirium tremens, you commence with a moderate quantity, and if you find that this agrees with the patient you can increase it; at all events, by pursuing this plan you will be certain of doing no harm. It frequently happens, too, that small doses act very well in this affection; you should, therefore, begin with a moderate dose, watch its effects on the patient, and then repeat it in increased or diminished quantity, according to circumstances.

Towards the conclusion of my last lecture, I drew your attention generally to the subject of adynamia in fever, and endeavoured to show that in most fevers you have to contend with indirect as well as direct adynamia. Direct adynamia is that prostration of the system which results from the original disease; indirect adynamia is that which is produced by the reaction of the secondary local affections. I showed you that in many fevers we had, in addition to the debility consequent on an affection of the whole system, another source of adynamia depending on the local diseases which arise during the progress of fever, and that consequently the treatment of adynamia must vary according to the circumstances under which it occurs. I said that the indirect adynamia must be combated by local antiphlogistic means directed to the inflamed organs, but that, in the employment of these means, we should not be so vigorous as in cases of idiopathic inflammation, and that the general state of the patient should be always kept in view. You will

readily assent to this proposition, when you consider that, in idiopathic disease, you have inflammation occurring in a constitution previously healthy; but in the case of local disease supervening on fever, in a constitution previously debilitated by general morbid derangement. It occurs, in fact, in that state of body in which there is a profound affection of the solids and fluids, of circulation, of digestion, and of the nervous system. You can, therefore, easily understand why the antiphlogistic plan must be employed with more caution, and followed up with less vigour, in the treatment of those secondary affections which arise during the progress of fever.

I mentioned, too, that in most cases of advanced fever, and sometimes even at an early period of the disease, the employment of stimulants may be necessary, *even where the fever is accompanied by local disease*. A period occurs in all fevers (generally at the advanced stage) when general stimulation will be demanded, even though you may at the same time be able to detect local irritation. In such cases as these we might say that direct adynamia was predominant, and that, as danger was to be chiefly apprehended from this cause, we must meet it with stimulants. You will recollect I mentioned, at my last lecture, that a state of the general system and of the local disease occurred at an advanced period, to which antiphlogistics were no longer applicable, and in which, by giving stimulants, we assisted the efforts of nature, and gained time for the treatment of the local affection; in a word, that the formula of treatment for most cases of fever was the same as in cases of idiopathic inflammation. In the early period we employ antiphlogistic means, both general and local; in the advanced stage we use stimulants and tonics. In all cases of simple inflammatory disease, a time arrives when you must change your hand, and have recourse to an opposite line of practice; and the same thing occurs with respect to fever, whether we view it as an original affection of the whole system, or as symptomatic of some local irritation. I showed you, too, that you might in many instances combine local antiphlogistics with a general tonic treatment—the latter to be regulated so as to support the patient's strength without increasing the local disease. It will often happen, that, in the advanced stage of fever, you will meet with enteritis, or pneumonia, or some other local inflammation; here, while you are engaged in reducing the local inflammatory affection, you may also support the patient's strength with light nutritious diet, and even a little wine. In Dr. Grant's work on fever there is an important remark bearing on this point, which I shall beg leave to quote. "I know," says he, "two physicians, both in considerable practice, and both honest men, who have constantly treated fever badly from different causes. The one never gave cordials, wine, or nutritious food, during the whole disease; the other, after the fourth day, never gave any thing else. A combination of both these modes, employing each according to the circumstances of the case, would be very excellent practice. Their error, however, was not entirely the same, nor did the same

effects result from these opposite modes of treatment. The patients treated by the physician who persevered in the antiphlogistic system seldom died, but their recovery was tedious and protracted; those who were treated by stimulants throughout the entire disease seldom recovered." Observe how well this agrees with the doctrines I have laid down for the management of fever. The first practitioner generally cured his patients, but their convalescence was slow and difficult, because he used antiphlogistics too boldly and too long. He was ignorant of the fact, that, after a certain period and at a particular stage of the disease, antiphlogistics lose their efficacy; and his patients may therefore be said to have recovered by the unaided powers of nature. He was ignorant also of the fact, that, at a certain period of fever, certain conditions of innervation exist, in which what are called stimulants have a powerful effect in removing the disease, even though characterised by symptoms which have been termed inflammatory; while he who used nothing but stimulants, being ignorant of the number and consequence of the local inflammations, lost his patients by a too early and indiscriminate stimulation.

I shall now proceed with the analysis of the symptoms of typhus, and the proper mode of treating them. Having discussed those which belong to the nervous, let us now take up the consideration of those which characterise affections of the respiratory system. Before, however, we leave the subject of nervous symptoms, I may mention that some researches have been lately made on the state of the spinal cord and ganglionic system in fever; but there has been no result from these investigations calculated to throw any *new light* on the nature and treatment of the disease. Indeed, it is a remarkable fact, that in many instances of fever, remarkable for the extent and persistence of nervous symptoms, we cannot detect any lesion of the brain or spinal cord. In certain cases it has been stated that the sympathetic nerves were found diseased; but this occurs so seldom, and with so little constancy or correspondence of the existing symptoms with the derangement of the nerves, that it is impossible to establish any connection between lesions of the ganglionic system and typhus.

I believe I may say that a considerable number of cases of typhus are either unaccompanied by thoracic symptoms, or, if they do exist, they are in general slight and easily manageable. In some instances, however, the pulmonary system is severely attacked; and, in many cases of this description, death is caused by asphyxia from exorbitant secretion, brought on by extensive bronchitis occurring during the course of fever. There is one general observation, which applies not merely to the derangement of the respiratory system, but also to the morbid changes which may take place in any viscus of the body during the progress of typhus; and this is, that the functional derangement observed in each particular organ during life is seldom in proportion to the changes discovered after death. With symptoms of an apparently trifling character, and by no means calculated to alarm, you may, in the case of the lung,

find after death marks of universal bronchitis, of enormous congestion, or extensive pneumonia. The same character of insidious latency applies to all the inflammatory affections which come on during the progress of fever. If we seek for an explanation of this fact, we shall perhaps find it in considering that, during the existence of fever, the nervous system appears to be labouring under some profound injury; and we know that where innervation is injured sensibility is impaired, and hence the symptoms expressive of suffering or disorganisation are more or less latent. Hippocrates observes, in one of his aphorisms, that in cases of severe disease, where no pain is complained of, there is disease of the brain; thus accounting for the absence of suffering by referring it to some lesion of the sensorium.

In speaking of the catarrh of fever, I beg of you to bear in mind that this, like all the other inflammatory affections supervening on fever, is to be looked upon as a secondary inflammation. When I use the term catarrhal or gastro-catarrhal fever, I do not mean to express fever symptomatic of catarrh, or of catarrh combined with gastritis, but that the catarrh, whether simple or combined with gastro-enteric disease, is one of the results of fever. There can be no doubt that, in the majority of instances, this is the case. All the arguments which I have adduced, to prove that affections of the brain were not the cause of fever, apply also to disease of the lungs; and we shall find the same thing to hold good when we come to the digestive system. The experience of Andral, Louis, Laennec, and various British authors who have written on fever, perfectly coincides on this point. You will see, in Laennec's work on auscultation, that he has devoted a good deal of his attention to those secondary affections of the lungs which occur during the progress of fever.

Now, in many cases, this bronchitic derangement may exist during the whole course of the disease without any degree of violence, and the patient will recover without having any attention paid to it; in fact, many will go through fever with lesion of the bronchial mucous membrane so slight as to have no claim to notice. The occurrence of an affection of so trifling a character as this is not sufficient grounds for altering or modifying your prognosis; still it should always put you on your guard, for it may, in the course of six or seven days, become greatly exacerbated, assume a very intense character, and even carry off the patient. Whenever, then, you discover any increase in the bronchitic symptoms, direct your attention immediately to the state of the lung, and be on your guard lest the disease should get ahead of you. What we generally observe in these cases is, that the patient has some cough, but this is neither hard nor troublesome; you can hear now and then a few sonorous *râles* over the chest, but it sounds quite clear on percussion. Such a case as this, as long as it remains unchanged, is of very little consequence. In some instances, however, the affection of the bronchial membrane becomes a most formidable symptom: the patient has lividity of face, cough, hurried breathing and expect-

toration, and all his symptoms point out the existence of a very extensive disease of the lungs. In other cases, the symptoms of bronchitis may be almost entirely absent; and yet, if you make an examination with the stethoscope, you will detect the existence of a latent but general and intense bronchitis. Here, persons unacquainted with the stethoscope, and depending on symptoms only, may be led into great and fatal errors. Nay, even persons acquainted with the use of this instrument may, by neglecting to make a proper examination, fall into great mistakes. Thus, if you apply the stethoscope to the patient's chest while he is lying on his back and breathing in the ordinary way, all you can ascertain is that there is feebleness of respiration, with here and there a sonorous *râle*. If you were to content yourself with such an examination, and decide as to the state of the bronchial mucous membrane from the signs afforded by ordinary respiration, you would be wrong; for, though the *râles* may be few and scattered during ordinary breathing, you will find that when you raise the patient up, and make him take a full inspiration, so as to cause the air to enter the minute bronchial tubes and air vesicles, the *râles* become intense and are audible over a large portion of the lung. You will find, also, that the disease generally affects the middle, inferior, and posterior parts of the lung, and that it produces here symptoms approximating to those of pneumonia. Another curious circumstance connected with cases of this description is, that, according as the patient gets better, the *râles* become louder and more distinct; and here we have the apparent anomaly of an increase of the morbid sound pointing out a decrease of the disease. This is explained by recollecting that, at the height of the bronchial affection, the minute bronchial tubes and air vesicles are chiefly engaged; and, as from the engorgement of the lining membrane, the quantity of mucus which fills them, and the feeble state of respiration, the air either does not enter into them, or, if it does, only imperfectly, the consequence is that the characteristic *râle*, or morbid sound, is scarcely heard. But when the disease is on the decline, and the minute tubes become free, the bronchial affection being then chiefly confined to the larger tubes, we can hear the sonorous *râle* distinctly during ordinary breathing. Many cases of this kind terminate fatally. The patient continues in a low adynamic state, with hurried breathing and lividity of face, for two or three days; then comes a rattle in the throat, and in the course of ten or twelve hours he dies. On dissection, the marks of general bronchitis are evident, and the bronchial tubes are found filled with a sanguinolent mucus. In some of these cases, super-secretion from the bronchial mucous membrane sets in with extraordinary rapidity, and carries the patient off at once.

This form of disease is ordinarily co-existent with derangement of the gastro-intestinal mucous surface, and this forms one of the most common and fatal varieties of fever in this country. In some cases the disease predominates in the respiratory, in others in the digestive, system; and in some instances you will see a very

curious alternation of this predominance of morbid action between the thoracic and abdominal cavities. To-day the breathing is hurried, the cough troublesome, the stethoscopic signs well marked; in two or three days after, the cough disappears and the breathing is relieved, but the belly is more swollen and tender, and the tongue has become redder and more parched. Then the abdominal symptoms will improve, and the indications of pulmonary irritation become again more manifest; and thus, even during the course of the same fever, we may have several alternations of this kind. This complication of disease of the thoracic and abdominal cavities is extremely common in fever; in fact, it may be almost laid down as a general rule, that, in the affections of the respiratory system in fever, there is a complication with disease of the digestive tube. Now, you should always bear this in mind, and, when you prescribe medicines to relieve the pulmonary affection, you should take care that they shall not produce irritation of the digestive system.

In most cases of severe bronchial inflammation, you will observe more or less lividity of countenance. This is not seen in cases of simple pneumonia occurring in fever. Bronchitis has a much more powerful effect in producing this phenomenon than pneumonia, and hence it is that slight bronchitis will cause more lividity of countenance than extensive pneumonia. This fact seems to be in accordance with the opinion, that the portion of the lung in which the process of aëration is carried on is the bronchial mucous membrane, and not the air vesicles. In inflammation of the air-cells of the lung we have little or no lividity, but we never see a case of extensive bronchitis without it. I may mention here, also, that I have seen several cases of bronchitis with very remarkable lividity, in which the temperature of the body was above the natural standard, thus giving a direct proof that animal temperature depends on something else besides the aëration of the blood.

We come now to speak of the pneumonia of fever. This affection may occur under two forms; that is to say, its symptoms may be manifest and prominent, or they may be latent and inappreciable, except through the medium of auscultation. Of these forms the latter is the most common. You may in typhus have all the symptoms present which characterise idiopathic pneumonia—you may have hurried breathing, cough, pain in the chest, bloody expectoration, &c.—but I believe that, in the majority of cases, the pneumonia of fever is more or less latent. It often happens, that, at an advanced period of fever, without any increase of dyspnoea or cough, the patient gets worse; and this change, you may be sure, is indicative of some great visceral lesion. The pulse gets smaller and more feeble, and, on making an examination, we generally find the cause to be an extensive but latent pneumonia. Without manifesting its existence by any of the usual symptoms, the disease comes on insidiously and rapidly; and, on examining the chest with the stethoscope, we hear the crepitating *râle* over a large portion of the lung, and the chest sounds dull on percussion to a very

considerable extent. Observe, here, the importance of a thorough knowledge of auscultation. I think I may safely venture to assert that a person, ignorant of percussion and the stethoscope, is totally incompetent to undertake the treatment of any case of fever. I know it has been asserted, by some authors, that these affections of the respiratory system are more of a congestive than an inflammatory nature. To this opinion I cannot subscribe. We find in such cases, on dissection, all the pathological appearances of active inflammation, redness, thickening, and ulceration of the mucous membrane, sanguinolent secretion, congestion, hepatisation, purulent infiltration, and even abscess of the lung.

It is a very remarkable circumstance, that the pleura seems, of all the pulmonary tissues, to be the least liable to morbid alterations in fever. From the details of a great number of modern dissections, it appears that, in many cases of fever where extensive bronchitis and pneumonia were found to exist, the pleura has in almost every instance escaped. If this be true, it would seem to point out some difference between ordinary idiopathic pneumonia and the pneumonia of fever. In the former, we have generally more or less pleuritic inflammation; in the latter, it is rarely met with. There is, in most cases of bad fever, an effusion of bloody serum into the cavity of the pleura, and the same thing has been observed in the case of animals who have been poisoned by the introduction of putrid substances into the veins. This leads us to the knowledge of a fact connected with the history of typhus, that the tissues which are most liable to secondary inflammation are the mucous membranes and parenchymatous organs. On the other hand, the serous membranes seem to enjoy a comparative exemption from active disease. We meet with arachnitis now and then, but very seldom with pleuritis, pericarditis, and still more rarely with peritonitis. Though you may have extensive pneumonia in the chest, or enteritis in the belly, you will have no trace of the existence of inflammation of the peritoneum or pleura; and there can be no doubt that serous membranes are least liable to inflammation in fever. We might attempt to explain this, by considering that serous membranes possess but very little sensibility, and consequently are less susceptible of irritation. In the healthy state they seem to be insensible, and we cannot trace nerves in their substance or on their surfaces. This insensibility appears to be one cause of their exemption; another is, that they are supplied with white blood, a portion of the circulation which seems to be least altered in fever—that which is most affected being the fibrine and colouring matter.

Let us now discuss the symptomatic affections of the respiratory system in fever. You may have dyspnoea, hurried breathing, and cough, and yet, on examination, you cannot find any distinct signs of bronchitis or pneumonia. You will often meet with symptoms of pulmonary derangement in fever, and yet, when you come to apply the stethoscope, you find either no sign of disease, or, if there be any, quite insufficient to account for the symptoms. Under these

circumstances, the affection of the chest must be looked upon as sympathetic, and the disease which is its most common cause is gastro-enteric inflammation. Here, again, we have another remarkable instance of the value of the stethoscope. Symptoms indicating marked pulmonary derangement exist; and, if we had not this means of regulating our judgment, we should most probably conclude that there was actual disease of the lungs, and would proceed to treat a sympathetic for a real affection. But, by the aids which physical diagnosis affords, we arrive at a knowledge of the real seat of the disease, and are enabled to direct our curative measures with certainty and effect. But, when I say this, I beg of you to understand that I do not mean that you should be satisfied with a slight or single examination of the case. It may happen that these symptoms (wholly dependent on sympathetic irritation) may go on in this way for several days, and then a change may take place, and *organic disease of the lung supervene*. It is a common, but, generally speaking, a true observation, that sympathetic irritation of any organ (when long continued) has a tendency to terminate in actual disease. The circumstances which favour this change are, first, the excessive violence of the functional derangement, and, in the next place, its long continuance. You must be, therefore, on your guard, and examine the chest from day to day; and, if any signs of inflammation appear, meet them with promptitude, and you will generally succeed in averting dangerous consequences. There is another circumstance worthy of remark. If you have sympathetic irritation of any organ without fever, the chances are that it may go on for a long time without producing any bad effects; but, if it be accompanied by fever, there is a greater probability of its being converted into organic disease. This should make you cautious in cases of sympathetic affections of the lung in fever. As far as my observation goes, I would say, that, when disease of the gastro-intestinal surface is followed by an affection of the lung, the morbid action generally takes place in the left lung and in its lower lobe. There seems to be a greater sympathy between the left lung and the stomach, than the right, and you should therefore direct your attention particularly to the left side of the chest. It is a curious fact, that inflammation of the lower part of the left lung is very frequently connected with pericarditis and gastritis. There seems to exist a very remarkable sympathy between organs on the same side of the body. This is a curious fact, and demands some other explanation besides that which was given by Mr. Hunter, who attributed it to contiguity of position.

The last subject to which I shall allude is, that a liability to chronic disease of the lung very frequently occurs in those cases of pulmonary derangement during fever, and this is apt to come on after the symptoms of the original attack have subsided. In several instances of this kind, you will observe that the bronchial symptoms which are present in fever, and which do not excite any degree of alarm, will very often, on the subsidence of the fever, assume a more threatening character, and pass into phthisis. There

is something in the course of fever which has a strong tendency to develop tubercles in persons who are in any way liable to them. We have had numerous examples of this fact among the fever patients treated in the Meath Hospital. Such of you as are anxious for more detailed information on this point, I would beg leave to refer to some researches of mine on this subject, published in the Transactions of the College of Physicians. In the epidemic of 1826, it was a common thing to see our patients, after recovering from fever, attacked and carried off by phthisis. This form of phthisis ran through all the usual stages, from the recent formation of tubercles up to the formation of cavities, and was generally very rapid in its progress. In one case the patient died of consumption in seven days after the subsidence of the fever.

If, then, you find towards the close of fever that the patient has dulness under one or both clavicles, with feebleness of respiration, and the other signs of phthisis, you should be on the look-out for tubercles. If you find the dulness increase every day, you may be pretty sure of the occurrence of tubercular development. On the other hand, you should remember, that, of all the cases in which bronchitis is most likely to simulate phthisis, it is when it comes on after fever. A patient, getting an attack of bronchitis after fever, will, in that weak and debilitated condition, exhibit in a short time all the symptoms of phthisis. He will have bad cough, laborious breathing, great emaciation, and well-marked hectic; yet, in many instances, I have seen such patients recover under a judicious treatment.

To-morrow we shall again take up the consideration of the pulmonary system; and this, with some observations on the secondary lesions of the gastro-intestinal surface, will conclude what I have to offer on the subject of fever.

LECTURE XLIII.

Peripneumonie des agonizans of Laennec—Congestion of the lungs from position—Avenbrugger's opinions on—Precaution of Boyer—Importance of position in typhus—Treatment of catarrh and pneumonia in typhus—Principles of treatment—Management of excessive secretion—Employment of emetics—Use of sulphate of quinine and opium in injections—Typhoid pneumonia—Gastric symptoms in fever—Broussais' physiological theory—Brown's *asthenia* and *asthenia*—Remarks of the physiological school—Different treatments of fever.

Before we proceed to the consideration of the abdominal symptoms of fever, there are a few more pulmonary affections on which I wish to make a few observations. There is one lesion of the lung, not unfrequent in this disease, which consists in a congested state of the posterior and inferior parts of the lung, and is chiefly observed in persons who labour under great debility, and who have

lain a long time on their backs. It is of importance to be acquainted with this, because there can be little doubt that though in all cases it is merely the effect of the fever, it may be, in many instances, the cause of death. You can easily conceive that it might be the cause of death, when you recollect that extensive congestion of the lung will produce asphyxia, by obstructing the pulmonary circulation. This affection has got from Laennec the name of *peripneumonie des agonizans*; but I feel satisfied, that, in giving it this appellation, he has taken only a limited view of the matter, for it may come on long before the fatal termination of the disease; in fact, it may occur in all cases where the patient has lain for a long time on his back, and where the fluids have undergone any amount of morbid change. Indeed, there seems to be some analogy between this affection and the discolorations of the skin of the back and nates, which result, in such cases, from a long continuance of the supine position, and which may be observed in many other affections as well as typhus. If you raise a patient in bad typhus, after having been in this posture for a considerable time, and examine the postero-inferior part of the chest, you will often find a degree of dulness on percussion, and perhaps some crepitus; but if the patient continues in the erect posture for a little time, these phenomena gradually disappear, and the lung resumes its ordinary permeability, except in cases where the congested state has been of several days' standing. Avenbrugger was aware of the occurrence of this affection of the lung. In his remarks on the use of percussion in the exanthematous diseases, he alludes to the occurrence of congestion in the posterior part of the lung, before the eruption makes its appearance. The celebrated Boyer has also alluded to this matter in his works. In giving directions with respect to the posture of the patient in case of fracture, he alludes to the probability of congestion of the lung from this cause, and dwells on the dangers likely to arise from long lying on the back. It would appear, from some modern researches of M. Piorry, that some persons are much more liable to this condition than others, particularly those who labour under old age, weakness, or disease of the heart.

Now, suppose you have a case of typhus, in which the position on the back has been long maintained, and particularly if the patient be advanced in life, in a state of great debility, or labouring under organic disease of the heart, you should be on the look-out for this congested state of the lungs. In some of these cases it has been found that local bleeding, counter-irritation, and changing the position as much as can be done, have succeeded in removing this condition; but in the vast majority of cases it has occurred in persons reduced to a very weak and exhausted state, and at a period when the fatal termination was fast approaching. We can therefore say little or nothing about its treatment. One thing appears certain, that although we cannot remove this affection when once established in the last periods of life, we may do a great deal towards preventing its occurrence; and for this purpose

nothing is of so much importance as *attention to position*. We know that it is the postero-inferior part of the lung which is subject to this mechanical hyperæmia and consequent solidification. Now, in the early stage of this affection, and in milder cases, we observe that this incipient congestion disappears when the patient is placed in the erect position, and from this we derive the following indication, that in all cases of typhus, where there is great prostration of strength, and where there is reason to suspect a deterioration of the fluids, it is advisable to change the position of the patient frequently. You should be careful in making him lie on his side, changing his position occasionally from one side to the other, and in not permitting him to lie on his back. Four circumstances tend to produce this hypostatic pneumonia: first, lesion of innervation; secondly, the change of the fluids; thirdly, position; and, lastly, the want of proper action in the lung; for when the patient is lying on his back, the posterior part of that organ expands but little on respiration. If you then place him upright, or lay him on his face or side, you get rid, more or less, of the bad effects of position, and enable him to counteract the inaction of the lung, the full expansion of which seems to be a powerful means of relieving this apparently mechanical congestion. In many cases, too, I have found that the use of the cupping-glasses over the affected position, followed in some cases with a blister, had the best effect in removing the affection. In the case of this hypostatic pneumonia in fever, it will generally be right, to support the patient's strength, to give a little wine, and the decoct. polygalæ, with carbonate of ammonia.

Let us now proceed to the treatment of catarrh and pneumonia in typhus. I have already stated that in most of these cases there is a complication with disease in the digestive system. Suppose, then, you meet with a case of typhus, in which you find the mucous membrane of the lungs extensively inflamed, is it advisable here to perform general bleeding? Some time ago I was strongly opposed to the use of general bleeding in any case of this secondary typhoid bronchitis, because we found that in many cases it did little good, and in some, by reducing the powers of life, seemed to do great injury. Since then, however, I have been less opposed to this practice, having witnessed its good effects in cases where the disease occurred in persons of robust constitution, at an early period of fever, and accompanied by hot skin, quick pulse, considerable reactive powers, and great dyspnœa, produced *not by the supervention of enormous secretion into the bronchial tubes, but by their inflammatory condition*, which could be ascertained by the stethoscope. Dyspnœa depending on inflammation may be relieved by bleeding; dyspnœa depending on excess of secretion is not. Here the loss of blood will increase the danger of asphyxia, for as the general strength is diminished, the powers of expectoration cease, and tracheal rattle comes on. Here we have a beautiful practical illustration of the utility and value of the stethoscope.

If, then, you have a case of extensive bronchitis occurring as a

secondary affection in fever, in a young robust subject, with heat of skin, quickness of pulse, considerable powers of reaction, and dyspnœa, not produced by over secretion at an early period, you will be authorised in taking blood from the system generally. I say this, because I have, in several of such cases, employed venesection with the best effects. But, as a general rule, I believe that, in cases of pulmonary disease occurring in fever, local bleeding is preferable. You will always be able to give relief by leeching or cupping the chest; indeed, I have frequently seen the patient relieved by dry-cupping over the thorax to so great a degree that I look upon it as one of the most important measures you can adopt in the bronchitis or pneumonia of typhus. I have frequently removed a degree of congestion *sufficient to cause manifest dulness*, by this measure alone. By this proceeding, the congestion has often disappeared in twenty-four hours, and with wonderful relief to the patient. You may also apply blisters, and these valuable agents may be employed at an earlier period than in cases of ordinary bronchitis. If the stomach will bear it, you may give small doses of ipecacuanha or calomel, and Dover's powders.

Suppose you meet with a case of this kind at an advanced period of fever, and find that the patient is very low, has great dyspnœa and cough, and that the stethoscope informs you that there is a copious secretion into the bronchial tubes. Here you must be prompt and decided in the employment of your therapeutic means, for you have a very dangerous affection to contend with, and if relief be not quickly afforded, the patient will die of asphyxia. You must immediately have recourse to extensive counter-irritation, and the use of the decoct. polygalæ, with carbonate of ammonia, and the camphorated tincture of opium; and if the patient's strength be much reduced, you must give wine with freedom, and light nutritious food. One of the most favourable circumstances connected with the use of wine in these cases, is the abatement of the frequency of the pulse. Great attention must be paid to the temperature of the surface. I have long been in the habit of ordering the patients to be clothed in soft flannel, and I feel certain that this measure is one of the utmost utility. If after using wine you find that the patient's pulse has come down, you may be sure that it was required, and that it will do good. Sometimes, during the existence of this condition of the lungs, a state of impending suffocation will occur, accompanied by tracheal rattle. Here the patient is in danger of perishing from a mechanical cause, and this may happen even at a time when he has not arrived at a state of great debility; in fact, while his pulse is still tolerably full, and his skin and extremities warm. It is in this stage that emetics have been recommended. When lecturing on chronic bronchitis, I gave the results of my experience on this point, and stated that, in many instances, powerful emetics, even though repeatedly administered, had failed to act on the stomach. The insensibility of the stomach here depends on various causes; on lesion of innervation, on asthenia of the stomach, produced by determination of

blood to another organ, and, as some think, on that condition which is observed to accompany the want of proper aëration of the blood. From whatever cause this insensibility proceeds, we frequently see that emetics, which in the ordinary state of the stomach exercise their full action, prove inefficient in many cases of this description. A scruple of ipecacuanha is given, then an emetic of sulphate of zinc, and, when this fails, an emetic of sulphate of copper, and these will not only fail in exciting emesis, but may also do a great deal of harm. Again, in many cases, even after vomiting has been produced, and the patient is, as it were, snatched from the jaws of death, we find that the same distressing symptoms return in the space of six or eight hours. Another emetic is then administered, and relief may be again obtained, but the dreaded state of suffocation returns. We had a case in the Meath Hospital, in which free vomiting was produced four or five times, and yet the patient died afterwards of suffocation from the extent of the effusion into the air tubes. To a person who had not witnessed this case during its progress, the appearance of the patient after vomiting would not appear to indicate any thing alarming; yet the awful state of suffocation returned again and again, until emetics lost their power over the stomach, and the patient died asphyxiated. It is plain that if you can produce emesis you gain time for the administration of other remedies, and arrest the progress of the disease towards a fatal termination. This time, I need not tell you, must be employed actively and sedulously in the application of means calculated to remove the disease, and subdue the morbid state of the lung; and no one can deny the meed of praise to my enlightened friend, Dr. Mackintosh, for his bringing this subject before the profession. But it is only one of the many boons his vigorous mind has conferred upon medicine. In a case of this kind, my colleague, Dr. Graves, succeeded in arresting the disease by the application of moxa along the course of the eighth pair of nerves, in combination with other measures. In a late number of the *Dublin Medical Journal* he has handled this subject. In this paper (to which I would refer you) he has proposed the use of sulphate of quinine and opium in the form of injection, and gives the particulars of some cases of impending suffocation, in which these remedies produced very favourable results. He says that tonics and opium exercise a very powerful influence in arresting the super-secretion of the bronchial mucous membrane, and that it has been frequently observed in cases of ordinary catarrh, that these remedies, when injudiciously exhibited, have checked the expectoration. Now one great object in this disease is to prevent this excessive secretion which threatens life; and as our remedies for this purpose are, unfortunately, very scanty, any addition must be of great importance. Dr. Graves has given the particulars of three cases in which he tried opium and quinine in the form of injection. One of his patients was a delicate female, who had got a violent attack of influenza, followed by enormous effusion into the bronchial tubes. In a few hours her strength became quite exhausted, her counte-

nance assumed a livid hue, and, to add to her sufferings, she had nausea and vomiting. An enema of thin starch, containing ten grains of sulphate of quinine and thirty drops of laudanum, was ordered to be given immediately. Shortly after the use of this, she fell asleep, her breathing became less laboured, and she awoke with considerable improvement in her strength; this patient recovered. Dr. Graves next details another case of the same description, where there were great debility, alarming dyspnoea, and a sense of impending suffocation. In this case the quinine and opium were administered as before, and the patient recovered. In another case of a patient in the Meath Hospital, who had every symptom of approaching dissolution, the exhibition of the enema was followed by such good effects that on the next day she might be pronounced out of danger. I have not used this injection myself, but I think the principle on which Dr. Graves employs it is just, and, as far as a trial of it has been made, there is reason to hope for favourable results. At all events, we may look upon it as a valuable addition to our means of combating this terrible symptom.

Now, with respect to the pneumonia of fever, I have, in the first place, to remark, that this affection, though less frequent than bronchitis, is of equal importance. Suppose that, during the course of fever, your patient gets cough, hurried breathing, and perhaps some pain in the chest; he has dulness of sound extending over a considerable portion of the lung, and, on applying the stethoscope, you find crepitating *râles* and feebleness of respiration. In cases of this kind, the value and efficacy of treatment will mainly depend on the period of fever, the strength of the patient, and the nature of the preceding symptoms. The more the fever is of the inflammatory type, the earlier the period of the disease is, and the smaller the amount of prostration, the more favourable the prognosis. If the patient is of a robust constitution, the fever of short duration, and the prostration inconsiderable, you may venture on a single small bleeding, but this must be performed with caution, and always bearing in mind, that an attack of this kind requires very different treatment from the ordinary idiopathic pneumonia. We next proceed to the use of local bleeding, and of calomel and opium, if the stomach will bear it. It generally happens that the system will come under the influence of mercury in two or three days, but in some cases you will not be able to affect the mouth, and these you may look upon as the worst cases, and very likely to prove fatal. We often witness here a kind of struggle between the mercurial action and the disease. To-day the gums are slightly sore and the breath mercurial; to-morrow all this is gone, although the remedy has been steadily continued. Next day you will observe a slight return, and so on. In such a case as this, full ptyalism is almost never produced, and the disease is commonly the victor. I have only known two cases to recover out of a vast number in which we observed this circumstance. With respect to blisters, counter-irritation, diet, &c., the same rules apply here as to the catarrh of fever. Tartar emetic is altogether inadmissible in

the treatment of this kind of pneumonia; it can be used with safety in those cases only where free general bleeding may be employed, and this can never enter into the treatment of typhus, where general debility is ordinarily the most prominent symptom. Besides, there is in almost all these cases a complication with gastro-enteric disease, and where this exists, the use of antimonial medicines is always dangerous. On this account we give a preference to the calomel and opium, assisted by blisters, stimulant expectorants, and a light nutritious diet to support the patient's strength. When you find the skin pale and cool, the pulse weak and small, and the features shrunk, you should always give wine. This disease is generally very slow in resolving, the patient convalesces tediously, and requires to be constantly watched.

We now come to the most important part of the analysis of the symptoms of fever, namely, those which have been termed gastric. In using the term gastric, I wish it to be understood as referring not merely to the stomach, but also to the whole digestive tube, the liver, and spleen. *Of all the symptoms of fever, the gastric are the most frequent, the most numerous, the most remarkable for their extent, persistence, and insidious latency, and the most frequently connected with actual organic change.* Of this proposition I think there cannot be the slightest doubt. If we look to the different parts of the system, we shall find that, of all others, the digestive tube is most liable to disease, whether we consider the typhus of this country, or the malignant fevers of warm climates. To undertake the treatment of fever, without an accurate knowledge of the physiology and pathology of the digestive system, and the intimate connection between it and fever, would be nothing better than practising in the dark; and I feel perfectly convinced that it is to the want of this knowledge we are to attribute the ignorance, the empiricism, and the want of success, which have so long disgraced the treatment of fever. If you were to do nothing for years but study the pathology of the digestive system in fever, your time would be well and profitably employed, and you would have occasion, almost every day of your professional life, to congratulate yourselves on the importance of the knowledge you have obtained. There is no system so intimately connected with fever as the digestive, and this leads us to a consideration of what has been termed the physiological theory of fever, which has created such a sensation in modern times. It may not be amiss, therefore, to make a few observations on the rise and fall of the physiological doctrines, before we proceed to discuss the gastric symptoms of typhus.

The physiological theory, as propounded by Broussais, may be considered under two general heads: first, those doctrines which apply to disease in general, and, secondly, those which apply to fever and its varieties. M. Broussais appeared before the medical world at a time when it was exceedingly natural that a man of his great talents should erect himself into the founder of a new school. Humoral pathology, which had been gradually falling into

disgrace, had already received a severe shock from the results of anatomical investigation; and it was before the time of Broussais that the immortal Bichat had described the different tissues which enter into the composition of the body, and the division of nerves into those of animal and organic life. Pathological anatomy was advancing rapidly in its brilliant discoveries; and, as every day added to the mass of important facts bearing on the nature and seat of disease, Broussais announced that many affections, looked upon as essential by the old pathologists, could be distinctly traced to a local cause; in fact, that a numerous class of diseases were reducible to the phenomena of local inflammation. These discoveries were hailed with acclamation by a host of admiring disciples, and were expected to lead to the most splendid and important results. The captivating simplicity of a doctrine which attributed all diseases to changes in the solids, thus deriving additional support from the investigations of anatomy, and coinciding with the feelings of the time, could not fail in attracting a host of admiring followers. Besides, most persons were disgusted with the glaring absurdities which were put forward as part of the doctrines of the humoral school. Their pathology consisted almost exclusively of crude notions of certain changes in the fluids, which they termed alkalinity or acidity of humours, corruption of bile, inspissation and tenuity of blood, putrefaction and fermentation of fluids. These, with some strange ideas of the existence of peculiar unknown fluids in certain parts, as the spirits of the brain, the cholera of the liver, &c., were the floating and obscure notions which preceded pathological anatomy. All the ingenious, the enquiring, and those who regarded not dogmas because they had been propounded by great names, became disgusted with the intangible obscurity and manifest absurdity of the doctrines of the humoral school; and embracing every new discovery in science which appeared calculated to shed a better and a steadier light on the nature of disease, they embarked with ardour in the pursuit of pathological anatomy. As anatomy and pathology advanced, the healthy and then the diseased condition of the solids became the topics for investigation, the attention of medical men was turned from the speculations of humoralism to the study of known and perceptible alterations in the solids; and hence it was that attention became exclusively directed to the solid parts of the body.

A little before this time, appeared the celebrated Dr. Brown, of Edinburgh, one of the most remarkable men of modern times, whether we consider his boldness, or his ingenuity in defending his doctrines. He declared that life was supported by stimulants, and that disease was referable to one or two states of the system of an opposite character—one of which he called *sthenia*, the other *asthenia*. Thus he divided all diseases into those of *sthenia* or excess, and of *asthenia* or deficiency of vitality. All diseases being thus reducible to one of two conditions, all treatment was reduced to the employment of general stimulation and general depletion; for these terms applied not to the local disease, but to the state of

the whole economy. Bichat, however, demonstrated the separation in function and structure which exists among the various organs of the body; and M. Broussais, being fully aware of the untenable nature of some of the doctrines of the sthenic and asthenic theory, proceeded in forming his own doctrines, and applied the principles of Brown, *not to the whole economy*, but to each separate organ and tissue. He declared that one part or organ might be in a state of asthenia, or, as he termed it, have a minus degree of vitality, at the same time that another organ was in a state of sthenia, and enjoying a plus degree of vitality. He thus modified very considerably the theory of Brown, applying to each separate organ and tissue those terms which Brown had applied to the system generally. Brown thought that there could not be sthenia of one part and asthenia of the rest at the same time; Broussais declared that all diseases were referable to a plus or minus degree of vitality, and that either of these conditions might exist in any particular tissue or organ; and that one part might be in a state of sthenia, while another part, or even the rest of the economy, might be at the same time in a state of asthenia, and *vice versa*.

Disease, then, according to Broussais, consisted in an exaltation or depression of the vital action of the affected part; and, as symptoms of disease were reducible to irregularity of function, and function being the product of organs, and organs being the solids of the body, it followed that affections of the solids alone are the cause of all diseases, and therefore that the doctrine that fever is an essential disease or general affection of the system, not referable to disease of any of the solids, was wrong, and the cause of it was to be sought for in some changes occurring in the solids. Now, as inflammation of the digestive tube is one of the most constant lesions observed in fever, he took it as the pathological character of fever, and therefore concluded that fever is only symptomatic of gastro-enteric inflammation, and not in itself an original affection of the whole system, of the nature of which no one knew any thing.

Such are the chief arguments of the physiological school. They contend that all the phenomena of fever are reducible to the symptoms of local disease of the digestive tube, and the sympathetic effects produced by it on various parts of the system. Many circumstances contributed to render this theory plausible, and recommended it to the adoption of medical men. In the first place, there was the great similarity in symptoms, which is so frequently observed to exist between the ordinary idiopathic gastro-enteritis and typhus fever. In the next place, the constant occurrence of cases of idiopathic gastro-enteric inflammation, in which the accompanying fever, though purely symptomatic, bears a strong analogy to typhus. This resemblance of the fever of gastro-enteritis to typhus was one great cause of the theory being so well received. Another circumstance was the discovery of the fact, that of all systems the digestive is that which most intimately sympathises with, and produces the most striking effects on, the brain and nerves. To give an example of this, if you compare the

inflammatory affections of different systems and organs, with respect to their tendency to produce prostration of strength, you will find that this tendency is produced by affections of the digestive system to a much greater degree than by those of any other. A patient may have extensive pleuritis, or pneumonia, or pericarditis, or even cerebritis, without any remarkable prostration; but let him get but a slight attack of gastro-enteric inflammation, and the prostration of strength becomes almost immediately a striking symptom of his disorder. Now in typhus, also, this prostration of strength is one of the most remarkable phenomena, and, as it has been generally observed that the prostration of strength in typhus is proportioned to the amount of gastric derangement, the disciples of the physiological school were thus furnished with an additional argument. A third circumstance was the known tendency of gastro-enteric disease to produce symptoms of morbid action in the respiratory and nervous systems. We find an infinite variety of these symptoms in typhus, and we know that in other cases they can be traced to the sympathy which attends irritation of the digestive apparatus. One of the most remarkable circumstances in typhus is the alteration of the fluids of the body. If you draw blood from the system, you will often find it of a dark colour, without any appearance of buff, that there is but little separation of serum, and that the clot is soft, without consistence, and easily broken up. There are also various morbid alterations in the perspiration, urine, and alvine discharges. Now, of all diseases, derangement of the digestive organs is the most likely to produce this alteration in the fluids. The digestive system is the main spring of the economy, the great alembic in which the materials for repairing the waste of the system are elaborated, the fountain from which the blood is supplied for all the various purposes of secretion. Now, if the digestive system be in a state of disease, and incapable of performing its functions in a perfect manner, it cannot furnish the materials of good or wholesome blood; and hence it is that the most remarkable alterations of the fluids of the body are observed in cases of gastro-enteric disease. A fourth circumstance, in favour of the physiological theory, is the great frequency of symptomatic fever in warm climates; and it is well known, that, in these countries, the digestive system is much more liable than any other to the impressions of disease. Fifthly, the results of pathological anatomy seemed to give support to the opinion, that fever is symptomatic of gastro-enteric inflammation, for, in a vast number of cases, we find not only irritation but even extensive inflammation and ulceration of the mucous membrane of the digestive tube. Lastly, the results of treatment tended to give these doctrines additional and, indeed, most important support. The founder of the physiological school claimed credit for his doctrines, inasmuch as a practice, based on these doctrines, had been much more successful than that pursued by his predecessors. This admits of a twofold explanation. In the first place, as the theory of fever was founded on local inflammation, it followed of

necessity that the treatment should be antiphlogistic. Now, it is a well known fact, that the antiphlogistic treatment of fever is, on the whole, more successful than the stimulant. Here was one circumstance, the change of practice from the old stimulant plan to the antiphlogistic, and consequently a greater degree of success. In the next place, was the remarkable efficacy of local antiphlogistic means, a practice for the introduction and inculcation of which the medical world is deeply indebted to Broussais. No one, I think, will venture to deny the great utility of local bleeding in the treatment of fever. But, though we must confess that it is one of the best remedies we can employ in a vast number of cases, still we do not regard it in the same point of view as the followers of Broussais, nor do we conclude that a fever is symptomatic, because leeching has been followed by favourable results. Local bleeding is of use, because it relieves or modifies a disease which has sprung up in the course of the fever, and which re-acts on the economy in proportion to its intensity, and the susceptibility of the patient; and the cessation of other symptoms, which frequently ensues on the application of leeches to the belly, is to be explained on the principle, that by removing an irritation, which powerfully re-acts on the system, and thereby impedes the efforts of nature, we reduce things to a state of simplicity, and favour the ordinary tendency of the disease to a critical termination:

Such are the circumstances which have contributed to support and advance the theory of Broussais; circumstances which, I think, have not been sufficiently considered, or properly appreciated by those who have decried his doctrines and practice. On the other hand, it is true that this theory was formed too hastily, and defended too dogmatically. Happy would it be for mankind if it were true, for then we should have a key to the nature and treatment of the most extensive and fatal of all human affections. The great error of Broussais consisted in the hasty generalisation of the facts on which he based his doctrines; these were neither sufficiently connected, nor was their value sufficiently appreciated. Since the publication of his opinions, many facts have been brought forward; and these of so strong a nature, as to induce men to take a different view of the subject, and to consider the gastro-enteric inflammation (so frequently observed in typhus) not as the cause and essence of the disease, but merely as one of the various secondary lesions which arise during its progress; that fever may occur without gastro-enteritis (which is the rarest cause), or with very little of it; that there is no constant relation between it and the extent of the disease; and, therefore, that a treatment, chiefly limited to the employment of local antiphlogistic means, is not founded on a rational view of the subject.

LECTURE XLIV.

Different lesions in typhus fever—Absence of gastro-enteric symptoms—Cases by Bouillaud, Andral, Louis, &c.—Andral's arrangement of fevers—Louis's opinion on the anatomical character of fever—Analogy of typhus with small-pox—Absence of pain in enteritis—Means for diagnosis—Variety of disease in serous tissues from typhus—Treatment of the digestive symptoms of typhus—Hiccup—Tympanitis in fever, treatment of—General treatment of fever—Conclusion of the course.

To-day we have to consider the treatment of the gastric symptoms which occur during the course of typhus.

At our last meeting, I endeavoured to impress on your attention, that of all the lesions of typhus those which belong to the digestive system are the most remarkable, on account of their extent, variety, frequency of occurrence, and number of sympathetic relations; and that, consequently, this was, perhaps, the most important part of our analysis of the phenomena of typhus. I spoke of the theory of Broussais, and showed that many circumstances tended to render that theory at least exceedingly plausible; but that the progress of medicine had tended to shake the stability of the physiological doctrines, as far as typhus is concerned, and to prove that, however extensive and frequent lesions of the intestinal canal may be, still they cannot by any means be looked upon as *the first link in the chain of morbid action*, nor typhus be considered as merely symptomatic of gastro-enteritis. If such were the fact, these lesions should exhibit an uniform constancy of occurrence, and their amount should be always proportioned to the severity of the symptoms. We cannot recognise either of these characters in typhus. The symptoms are very seldom in proportion to the gastro-enteric affections, and they are not of constant occurrence. Indeed, cases of typhus have been recorded by medical writers, in which the phenomena of gastro-enteric inflammation were totally absent. You will find several cases of this description mentioned by Bouillaud, Andral, and other practitioners of eminence; and even Louis, who says that the anatomical character of fever seems to consist in organic derangement of the mucous glands of the intestines, acknowledges that he has met with cases of typhus without any gastro-enteric lesion. We may then, I think, freely admit the conclusions which Andral has drawn from a most extensive analysis of the pathological characters of fever, that lesions of the digestive tube are not of constant occurrence, and that fever may exist independent of gastro-enteric disease; but, on the other hand, that in the majority of cases we meet with traces of more or less extensive derangement of the intestinal canal. If this be true, and I think there can be little doubt of it, it is quite sufficient to prove how important, how indispensable, must be the study of the digestive system in typhus. If you refer to the best practical treatises on fever, published in this country, you will meet with innumerable instances of the remarkable prevalence of gastro-intestinal disease in typhus; and in France so constantly have inflammation and

ulceration of the mucous glands of the ileum, and other portions of the intestinal canal, been discovered, that Andral, in his last edition of the *Clinique Medicale*, ranges fevers under the head of abdominal diseases. It is very probable that, in France, these secondary lesions are more frequently met with than in this country, and this I freely admit; but I can say, that even here they are exceedingly common, and form a most important and prominent feature in the pathology of typhus.

I need not tell you that the part most commonly affected is the mucous expansion of the digestive tube. The stomach and the lower third of the ileum, particularly the latter, are most liable to disease, and it is here we generally look for organic alterations of the bowels in fever. We find many cases of typhus without any disease of the stomach, or with lesions of a very trifling nature; but this observation does not apply by any means in an equal degree to the small intestine. Again, the morbid appearances of the stomach in this disease have not in them any thing special, or connected with the phenomena of typhus, and they exhibit very little difference in persons who have died of typhus and those who have not; but in the small intestines there is, under the same circumstances, a very remarkable frequency and uniformity of lesion. Louis has stated it as his opinion, that the anatomical character of fever is inflammation of the intestinal mucous glands. Where gastro-enteric symptoms are present, we generally find, on dissection, that the mucous membrane, particularly of the lower third of the ileum, is in various states of disease. In some cases, the lesion is confined to the mucous membrane itself, which becomes softened and unusually vascular; but, in the majority of instances, you will find both the mucous membrane and glands affected. The solitary and aggregate glands become enlarged, and appear to be increased in number. This, however, is not the case. Under the influence of the inflammatory process they become hypertrophied, and, being thus rendered prominent and distinct, vast numbers of them are easily seen, which, in the natural state, would have escaped the eye of the closest observer. This disease sometimes appears to affect the glands primarily, and has been termed *exanthème interne*, from its resemblance to small-pox. It commences with increased vascularity and turgescence, followed by softening and ulceration.

In the first accounts of this affection, it was supposed to be quite analogous to small-pox, and that the one was an eruption on the mucous surface of the bowels, as the other was an eruption on the skin. This opinion, however, has been disproved by the following facts:—In variola, we all know that there is a constant relation between the state of the cutaneous affection and the period of fever. There is no such thing as this in inflammation of the intestinal mucous glands. In some advanced cases of fever, we find the glands only in the first stage of inflammation; while, in other cases, where fever has been of very brief duration, we find them in the last or ulcerative stage of the disease. These facts tend to invalidate the notion of there being an analogy between this affection

and small-pox, and shows that what has been looked upon as a pustular eruption affecting the surface of the intestinal tube, is nothing but an inflammatory state of its mucous glands, which may occur at any stage of typhus, and bears no relation to the particular period of fever.

With respect to the extent of this disease of the intestinal glands, there is considerable variety. In some patients we find it very extensive, and forming a large sheet of ulceration; in others it is confined to a few small isolated patches. We may meet with it under its different phases of turgescence, softening, or ulceration; or we may have these three states co-existing in the same individual.

It may be asked here, if this ulceration of the glands be a common occurrence in fever, how is it that persons will recover? This is answered by the researches of pathological economy, which have shown that these ulcerations, even though numerous and extensive, are capable of taking on the process of cicatrisation and cure. This fact has been established by the discovery of numerous cicatrisations in the situation of the glands of those persons who recovered from fever and died of some other disease. Under these circumstances we find the mucous membrane of the intestine presenting numerous minute patches, covered by a fine membrane, destitute of villous processes, and resembling those cicatrices on the surface of the body, which point out the situation of portions of the skin which have been deprived of vitality.

When speaking on the subject of enteritis, I drew your attention to a valuable observation in Dr. Cheyne's report, that, in many cases of fever, what has been termed doubtful and imperfect convalescence, depends on inflammation of the intestinal mucous glands. This leads us to the knowledge of a curious and important fact, namely, that a crisis may take place and fever subside, and yet the disease of the digestive tube may still go on, and even terminate in the destruction of life. Circumstances analogous to this have been also observed in other diseases, in which we find the local affection remaining after the subsidence of the fever, by which it was accompanied or preceded.

In speaking of enteritis, I drew your attention also to the occasional absence of all pain in cases where dissection proved the existence of extensive inflammation. This occurrence is still more remarkable in typhus. It has been often remarked, that idiopathic inflammation of the mucous glands is generally a painless and more or less latent affection. Now, if this be the case with idiopathic disease, how much greater will be the chances of latency and absence of pain in the glandular derangement which occurs in typhus, where the general sensibility is impaired, and where every form of local inflammation is remarkable for the trifling amount of suffering it produces, and the insidious latency of its origin and progress. And hence it is that extensive disease may arise and proceed unchecked in its career to a fatal termination, without ever attracting the notice of the physician, or eliciting

a single expression of suffering, or even uneasiness, from the patient. But the absence of pain is no proof of the non-existence of this treacherous malady. I have said before, and I now repeat the expression, that inflammation of the intestinal glands may exist in its most deadly form without any manifestation of pain or discomfort on the part of the patient, which would indicate where the evil lurks, and teach us where we should apply the resources of our art for its palliation or removal.

You will ask me, is there any thing capable of furnishing us with means of detecting this insidious affection? There is. Sometimes we find a degree of tenderness on making firm pressure over the region of the ileum, but this is by no means of constant occurrence. But the most important aids towards forming a correct diagnosis are furnished by three symptoms, most commonly found in connection with these latent pathological changes. The first of these is great thirst without vomiting, and, generally speaking, a preference for warm drinks. The next is early tympanitis. The last is the occurrence of diarrhœa. These are the symptoms generally observed in cases where disease predominates in the small intestines. Thirst, with a preference for warm drinks, is a very common symptom of this affection; and the occurrence of early tympanitis is, generally speaking, an ordinary accompaniment, and tends to throw much light on the nature of the disease. With respect to diarrhœa, it may be stated, as a general proposition, that, in the course of typhus, diarrhœa, when not critical, is one of the most positive indications of an inflammatory condition of some portion of the digestive tube. In many cases, diarrhœa does not appear until ulceration of the mucous membrane has taken place. Previous to the occurrence of ulceration, we have an increase of fever, with thirst, and perhaps some tenderness of the belly, and then diarrhœa sets in. There is nothing more common in hospital practice than to see a case of apparently simple essential fever going on without any particular abdominal symptom for ten or fifteen days, when it begins to change its character, diarrhœa sets in, and runs the patient down rapidly, and, on dissection, we find extensive ulceration of the small intestines.¹

I have told you that in cases of fever, where the digestive system is engaged, we very seldom meet with disease of the peritoneum, or other serous tissues. Still it is possible, and examples of it are occasionally observed. In some instances, where this inflammation of the mucous membrane and glands of the small intestines (or, as it has been termed, dothinenteritis) existed, we have seen peritonitis coming on at an advanced period of the disease. Now, this may occur by the direct transmission, or extension, of disease from the mucous to the serous surface, or it may arise from ulceration producing perforation of the three coats of the intestine, and effusion of its contents into the cavity of the peritoneum. A case of

¹ See, on the presumed distinctions between typhus and typhoid fevers, the "American Medical Intelligencer," April 1, 1837, p. 10.—*R. D.*

this description is almost invariably fatal. Neither is this perforation of the intestine an accident of such rare occurrence as might be supposed. Indeed, when we consider the frequency of dothin-enteritis, we are rather inclined to wonder that it is not more common. This, however, seems to be explained by recollecting, that these ulcers almost invariably open on the mucous surface of the intestine and spread laterally; that they are generally superficial, and have very little tendency to pass deep into the wall of the intestinal canal. Still a patient in typhus, labouring under dothin-enteritis, is in a very bad and precarious condition. A man in fever, with prostration of strength, thirst, tympanitis, and diarrhoea, is always in danger; for he may in a moment be seized with alarming peritonitis, and sink in a few hours. Under these circumstances, you may be pretty sure that he has been carried off by peritoneal inflammation, and you may make the diagnosis of perforation of some part of the last twelve inches of the ileum. The truth and accuracy of this diagnosis we have confirmed in the Meath Hospital by numerous dissections. Its principle is founded on three circumstances: the pre-existence of symptoms of disease of the mucous glands; the *sudden* supervention of symptoms of peritonitis, without any thing to account for it; and the rapid sinking of the powers of life. I have made this diagnosis with success in ten cases, which occurred at the Meath Hospital, and in others in private practice. You will find a great deal of interesting matter on this subject in the *Anatomico-Pathological Researches of Louis*, who first made and established this diagnosis. I would also beg leave to refer you to my paper on Peritonitis, published in the *London Cyclopædia of Practical Medicine*.

We come now to the treatment of the digestive symptoms of typhus. We often find the patient complaining of thirst, with a desire in some cases for cold, in others for warm, drinks. As far as I have seen, the preference for cold drinks is most evident in cases where signs of irritation of the stomach exist, as tenderness of the epigastrium, nausea, and vomiting. One of the first observations I shall make is, that, in all cases where there is great thirst, you should be cautious in the administration of stimulants. The circumstance of intense thirst always points out the existence of irritation in the stomach or intestines, or both, and this should make you very cautious (particularly in the early period of fever) in giving any stimulant, whether in the way of medicine, food, or drink. If the thirst be great, and accompanied by vomiting, the best thing you can do is to apply leeches, and this you will find productive of very great relief, particularly where there is epigastric tenderness. You may allow the patient also to drink cold water *ad libitum*. Many persons are afraid of doing this; but it never does any harm, and is a great source of refreshment to the patient. We frequently give ice in cases of this description in the Meath Hospital. Against this practice, also, an outcry has been raised by some unthinking persons; they say cold water, and more particularly ice, must be injurious, because we know that under cir-

cumstance of fatigue, &c., their use has brought on a gastritis. This is a false argument. The circumstances are totally different, and the remedy in fever, with gastric irritation, has a wonderful value. If after the use of leeches, ice, and cold drinks, you find the disposition to vomit continuing, you will have recourse to the administration of opiates, and those best adapted for this purpose are the acetate or muriate of morphia, or the black drop. In cases of obstinate vomiting, the application of a blister to the epigastrium, and the sprinkling the raw surface with a small quantity of the acetate of morphia, will be found very useful. With respect to effervescing draughts, which are so commonly employed in cases of this description, I have already stated, when lecturing on gastritis, that their employment is very questionable, if not absolutely objectionable. They are very frequently rejected by the stomach, and the distension, which is produced by the evolution of gas, causes much distress and irritation.

Another symptom, very often observed in connection with gastric derangement in fever, is hiccup, a symptom of which, in the great majority of cases, we know very little. In the early period of fever, I have found it co-exist with inflammation of the cardiac orifice of the stomach; in the advanced stage, as far as my experience of it goes, it seems to be more connected with general lesion of the nervous system. With these views, we may divide the treatment of hiccup into two parts; first, that which occurs in the early stage of fever, and, in the next place, that which comes on towards the close of the disease. In the former, we have found leeching, cold drinks, and abstinence, the best remedies; in the latter, anti-spasmodic and stimulant remedies appear to be more applicable. By reasoning, then, from experience, we find that hiccup, in the beginning of fever, demands local antiphlogistics; but in the latter periods, when there is profound adynamia, it may be treated with opiates, antispasmodics, and even stimulants.

A very common symptom in fever is tympanitis. Here, again, I beg to refer you to my lecture on enteritis, as all I have said there on this point is equally applicable to the present case. Tympanitis sometimes occurs at an early, sometimes at an advanced, period of the disease. It is a symptom which is very often badly treated. Many practitioners, the moment they recognise its existence, and without ever enquiring into its cause, prescribe at once their favourite nostrum, turpentine, and bad consequences are too often the result. What generally takes place is this, that after the exhibition of turpentine the tympanitis disappears, but its removal is purchased at too dear a price. All the symptoms of fever, thirst, heat of skin, excitement of the vascular and nervous systems, are increased, and, what is still more annoying, the tympanitis returns, and is as bad as ever. The rule by which you should guide your practice in the treatment of tympanitis is this: if it occurs at an early period, when the skin is hot, the pulse quick, and the strength undiminished, it is a sign of irritation or inflammation of the mucous membrane, and is to be met by appropriate treatment.

Never employ stimulants for the removal of tympanitis occurring at an early period of fever. But in the advanced stage, where the general inflammatory symptoms have subsided, where the skin is cool, the pulse weak, and the strength reduced, then you may exhibit turpentine with decided advantage; but as long as the condition of the patient requires and admits of local antiphlogistic treatment, never have recourse to it.

Diarrhœa is another very common symptom in fever. I have told you before that diarrhœa, when not critical, may be looked upon as an unequivocal indication of disease of the mucous glands of the intestines. With respect to the period of its occurrence, we may have it either in the early or in the advanced stage of fever. When it makes its appearance at an early period of the disease, it should not be too hastily interfered with; for great danger has been found to result from stopping it at this time by the exhibition of astringent medicines. Bear this in mind. It is a common practice with some persons to prescribe astringents the moment diarrhœa appears. Now, this is founded on false notions of pathology. In cases of inflammatory disease, one of the principal modes which nature adopts in giving relief is, by super-secretion from the affected organ. Now, if, under such circumstances, you arrest a diarrhœa of this kind occurring at an early period, you do it at the risk of producing intense enteritis or effusion into the peritoneum. When it comes on at an early period, all you should do is to apply a few leeches to the belly, and give small doses of hydrarg. c. creta with Dover's powders. You may also use mild anodyne injections; but in the treatment of this affection, always seek to arrest, not the disease but its cause. This is the true mode of proceeding.

In the advanced period of fever, where the pulse is small and low, and the strength sinking, diarrhœa is a very alarming symptom; for, if not speedily arrested, it will run the patient down in a very short time. Here, and here only, is the use of astringents demanded, and it is here that their beneficial effects are manifest. To arrest this dangerous symptom which is precipitating the case to a fatal termination, you must use opiates combined with astringents, and you may give wine if necessary.

From a consideration of the physiological characters of the digestive system—from the study of its pathological condition in fever—from a review of its form, structure, and functions, in health—and from the fact of its extraordinary liability to inflammation in fever, I think you will be prepared to admit, that the common practice of purging day after day, in typhus, must be productive of the worst consequences. I beg you will impress this firmly on your minds, for so universal has this practice been, that you will find it difficult not to be carried away by the tide of prejudice, when you come to be engaged in the duties of your profession. A common practice has prevailed in these countries, and, indeed, still exists to a very great extent, of making the patient take purgative medicine every day; and this, I regret to say, is too often done even in cases where the surface of the small intestine presents

extensive patches of ulceration. Now, I will ask you, can any thing be so barbarous as this, or can it be exceeded in folly and mischief by the grossest acts of quackery? Here we have an organ in a state of high irritation, and exhibiting a remarkable excitement of its circulation; and yet we proceed to apply stimulants to that organ, and to increase the existing irritation. Would it not be absurd, in a case of inflammation of the knee or elbow joint, to direct the patient to use constant exercise and motion? Would it not be very strange practice to apply irritants to a raw and excoriated surface? Yet something equally absurd, and equally mischievous, is done by those who employ violent purgatives in a case of inflammation of the digestive tube in fever. This has been the great blot in the history of British practice. Calomel and black bottle, and even jalap, and aloes, and scammony, have been prescribed for patients labouring under severe and extensive dothenteritis. Morbid stools are discharged; and the more morbid they are, the more calomel and purgatives does the physician give to change their character, and bring them back to the standard of health. I want words to express the horrible consequences. Too often have I seen fever patients brought into the hospital with diarrhœa, hypercatharsis, and inflammation of the mucous membrane, from the use of purgatives, administered before their admission. Practitioners will not open their eyes. They give purgatives day after day—a very easy practice, and one for which there are plenty of precedents—but it is fraught with the most dangerous consequences. I will freely admit, that the disciples of the school of Broussais have gone too far in decrying the use of laxatives altogether. But if they have lost hundreds by this error, British practitioners have killed thousands by an opposite plan of treatment. In cases of fever, where there is no decided symptom of gastro-enteric disease, there can be no objection to the use of laxatives if required, but they should always be of the mildest description. You will gain nothing by violent purging in fever. Mild laxatives alone can be employed; and where there is any sign of intestinal irritation present, even these should be used with caution. There is one mode of opening the bowels which you may always have recourse to with advantage in fever, viz., the use of enemata. There is not the slightest doubt that, occasionally, accumulations of fœcal matter will take place, and tend to keep up irritation, but they should always be removed with the least risk of producing bad consequences. To purge in fever, when intestinal irritation is present, is a practice opposed alike to theory and experience, and I have already stated that its results are most horrible.

It generally happens, that in cases of severe fever, even where symptoms of gastro-enteric disease are still present, that a period will arrive when the patient's strength must be supported by nutritious diet, wine, and other direct stimulants. Here the school of Broussais fell into a great practical error; their notion being that the fever was symptomatic of the gastro-enteritis, and not the gastro-enteritis of the fever, they followed a rigorous antiphlogistic

plan of treatment *throughout the entire disease*, and thus many of their patients died, not of gastro-enteritis, but of the debility and exhaustion brought on by protracted starvation. You should bear this in mind, and recollect that, even in health, a man cannot exist beyond a certain period without some kind of solid and nutritious aliment. In Dr. Paris's work on Medical Jurisprudence, you will find an account of a nobleman, who starved himself to death, which took place about the twenty-first day. But how many fevers exceed this period? and can it be supposed that patients will survive, or be able to wrestle successfully with disease, unless their strength be supported by a certain quantity of nutritious aliment? It is decidedly a bad and erroneous practice to confine patients for weeks to the *diète absolue*; to give them, in fact, scarcely any thing but gum-water throughout the course of a long and exhausting disease. After the second week, you must look to the strength of your patient, and if you find this sinking, endeavour to support it by a mild nutritious diet; and if this will not be sufficient, you must have recourse to wine and stimulants. You may give nutriment and wine with advantage, even where you are still employing local antiphlogistic means, supporting the system generally, while you are using measures to effect the removal of local disease.

It would be impossible to lay down any rules with respect to the use of wine. Sometimes it will be required at an early, more generally at an advanced, period of the disease. You may, however, adopt this rule as applicable to the greater number of fever cases, that as long as the patient's strength and condition are such as to demand the use of antiphlogistic measures, you need not give wine. But when antiphlogistics are either no longer necessary, or when the patient's strength will hardly admit of them, when the pulse falls, and the skin loses its morbid heat, then you may give wine, and in such cases it very often produces the most remarkably favourable effects. It matters not how quick the pulse may be at this period, for one of the first effects of wine is to diminish its frequency, if it agrees with the patient. Here we arrive at a very important fact—that recovery almost invariably takes place where the pulse falls after the use of wine; and this diminution in the frequency of the pulse not only affords good grounds for making a favourable prognosis, but also shows that we may persevere in the employment of wine with decided advantage.

I feel certain that in these lectures many things have been imperfectly handled, and I know that many have been omitted altogether; but I knew this would be the case from the commencement, and that it could not be remedied. I thought, too, that it was much better to study a few subjects well than many superficially. I was convinced that the present mode of compressing into a limited course of lectures observations on all diseases, is a useless attempt, impracticable in its nature, and having a most injurious effect on medicine. This mode of teaching generates bad habits on the part of the teacher and the pupil; it makes the one satisfied with imparting, and the other satisfied with receiving,

imperfect and limited information. It is better, I repeat, to know a few things intimately than many things superficially. If you examine the history of those men who have acquired the most brilliant reputations in the world of science, you will find that it was not so much by the universality of their information, as by their profound knowledge of a few important subjects.

In the present course of lectures, I have selected for my subject three great classes of disease, namely, those of the digestive, the respiratory, and the nervous systems, and, having done this, I have proceeded to the consideration of fever. On these important affections, I have endeavoured to communicate as much knowledge as my brief and limited space would allow. I have not touched on hydrophobia, tetanus, hysteria, and some other nervous diseases of less interest; neither did my time permit me to direct your attention to the derangements of the circulating system, or cutaneous complaints. Still I do not regret this, for I feel satisfied that I have done you more justice by pursuing this plan, than if I had taken a hurried and unsatisfactory sketch of the long list of human maladies.

It has been one of my principal objects in these lectures to avoid, as much as possible, taking up your time with literary disquisitions. I was anxious that every observation of mine should bear on some point of practical importance; I wished to communicate, as far as my information went, the true principles of treatment, and I shall deem my exertions more than repaid, if I have succeeded in impressing any one useful practical precept. The length of time necessary for giving a full course on the theory and practice of medicine should be much greater than it is at present. If spared by Providence to give another course of lectures, it would be my earnest desire to adopt, if possible, a different mode of lecturing; to commence the subject of theoretical and practical medicine, and continue it through two, or, perhaps, three sessions, and to give minute and comprehensive lectures on all or the great majority of subjects which the ordinary courses on medicine certainly cannot comprise. It is only in this way that the whole of our knowledge on the present state of medical science can be made available.

PATHOLOGICAL INDEX.

- Abscess, hepatic, 128
 Anatomy, pathological, importance of, 8
 Aneurism of the hepatic artery, 140
 Apoplexie foudroyante, 257
 Apoplexy, 246
 — congestive, 248
 — nervous, 182
 — serous, 250
 — simple, 249
 Arachnitis, 236
 — cerebelli, case of, 214

 Brain, inflammation of the, 199
 — softening of the, 233

 Calculi, biliary, 113
 Cerebritis, 199
 Colic, flatulent, 96
 — painters', 177
 Colica pictorum, 177
 Convulsions, 238

 Delirium tremens, with acute gastritis, 42
 Diarrhœa, 81
 — chronic, 87
 — of phthisis, 86
 Digestive system, diseases of the, 19
 Diseases, local, remarks on, 13
 Duthinenteritis, 62, 70, 399
 Duodenitis, 59
 Dysentery, 88
 — epidemic, 88
 Dyspepsia, 45

 Encephalitis, 199
 — partial, 201
 Enteritis, 61

 Fatty matter discharged from the bowels, 119

 Fever, 323
 — continued, 359
 — infantile remittent, 71
 — intermittent, 333
 — primary, 326
 — secondary, 326
 — typhus, 360
 Flatus in the intestines, 96

 Gall-bladder, distended, 137
 Gall-stones, 113
 Gastritis, 22
 — acute, 24
 — chronic, 44, 48
 Gastro-enteritis, follicular, 62, 70, 399

 Hæmatemesis with acute gastritis, 42
 Hemisrania, 302
 Hepatic abscess, 128
 — neuralgia, 156

 Hepatitis, 123
 — acute, 123
 — chronic, 147
 Hippocratists, 11
 Hydrocephalus, 241
 Hypochondriasis, 47

 Icterus infantum, 104
 Ileitis, 61
 Infantile remittent fever, 71
 Inflammation, erysipelatous, 22
 — phlegmonous, 22
 — of the vena porta, 154
 Intermittent fever, 333
 Intestines, large, diseases of the, 81
 — small, diseases of the, 59

 Jaundice, 97
 — from gall-stone, 113
 — of infants, 104
 Jejunitis, 61

 Liver, inflammation of the, 123

 Magnetism, animal, 280
 Malaria, 344
 Medicine, mode of teaching, 15
 — objects of, 3
 — theory of, 2
 Meningitis, 199
 — of the cerebellum, case of, 214
 Meteorism, 94

 Nervous system, diseases of the, 195
 Neuralgia, 300
 — hepatic, 156
 Neuroses, 296

 Paralysis, 263
 Paralyse croissée, 263
 Paraplegia, 284
 Pathologico-anatomists, 11
 Peripneumonie des agonizans, 388
 Phrenology, remarks on, 207
 Poison, metallic, effects of, 178
 Puro-hepatitis, 126

 Ramollissement of the brain, 233

 Scrofula, 311
 Sero-hepatitis, 126
 Stomach, cancer of the, 58
 — organic disease of the, 56
 Symptoms, meaning of, 7

 Tabes mesenterica, 68
 Tympanitis, 94
 Typhus fever, 360

 Worms, intestinal, 161

