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TYPHOID FEVER.
RELAPSING FEVER.—PLAGUE.

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TYPHOID FEVER.

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The last four or five years have been by no means inferior to their

predecessors in the amount of attention and interest which this subject has demanded at the hands of professional and scientific men. In fact, investigations, observations, and theories have increased rather than diminished. As might have been anticipated, however, there is little or nothing to add to the pathology of the disease, and the work which we have to record, whether in the form of experiment, observation, or theory, falls almost entirely under the divisions of etiology and treatment.

Of work, there has been much, of advanced posts in the unexplored or debatable regions which have been taken possession of in the name of accurate knowledge and securely held against all assault, until a fair title has been established for science, we can point to very few.

There is still much discussion as to whether typhoid fever is a contagious disease, and as to whether it has at times a *de novo* origin. The question of contagion continues to be obscured by the varied uses of the word contagious. If all writers would agree as to the subject, there would be less conflicting differences of opinion. The poison of typhoid fever, it is generally agreed, is probably particulate, neither liquid nor gaseous. The classification of the disease as miasmatic-contagious is perhaps still the best possible, in so far as it expresses the more usual manner of its propagation. In formulating the statement that the disease is not contagious, it should be remembered that strict personal contact is meant, and if what we are able to affirm is to be limited to so small and unimportant a point, it might be as well to express the relation of typhoid fever to some other zymotic diseases from the stand-point of a contagium as a matter of degree rather than of kind. If we say that typhoid fever is never directly transmitted from person to person, the statement must be qualified by the admission that at times, even if rarely, the stage of development of the poison outside the human body, if such be necessary for a further reproduction of the disease, occupies a very short period, perhaps some hours only. Otherwise we must await further knowledge to explain a by no means small class of carefully observed cases. In the United States at least, there no longer exists any confusion between typhus and typhoid fevers to complicate a problem which is sufficiently difficult without it. A few points, among many, which affect the question of contagion may with propriety be touched upon.

Jacobi, contrary to the usually received opinion, maintains that typhoid fever is not rare either in infancy or childhood. *Dr. Wiltshire*, of St. Mary's Hospital, London, after reporting a case of enteric fever in an infant six months old, expresses a suspicion that this disease is not unfrequently overlooked in young children; "gastric fever," "infantile remittent fever," "low fever," "slow fever," being assigned as the cause of illness. Others also suggest that one reason why adults are at times exposed with impunity is, that having had the disease in infancy they are protected. These suggestions again remind us of that border-land of typhoid, where we find "cess-pool" fever (*Clark*), "typho-malarial" fever (*Woodward*), "slow" fever, all of which by complicating observations confuse our views, not only as to etiology, but also as to contagion.

Constant observations, moreover, only serve to confirm the tenacity of life of the typhoid poison under favorable circumstances for long periods, its activity even when extremely diluted (Caterham epidemic), and its transportability in bedding, clothes, etc. These facts must be kept in mind in forming opinions as to contagion.

Dr. *Collie*, of the Homerton Fever Hospital, a strong contagionist in the strict sense of the word, has published a number of cases occurring at Homerton among the nurses and attendants which he is convinced arose from *fresh* enteric stools. He thinks their age—so large a proportion being over thirty—has some bearing upon the small percentage of physicians and attendants in hospitals who contract this disease. The greatly diminished liability of persons over thirty years of age scarcely needs further illustration. Out of 4,928 total admissions of enteric fever to London, Stockwell, and Homerton Fever Hospitals, 3,915 were under 25 years of age, 4,352 under 30; out of 1,436 admissions to Homerton between 1871–1878, inclusive, 1,158 were under 25, 1,279 under 30 years. *v. Rothmund* reports nine cases of typhoid fever among the attendants in an army hospital within nine months, although the sanitary arrangements were especially good in his estimation.

Mr. *Murphy*, in discussing Dr. *Collie's* conclusion that the twenty cases of enteric fever, reported by him as occurring among the attendants at the Homerton Fever Hospital, had their origin in the fresh stools of typhoid patients, shows that between the years 1855–1878 inclusive, five thousand five hundred and sixty-nine enteric patients were located in the London Fever Hospital, and during that time nineteen persons engaged in the Hospital were attacked, ten of these were in no way connected with enteric fever patients or wards, and one was a laundry-maid. Estimating that about the same number of nurses are engaged in the two hospitals, he calculates that if the Homerton Hospital were to maintain the same number of attacks among attendants it has done for the past six and a half years, it would in twenty-four years have nearly sixty of its nurses attacked. He concludes that such a difference must be due, not to causes common to both hospitals, but to some difference in the structure of the hospitals themselves; and the only difference suggesting itself is the drainage. Dr. *Collie*, in his reply, reasserts his former view, and exculpates the drainage which had been thoroughly investigated. He explains the different results given by the statistics of the two institutions, by a difference in the age of the nurses; those attacked at Homerton being all under thirty, except one of that age. Unfortunately we do not know the proportion of nurses at the London Hospital under thirty years of age.

Sir *Thomas Watson* says: I hold that, like small-pox, etc., the disorder may be taken by breathing emanations from the sick person's body or bed, and that scrupulous care should be taken in all cases not to inhale the poison in that way.

The practical point which the present state of knowledge in regard to the contagion of typhoid fever should cause to be emphasized is the immediate disinfection of the alvine discharges of typhoid patients, or

even of those suspected of this disease. Without denying the elimination and absorption of the poison by other surfaces than that of the bowel, the evidence in regard to the propagation of the disease does not, to our mind, demand that isolation be practised with it as with small-pox, at least where there is no overcrowding and good ventilation can be secured. Greater precautions doubtless are necessary when dealing with this malady in small, ill-ventilated rooms with foul air, than in the wards of a well-ordered hospital, and it is very difficult to resist the evidence of contagion afforded in some cases under the former circumstances. If contagious, the poison is at least much less volatile than those of the more strictly contagious diseases.

ETIOLOGY.

The last five years have brought us apparently but little nearer a definite solution of the origin of typhoid fever, or of the process of elaboration of the poison producing the disease; but the manner of its transmission to man has been further elucidated by the continued careful investigation, especially in England, of drinking water and milk supplies, and illustrated by an extensive epidemic among the frequenters of a musical festival at Kloten (Zürich), seventy per cent of whom were attacked with a disorder which numerous autopsies showed to be typhoid fever, in so far as an autopsy performed by skilful hands is still accepted as proof of this disease. Some of the meat consumed at this festival is reported to have been putrid, and the symptoms manifested by many cases would indicate the disturbing element of septic poisoning. The chief source of the poison in this outbreak, however, was traced by so competent an observer as *Huguenin* to veal coming from a sick calf. Persons living away from Kloten, who were not present at the festival, but who purchased and eat the brain and lungs of this animal, were similarly taken ill. Confirmation of *Huguenin's* unavoidable conclusion was believed to have been found by another observer, *Walder*, in the apparent communication to their own calves by some of those infected at Kloten of a disease producing intestinal and glandular lesions, resembling those of typhoid fever in man.

Curiously enough, an outbreak of sickness occurred at Andelfingen, in this same province of Zürich, in 1839, among five hundred persons who partook of *putrid* meat. This was long cited and generally accepted as proof of an autochthonous origin of typhoid fever. The meat was simply putrid, it was both veal and pork; the animals from which it came were not known to have been ill, and it has been since shown (*Liebermeister*) that the disease was not typhoid.

The Kloten outbreak stands on a different basis, and the number and character of the physicians who attended those attacked, assisted at autopsies, or report the circumstances, lend additional value to the occurrence.

Every year our knowledge of the intimate nexus which binds together our domestic animals and man seems to become closer, and we are slowly becoming aware that the presence of such diseases in animals is fraught with evil consequences to ourselves. Of late we have begun to

suspect that typhoid fever and diphtheria may be in some cases communicated to the human subject, not merely by milk, but through the cow by some modified disease which we do not know (*Greenfield*).

In the contamination of milk supplies, whether by a legitimate use of infected water for washing dairy utensils, by its illegitimate use for dilution, by cattle themselves diseased, or bringing on their bodies poisonous particles from fields manured with the excrement of human beings suffering from typhoid fever; and again in diseased meat we have two modes of transmission which were hardly thought of until quite recently, and in contaminated water-supplies a third mode, which, though generally accepted, was not admitted by so distinguished a sanitarian as *v. Pettenkofer* four or five years ago. Other now unsuspected modes of transmission will undoubtedly be traced in the near future. In the mean-time, the source of many outbreaks is clear which would before have remained a mystery, and the temptation is diminished to resort to so unsatisfactory a theory as the pythogenic for a source, or to so unphilosophical a belief as that in spontaneous or equivocal generation to explain what we cannot understand in the transmission of this disease. Epidemic outbreaks and sporadic cases, inexplicable under our present knowledge and methods of observation, are and will be reported.

Typical of such epidemics, though longer than most in duration, was that occurring at Ascot, which prevailed intermittently during a period of four and a half years. It was worked up by *Dr. Ballard*, a competent and skilful investigator. There were two apparently distinct outbreaks, the first apparently ending in September, 1875, the other apparently commencing in July, 1876. Before the first outbreak, no similar epidemic of enteric fever had been known at Ascot for over forty years. The inquiry left no doubt as to the relations of the milk of a particular dairy-farm with the epidemic; it was found that of 68 cases of fever in 39 families, no less than 58 cases in 31 families occurred amongst persons using this milk. *Dr. Ballard* found abundant opportunities at the farm for the contamination of the milk with filth, and especially with excremental filth, but was unsuccessful in establishing its contamination with a specific contagium. Not being content with the explanation of a *de novo* origin, and having to find an intermittent cause to account for the intermittent outbreaks, *Dr. B.* suggests several ways possible or probable for introduction of the specific poison. A theory based upon the geological formation of the country, and the conveyance of the specific poison in the subsoil water as a vehicle, down about half a mile of hillside, is the one preferred by him. Bearing in mind the epidemic at Lauten (*Liebermeister*), such a hypothesis is worthy of consideration, but it is precisely in connection with a milk epidemic like this that we may hope for more light in the future. In this instance, there is no report of the condition of the cows.

As an example of the sporadic class of cases, the following reported by a medical officer of health for an English rural sanitary district will serve:

Three members of a family of four were attacked successively in as many weeks with typhoid fever. They occupied a house consisting of four fair-sized rooms. It was built on a hillside, facing the junction of four wide valleys, "a more exposed situation could hardly be imagined." Above the house were miles of moorland; both house and privy were at a distance from any highroad, and

quite out of the way of tramps. "There was an entire" absence of enteric fever from this district, a very thinly populated one; neither the father, the first victim, nor any of the others had been out of the district for a very long time, they had no visitors, the water supply was examined and found pure, the house was clean; the privy was full of filth and running over, and the fourth member of the family, a boy, who was not ill, is said not to have used this privy.

Given these data, and they are almost more than typical of those presented by the mountain farm house in Virginia, cited by the President of the American Public Health Association, of cases in New England reported by *Nathan Smith* sixty years ago, by *Dr. Stone*, of Maine, recently, and by many country practitioners who are shrewd observers, and we have, if we choose, a *de novo* generation of typhoid fever with its origin in filth. Such was the conclusion of the health officer, who was probably trained under the supervision of *John Simon*. The imagination, however, is no more exercised by the dried particle carried on the wind, and real knowledge is much furthered by a continuance of careful observations unbiased by preconceived theories until another such a step forward is taken as was made in tracing the milk epidemics, the first stage to which was the now universally accepted infection of water supplies; the Klotten epidemic again may present us with another link in this chain.

The advance in knowledge of the causes of disease by statistics is of a mathematical necessity extremely slow, as well stated by *Dr. Billings* in the introduction to *Hygiene and Public Health* of this series, and advance by the other most valued method, experiment and investigations in comparative pathology, is limited, as was there pointed out, by the fact that specific diseases seem usually specific to certain animals, and that many of the diseases of man cannot be communicated to the lower animals. The difficulties in this direction are increased in investigating typhoid fever by our continued ignorance of the specific germ or particle, the unknown factor of the problem. The last five years, however, offer encouraging advances in neighboring fields of inquiry. *Dr. Cossar Ewart*, following in the steps of *Cohn* and *Koch*, has shown by his investigations the close resemblance existing between the harmless *bacillus subtilis* and the dangerous *bacillus anthracis* of splenic fever; the existence of the *spirochæte obermeieri* in the blood of those suffering from relapsing fever has been pretty well established, the infectiveness of the blood has been proved by the inoculation of monkeys with the disease by *Carter* and *Koch*, and of men by *Motschutkoffsky*, the microphyte has been cultivated outside the human body by *Carter* and *Koch*, and the history of the organism worked out by several observers; the cause of an infectious disease of hogs, the hog plague or typhoid fever of the pig, a disease named by him "infectious pneumo-enteritis," has been announced by *Dr. Klein* to be a microbion very similar to the *bacillus subtilis*, which he has succeeded in cultivating, thus observing the various stages of its development, and in transmitting the same disease to mice and rabbits by inoculation; *Klebs* and *Crudele* believe they have found the

vegetable organism giving rise to malaria in man, and have produced, as they report, intermittent in animals by inoculation. *M. Pasteur*, pursuing the observations of Messrs. *Moritz*, *Perroncito*, and *Toussaint*, veterinary surgeons, considers it proved that a diarrhœal disease of chickens, "choléra des poules," is caused by a microscopic organism. This microbion is extraordinarily virulent, and *M. Pasteur* has not only cultivated it outside the living body, and reproduced the malady by inoculation, but shown that the disease is self-protective, and moreover that a modified microbion developed in the body of the guinea-pig occupies the same relation to this disease as the vaccine virus to small-pox.

Following in this general direction, *L. Letzerich* believes he has transmitted typhoid fever from man to rabbits by introducing by the mouth as well as by injecting subcutaneously inferior organisms (which he classifies as schistomycetes), suspended in distilled water, and obtained by repeated washings from the stools of persons affected with typhoid fever. In view of the preceding uncertain results of *Birch-Hirschfeld's*, and negative results of *Bahrdt's* experiments in this direction, these results of *Letzerich* stand greatly in need of further confirmation. According to this experimenter, the four rabbits receiving the injections died after a varying period of pyrexia, and a post-mortem examination disclosed infiltration of Peyer's patches, enlargement of the spleen, and invasion of the various organs and tissues, especially of the spleen and mucous coat of the intestines, by these schistomycetes.

Septic poisoning may possibly, in the opinion of some, account for all the changes observed; we will only venture to say that the experiments as well as the conclusions require much corroboration before we can regard typhoid fever as a "schistomycosis." They are mentioned merely as indicating a line of inquiry which has proved fruitful in connection with other diseases, and may yet disclose the unknown term in the typhoid fever problem.

In the mean time, we are doomed to constant disappointment in seeking in any one agent an exclusive or even controlling influence upon the propagation of this disease. It is always much more difficult to explain the existence and absorption of immense quantities of filth without typhoid fever, than the comparatively few cases of typhoid with filth surroundings, where the source of infection cannot be traced.

"Typhoid fever is by no means a disease of the filthiest towns, or of the filthiest parts of towns. Since I commenced investigating this question, I have been amazed at the immense amounts of urine and excrement—oxidized, incompletely oxidized, and as they come from the bladder and intestines—that are consumed in drinking water, and inhaled at the rate of 9,000 litres of contaminated air a day, and this for years, by young, old, and middle-aged persons, without any disease resulting that may be attributed to filth. I do not mean to deny the danger of filth. Especially filth decomposing without the free access of air constitutes, in my mind, one of the most important secondary factors of disease; one, too, which may become so essential a factor in some diseases as to absolutely

be the one without which, in an aggravated form, the disease cannot exist, as, for example, in cholera, plague, and yellow fever" (*Folsom*). Dr. *J. Ewart*, after giving in detail a description of the utterly indescribable filthiness of the back-slums of many parts of the European and most parts of the native quarter of Calcutta, observes: "That the difficulty does not consist in not finding plenty of animal filth to account for any amount of enteric fever, but in understanding how it is not still more prevalent than it is in such an unsavory city. The 'reason why this form of fever is not more frequently met with amid such an abundance of fæcal matter in a state of putrescent fermentation, is probably to be discovered in the fact that fortunately most of it is not pent up in ill-ventilated sewers, but exposed daily to the powerful antiseptic influences of the atmosphere and sunlight in open drains, open privies and latrines, and in comparatively open cesspools. The impression which has been gaining ground of late years that, as the sewerage of the town has been advanced, so has enteric fever been more frequently observed, lends countenance in support of this view. This augmented prevalence, however, may, in some measure, be owing simply to greater recognition of the fever, although it must not be forgotten that attention has been directed to the endemic existence of the disease during the past fifteen or twenty years."

Foreign physicians who have written upon the sewage, drainage, and sanitary condition of many different towns in China, including Peking the capital, comment without exception upon the wretched condition of drains where there are any, the offensive methods for discharging the functions of drains where there are none, and the general neglect of sanitary laws in public streets and private houses. In this connection these same authorities call attention to the remarkable infrequency of enteric fever. In Canton only two cases of typhoid were observed by one practitioner during a period of more than ten years. Two physicians report an entire absence of typhoid and typhus from Amoy, though full of typical fever-dens. In Foochow only seven or eight cases of typhoid were seen by a Scotch physician during eleven years' practice, until 1872-'73, when four cases occurred. There seems to be the same immunity also from the other exanthemata, except small-pox: intermittent and remittent fevers are common. To the objection that much typhoid probably exists in such localities which is overlooked, it is fair to reply that it evidently does not occur with the frequency or severity which might be anticipated.

It is unnecessary to pursue this point further: filth does not create typhoid, though frequently affecting its development. Something is necessary besides the decomposition of healthy or even of albuminous (*King*) excrements.

Neither is the key to the whole subject to be found in low ground-water. The labors of *v. Pettenkofer*, *Buhl*, *Wagus*, *Buchanan* show that for some places, Munich, Berlin, etc., a periodic connection, at least, exists between falling ground-water and increasing typhoid, but the con-

nection is not invariable for every year and for all places. Wet seasons, as well as dry, are followed by and coincide with abundant typhoid. The inhabitants of the ground floor and lower stories of houses, in Berlin at least, do not yield the largest ratio of typhoid cases. Of 1,087 cases of typhoid fever recently investigated in Berlin, the results of which were published by Dr. *Skrezcka*, the ratio of the sick per 1,000 inhabitants increased from 0.83 in the basement to 0.84 on the ground floor, 0.96 and 0.95 in the first and second stories, 1.1 in the third story, and 1.4 in the fourth and fifth stories. Similar statistics are given for the 939 deaths from typhoid fever reported in the year 1875, excluding those where the story of the house was not stated in the death returns; the figures are as follows: 0.89, 0.91, 0.95, 0.90, 1.03, 1.74. The calculations in both cases were based on the census of 1871; they may not be absolutely exact, but are pretty nearly so, and relatively are quite so. The highest rooms being the cheapest, are occupied by the poorest, the least cleanly, the most crowded population. Other influences besides telluric thus make their weight felt. *v. Rothmund* states that in Augsburg considerable increase in typhoid occurs with scarcely noticeable changes in the level of the subsoil water. *Thomson* declares in regard to Melbourne, Australia, and the neighboring country that, from clinical histories, combined with all available telluric and meteorologic data, *v. Pettenkofer's* subsoil theory of typhoid fever malaria is found inapplicable. Drs. *Lewis* and *Cunningham*, in a report to government on soil changes as affecting disease in Calcutta, say: the greatest prevalence of fevers during the period of observation occurred coincidentally with the period of highest water level.

Moreover, of late years the Munich observers themselves seem to have desisted, for the present at least, from explaining the connection between fluctuations in the subsoil water and the frequency of typhoid, and to confine themselves simply to the continued repetition of the fact (*Liebermeister*, 1876).

Touching the influence of the season of the year upon the frequency of typhoid, it may be remarked that, as far as observations go, the northern part of the United States, and certainly New England, offer no exception to the general rule that the frequency of typhoid increases as the summer advances; that is, typhoid increases in frequency from July to October or November, and then declines. Curves of fluctuation for Basle, London, or Berlin would probably represent roughly most of our towns. It is different where peculiar local conditions operate, as in Munich.

A statement of the deaths from typhoid occurring in Basle during fifty years gives the following for the different months:

Jan.	Feb.	March.	April.	May.	June.
192	143	137	121	160	169
July.	Aug.	Sept.	Oct.	Nov.	Dec.
186	202	237	237	236	193

(*Hagenbach*).

Relatively this might stand for Boston, New York, or Chicago.

INFLUENCE OF AGE.—The same authority gives a table of the deaths from typhoid during 50 years in Basle according to the age so far as that was noted in individual cases, to which we have added the percentages. Total number of cases 2,059.

AGE IN YEARS.	NO. OF CASES.	PER CENT OF TOTAL.	DISPOSITION IN COMPARISON WITH AVERAGE =1.	AGE IN YEARS.	NO. OF CASES.	PER CENT OF TOTAL.	DISPOSITION IN COMPARISON WITH AVERAGE =1.
0-5	108	5.24	.95	46-50	107	5.14	.94
6-10	85	4.12	.74	51-55	100	4.85	.87
11-15	106	5.14	.93	56-60	60	2.91	.53
16-20	239	11.12	2.03	61-65	53	2.57	.47
21-25	404	19.62	3.54	66-70	45	2.13	.39
26-30	290	14.08	2.54	71-75	26	1.26	.23
31-35	186	9.03	1.63	76-80	8	0.39	.07
36-40	143	6.94	1.25	81-85	2	0.09	
41-45	129	6.26	1.13	86-90	2	0.09	

SEWERS, DRAINS, WATER TRAPS AND CLOSETS.—Prof. *Frankland's* experiments with lithium in the sewers of London show that where the current is continuous and the liquid quiet, the lithium cannot be detected by spectroscopic examination in the sewer gas, but when the liquid is splashed and agitated it readily can be; for lithium substitute specific particles. It is well when possible to have sewers so arranged that they can be occasionally flushed with pure water. The importance of placing house drains in sight, of thoroughly ventilating them, of connecting with the sewer by iron pipe and hanging it to the house, are now better appreciated.

Dr. Carmichael, of Glasgow, has lately published some valuable experiments in regard to water traps and closets. He concludes that: Water traps are for the purpose for which they are employed, that is, for the exclusion from houses of injurious substances contained in the soil pipe, perfectly trustworthy. They exclude the soil pipe atmosphere to such an extent that what escapes through the water is so little in amount, and so purified by filtration, as to be perfectly harmless; and they exclude entirely all germs and particles, including, without doubt, the specific germs or contagia of disease, which, we have already seen, are, so far as known, distinctly particulate. Other sources of contamination must be looked for, except where there is some faulty construction of the trap itself, and these faults of construction are sufficiently numerous. Among other sources of foul air than faulty traps he considers none more common and pestilential than the ordinary pan water closets, with its perpetually nasty trunk. His views in regard to the pan water closet are those of *Ford*, of Philadelphia, expressed in *Buck's Hygiene*, of *Bird*, of *Latham*, and it is to be hoped of all intelligent people who unfortunately know anything about it.

CASES WHICH RUN AN IRREGULAR COURSE.—Recent years have

scarcely put us in a more favorable position for discussing this part of our subject with profit. Materials and observations have accumulated, especially in the United States, but the difficulty of differentiating a diagnosis between mild typhoid and "cess-pool" fever without an autopsy still exists, and the pathological distinctions between the lesions caused by typhoid fever and septic poisoning are not sufficiently defined to make an autopsy in all cases a positive indication. These fundamental difficulties are reflected in the views of good authorities and of practitioners of extensive experience. The utmost caution is still required in judging of such cases.

The influence of malaria in excluding or modifying typhoid is also an important factor.

Prof. *Clark*, of New York, thinks the question is a little unsettled whether the excretions from healthy persons can produce typhoid fever; his impression, however, is that, when these cases are closely examined in the new light, they will be found to be what is described as "cess-pool fever." Dr. *Loomis* ascribes the adynamia in the so-called typho-malarial fevers to pythogenic influences, which cannot, he thinks, cause enteric fever in the absence of a specific germ. Dr. *Cabell* reports that this is the view taken by many Southern physicians, with whom he has corresponded on this subject, and whose residence in malarious regions gives them opportunities for extensive observation. The majority of his correspondents express the common opinion that typhoid and malarial fevers have each its distinct specific cause, and both may prevail at the same time in the same place; a small proportion of these believe that the two diseases may co-exist in the same individual as a hybrid typho-malarial fever. This is the view advocated by Dr. *Woodward*, supported by Dr. *Hoff*, and accepted by Dr. *Cabell* himself. This last gentleman, however, acknowledges, as we believe very properly, a certain degree of antagonism between typhoid and malarial poisons, that enteric fever is far less common in malarious regions than elsewhere, and that its frequency increases in these very regions during periods when there is a suspension of malarious influence.

Dr. *Hoff*, surgeon in the U. S. Army, gives the clinical histories of five cases of a disease which he identifies with *Woodward's* typho-malarial fever, which occurred among troops under his charge in Wyoming Territory, not far from the Rocky Mountains. He considers himself forced to attribute the source of the poison to the water in a stream having its origin in the melting snow of the mountains. The cases were all of the same type; there was one death in which an autopsy revealed the characteristic lesions of typhoid fever. Dr. *Hoff* sums up his observations and conclusions in brief as follows: 1st. The fever of the Rocky Mountain region is a hybrid disease, the prominent features of which are typhoid, the modifying, intermittent. 2d. It appears during or after exposure to field service, generally, though not necessarily, in late summer and early autumn, and seems to bear no relation to typhoid infection as now usually accepted by the profession. 3d. At its incep-

tion, this disease manifests itself as an intermittent or quotidian, tertian, or other form; this stage is followed, in about two weeks, by the typhoid stage, lasting about four weeks, in which typical typhoid symptoms may be observed, modified in a greater or lesser degree by intermittent indications. 4th. The pathological anatomy of the disease is that of typhoid fever. 5th. The treatment should be antiperiodic and antipyretic.

TREATMENT.

Next in importance to the etiology, as measured by the amount of attention it has received of late years, comes the treatment of typhoid fever.

In reference to the general treatment of the disease and to especial manifestations, Sir *Wm. Jenner's* address delivered in Birmingham, before the Midland Medical Society, forms, on account of that distinguished gentleman's long experience and generally recognized authority upon this subject, a very valuable contribution. He believes that, in the present state of pathological knowledge, it is impossible to fix the treatment of typhoid fever on a more sure basis than individual opinions founded on experience. Owing to the complexity of the disease and the great individual differences in the sick, he regards it as scarcely possible to find two cases in all respects identical, and quite impossible to collect records of a sufficient number of cases practically identical to determine by numerical analysis the best mode of treatment. This leads him to advocate careful attention to the symptoms in each case as they arise, and so-called specifics either as antidotes or forms of treatment do not find favor in his eyes. The fever he would meet by rest, quiet, fresh air, mixed liquid food, and bland diluents, and the exclusion of fresh doses of poison; the intestinal lesion by the careful exclusion from the diet of all hard and irritating substances, and the removal from the bowel of any local irritant. Of pure water internally, *Jenner*, with *Liebermeister*, advises in this disease an unlimited supply, and this advice is worth taking to heart.

DIET.—In common with others, *Jenner* admits the value of milk as an article of diet in fever, but cautions against its indiscriminate employment in almost unlimited quantities, reminding us that a pint of milk contains as much solid matter as a full-sized mutton-chop. Undigested and irritating curds, and very abundant fecal accumulations are the frequent accompaniments of a simple milk diet as carried to an extreme of late years not unusual. When catarrhal inflammation of the intestines is present, milk is the best diet; if the curd appears in the stools, the milk should be diluted with water or lime-water; when the bowels are torpid, beef or mutton broth may be given alternately with the milk, though neither of these is anything like as nutritious as milk (*Pepper*).

Opium, *Jenner* is convinced, is on the whole a most dangerous remedy. In the early stages of fever, it disturbs digestion and checks secretion; in the later stages of the disease, its influence on brain, heart, and secret-

ing organs is sometimes fatal. With hemorrhage its use in enemas may be, and with perforation its use in some way would be unavoidable.

Calomel.—Dr. *Alonzo Clark* says we have been very familiar with the use of *calomel* in the treatment of typhoid fever in the United States, but that it has been pretty generally abandoned. Nor would a more general reliance be placed, as far as we can learn, upon any specific action to be expected from iodine either in altering the character or shortening the duration of the disease, whether administered as *Lugol's solution* or as iodide of potassium.

Alcohol.—When, forty years ago, Dr. *Todd* introduced to the profession the alcoholic treatment of fevers, he was as much alone as are those who now propose to do away with it entirely. Since that day the spectacle has been presented of the constant and indiscriminate use of alcohol in all forms of disease. To such an extent has this been carried that a large body of distinguished practitioners in England have felt called upon to issue a circular denouncing this indiscriminate use, and demanding a much greater restriction in its application (*Caswell*). The tendency at the present time in the United States and in Great Britain is undoubtedly a reaction against the excessive use, and toward a more rational use of alcohol in fever, and especially in typhoid. Stimulants are only demanded for the relief of certain symptoms (*Pepper*). Alcohol, by its influence on the nervous system, is of the greatest value in the treatment of typhoid fever, but it should only be given for the purpose of attaining a definite object; its effects should be watched that the desired result may be obtained from as small a dose as possible. It calls forth strength, but does not create it, and is therefore more frequently indicated and in larger doses in the later stages.

Tremor out of all proportion to other signs of nervous prostration is evidence of deep destruction of the intestine. A small, deep slough will be accompanied by great tremor; a large extent of superficial ulceration may be unattended by symptoms. Now it is deep ulcers following separation of deep sloughs which are especially liable to give rise to severe hemorrhage and perforation. In these cases of tremor, alcohol should always be given to increase nerve energy and to limit sloughing and ulceration. Delirium is, as a rule, one of the symptoms which are influenced for good by alcohol, but in which its effects should be most closely watched. When the urine contains a considerable quantity of albumen, alcohol should not be prescribed, unless for the relief of some symptom immediately threatening life, and then with the greatest caution. It is in exceptional cases only that more than twelve ounces of brandy in the twenty-four hours can be taken without inducing some of the worst symptoms of prostration. Nearly all the good effects of brandy, when its use is indicated, are obtained by four, six, or eight ounces in twenty-four hours. When in doubt of the wisdom of giving alcohol in a case of typhoid fever I do not give it, and when there is a question of a larger or smaller dose I prescribe, as a rule, the smaller (*Jenner*). The use of stimulants is regulated by the state of the pulse; if the beats are growing

in rapidity and losing in strength, if they pass 110, 115, 120, a half-ounce of spirits or a glass of champagne is given; if there be from any cause doubt of the need of alcohol in a rising pulse the dose is given with the finger on the wrist; if the pulse steadies or slows, the wine is repeated on its rising. Twelve ounces of spirits daily is said to be all that the worst case needs, but this limit is constantly and necessarily exceeded with favorable results (*Stedman*).

ANTIPYRETIC TREATMENT.—The combat with pyrexia has gone on briskly, and quinine, salicylic acid, salicylate of soda, digitalis, alcohol, and cold baths continue to be experimented with as antipyretics, whether separately in larger or smaller doses, or in varied combinations. There is nowhere great unanimity of opinion as to the positive value of these agents, or the best methods of administration, but the evil effects of excessive or prolonged elevation of temperature are generally conceded, as well as the fact that, in some uncomplicated cases, an otherwise inevitably fatal result may be averted by the timely employment of one or more of these antipyretics.

In general, it may be said that, in Germany, where professional attention was recalled to the importance of controlling febrile action in typhoid, the antipyretics still retain the confidence originally expressed in them, though in a modified degree; in France, they have gained a place, though their use there has certainly never excited much enthusiasm; perhaps the German indorsement proved unfavorable to them; whilst in the United States and Great Britain, where the experiments with cold baths at least have been much less extensive than in Germany, there seems little disposition to adopt cold baths as a constant form of treatment, though their value is recognized in certain cases and at certain periods of this disease. Many good authorities in those countries doubt the wisdom of the employment of large doses of quinine and salicylic acid, whilst others seem more than satisfied with the results obtained from these drugs. The truth is, that some patients bear best one form of antipyretic and others another, and, in a certain proportion of cases, all fail, either partially or wholly, to effect the object aimed at. If, in any given case of typhoid fever, the pyrexia gives cause for anxiety, and fifteen or thirty-grain doses of quinine are well borne, without giving rise to symptoms of nervous or gastric irritation, the administration of these full doses, especially at evening, will often control the fever. Salicylic acid is now less used than salicylate of soda; salicin is thought to produce a more prolonged, though slower effect upon the temperature than quinine; and in combination with this last the others are considered to increase its efficiency and to diminish the liability to relapses, which most authorities are now agreed is greater under antipyretic treatment. When any or all of these drugs are ill borne or insufficient, resort must be had to the application of cold—whether in the form of cold bath, gradually reduced bath, sponging, or sprinkling on a sheet—and that either alone or in conjunction with the above drugs. There is no doubt that, when quinine is well borne, its combined use diminishes the number of baths required.

Each case must be its own rule for the dose of the drug, for the form of application of cold, and if baths are given, for the temperature and duration of the bath. Mild cases do not require baths, and, in advanced cases (the third week) they are not safe. Great care should undoubtedly be exercised in administering them to young children and old persons.

Liebermeister, in Germany, and *Dumontpallier*, in France, now employ an apparatus for applying cold. There is a refrigerating envelope through which the water runs, and by means of stop-cocks and thermometers the temperature of the body can be lowered continuously or intermittently, and the change effected readily noted.

In a late reply to a vigorous attack by Prof. *Gairdner*, of Glasgow, upon the German antipyretic treatment, *Liebermeister* says: The antipyretic treatment is not, as you believe, a matter of routine. On the contrary, with it the cases are more individualized than with any other method of treatment. Each agent is used only when necessary, and when none is necessary none is used. But, in order to know what is necessary, one must, of course, observe each individual patient much more thoroughly than has hitherto been customary. It is just the routine hitherto pursued for which we propose to substitute a method of close observation, very exacting on the medical attendant, which offers the greatest obstacle to the introduction of the antipyretic treatment.

Dr. *Austin Flint* says that his observations as regards the antipyretic treatment are sufficient to lead him to believe the statement by *Liebermeister* that, under this treatment, "the old picture of a typhoid fever patient is no longer to be seen," to have a solid foundation in clinical experience, although somewhat extravagant, and to concur with this writer in saying that thereby "typhoid fever has lost a great part of its terrors."

Drs. *Edes* and *Stedman* report the use of graduated baths, combined with internal antipyretic treatment, in a considerable number of cases, at the Boston City Hospital, with favorable results.

Out of 32 cases entering, during the first week of fever, in Dr. *Edes'* service, in the course of three years, 1872-1874, there was only one death. A series of 46 cases entering this hospital at an earlier period, likewise during the first week, but not treated antipyretically, showed a mortality of a little over 10 per cent.

The statistics of the results of treatment and percentage of mortality at the Bellevue Hospital, New York, since the introduction of antipyretics, up to the year 1879, are not of a character to be of much practical value, but, as far as they go, are not especially favorable. Graduated baths have been tried there a number of times, and the opinion is expressed that they are uniformly annoying and depressing to the patients. At the Massachusetts General Hospital, antipyretics, and certainly cold baths, have not been systematically tested, but the usual rate of mortality at that institution, about thirteen per cent, is not an unfavorable one. Dr. *Smythe*, of Indiana, from an experience of eighteen cases, considers cold baths and quinine as indispensable in typhoid, but the latter as the more valuable of the two.

Dr. *Nathan Smith* published a short essay on Typhoid Fever in 1824, in which there would be little to change, even at this date; in it he gives the results of his experience of this disease in New England, and shows that he was in the habit, since the latter part of the last century, of using cold water, when indicated, externally as well as internally, with freedom and courage, and that he recognized its value in certain cases. Sir *Wm. Jenner* says that neither his own limited experience nor the evidence adduced by others in its favor has carried conviction to his mind of the advantage of the treatment by cold baths. At the same time, he recognizes the fact that the direct cooling of the body is, in some cases, essential to the preservation of the life of the patient, and mentions with favor the graduated bath, tepid sponging, the wet pack, and cold to the head by the india-rubber tubing cap. He expresses himself as having been disappointed in the effects of quinia and salicylate of soda as reducers of temperature, while he has occasionally seen both do much harm by disturbing the stomach and interfering with digestion. Dr. *Pepper* expresses himself even more strongly, and says that he never administers the enormous doses of quinia given by German physicians; that there is so much unnecessary irritation of the mucous membrane produced that heroic treatment, such as this, should only be adopted as a last resource; that he is in the habit of giving about twelve grains in the twenty-four hours.

The graduated bath was adopted at St. Thomas' Hospital, London, and Dr. *Ord* says, in regard to its use there, which was quite general, that the practice, though not uniformly successful, has not, in any instance, done any known harm, and has, in all probability, averted death in several cases.

In regard to the graduated bath, Dr. *Greig Smith*, of the Bristol Royal Infirmary, is confident (though he thinks the contrary is very generally believed) that the endeavor to prepare the system for a temperature of 60° by lowering it from one of 90° in half an hour or so causes more depression than to plunge the patient at once into the requisite cold. It certainly causes more discomfort. In this opinion Dr. *Collie*, of the Homerton Fever Hospital, where baths have been successfully used, coincides. Moreover, he says they have never given more than three baths in twenty-four hours, and should not, in any case, give more than four; that the duration of the bath should not, as a rule, exceed ten minutes, and, in children of ten, not more than seven minutes; that, in most cases, the bath should be discontinued toward the end of the second week, and, if an antipyretic is needed, quinine should then be given. To young children and elderly persons quinine should be given instead of the cold bath.

Mr. *Murphy*, Resident Medical Officer of the London Fever Hospital, writing in 1877, stated that up to that time the cases of typhoid treated antipyretically in that institution had been too few to urge as statistical proof of the efficacy of that treatment, but the results on individual cases had been sufficiently satisfactory to lead to its general adoption in this

hospital. He further says: It is not contended that it will insure the recovery of every patient suffering from enteric fever; that it will save life in those patients who die in the second week of their illness from the direct influence of the poison; nor that, when commenced late in the third week, it will undo the injury that has already occurred. It is not believed that it will shorten the period of illness, for it has even appeared to prolong it; although this may be due to the fact that the more severe cases have been bathed—cases which would under any circumstances have run a long course. But there is a large class of cases which, under the expectant treatment, die at the end of three or four weeks, worn out by the continued pyrexia, and these can without doubt be saved by an early and systematic antipyretic treatment. Without the bath, quinine will only control the temperature in the milder cases. Quinine will never altogether replace the bath, for it is not followed by the favorable effects of the latter on delirium and in producing sleep, but in conjunction with the bath it is a most useful aid; sponging or wrapping in sheets saturated with cold water are useful in mild cases, but cannot be relied on in severe ones. The difficulties of the treatment are inconsiderable, it is not attended with any danger, the patients rarely object to it, and by some it is liked. The reduction of delirium, the quiet sleep, the general feeling of comfort it gives the patient, are sufficient to convince the most sceptical that the cold bath is one of the most useful therapeutic agents we have in the treatment of enteric fever.

The following from the *Revue des Sciences Médicales*, 1878, reflects very fairly the French estimate of baths up to that period. To our knowledge, there is not a single hospital service in France where baths are used as a general method of treatment. M. *Féréol*, one of the physicians who recently has resorted the most to baths as a method of treatment, cited a series of forty-three cases, of whom only two were bathed. M. *Bernheim*, at Nancy, says that often out of ten or twelve cases of typhoid only one will require baths. M. *Raynaud* regards baths as an energetic form of treatment, with which one should not be extravagant, but reserve it for cases where the situation of the patient is sufficiently grave to authorize the physician to combat it.

One must, moreover, distinguish the conditions in which baths should be ordered regularly and continuously, the only true refrigerant method, and those cases where occasionally in the course of the disease recourse is had in a temporary manner to some one of the processes for applying cold.

Physicians at Paris who have had recourse to the new method of baths agree that for grave cases the advantage is on the side of cold baths (*Féréol*); some are inclined to think that a number of recoveries would not have been procured by any other method, and think they cannot with certainty attribute any death to the treatment. It does not seem probable, however, that cold baths will be accepted in France in the immediate future as a general method of treatment in typhoid fever, but the indications for their use are constantly better defined, and the

different processes of hydrotherapy are gradually more and more employed, whether as a regular and constant application in certain severe cases, or in a casual way, and as an exceptional measure to combat temporary complications. They are not a specific, but a very energetic agent destined to bestow great benefits in proportion as we understand better the conditions in which they are to be resorted to (*Homolle*).

M. Peter, in an article which is robbed of much of its value by a too evident anti-German feeling, attempts to show that the treatment by baths is less a process of cooling than one of revulsion; that its good effects do not arise from the lowering of the temperature, but from a profound perturbation of the nervous system; in short, that the lower temperature is but a very indirect result of the treatment. He complains that in the struggle of the "hypo" with the "hyper" of "cold" with "warmth," the patient has disappeared altogether. That there are no longer therapeutics or treatment of symptoms, but a mere arithmetical problem, a subtraction. He thinks, apparently with *Jenner*, that the best therapeutic system, especially in the treatment of typhoid fever, is, and always will be, to have none.

Raynaud's experience with the treatment by cold baths in the Lariboisière Hospital was rather favorable than otherwise. In the years 1874-1876, after excluding the mild cases, there were 117 typhoid patients with 17 deaths, 2 of which occurred immediately after entrance; this gives a mortality of 14 per cent. During the year 1873, out of 30 cases under expectant treatment, there were 8 deaths, 26.7 per cent. He attaches more importance to prolonged pyrexia than to mere elevation; after the twelfth day of the disease he expects but little benefit from baths, and never applies them after the eighteenth day. Given a temperature of 104°, lasting five to six days with only slight remissions, he would regard the cold bath as a necessity. A bronchitis would not offer an impediment to baths, pneumonia and cardiac weakness with small pulse are decided counter-indications.

M. Foltz found physiologically that cold clysters have a local action on the intestine, and a general action shown by a decided slowing of the pulse and fall of temperature after a lavement of half a litre of water at 50° Fahr. This coincides with *Dr. Rutenberg's* experiments. In febrile conditions analogous effects are produced; moreover, nervous manifestations are soothed, thirst is appeased, the appetite is stimulated, the secretions augmented. These effects are the more manifest and persistent the lower the temperature of the water, the greater the quantity injected, and the more frequent the injections. *M. Foltz* orders, as a rule, lavements of about a pint at 50° to 55° Fahr., repeated every two, three, or four hours, or at longer intervals during sleep or when the temperature falls. In his cases, the number of lavements varies from thirty to three hundred for each case.

Twenty-seven typhoid patients were treated, with one death, fifteen of these had a severe form of typhoid. This mode of treatment has objec-

tions which readily present themselves, and is not likely to become general, but may be occasionally of service.

Prof. *Immermann* has published some interesting remarks on the prevention of relapses. Between 1872 and 1877, *Immermann* treated about 1,200 cases of typhoid fever in the hospital at Basle; 15.6 per cent of these suffered relapses. The proportion of relapses varied in different years from 12 per cent to 19 per cent. Having in vain guarded his patients from all error in diet, and kept them in bed until the end of the second week of continued absence of fever, he was unable to doubt that the relapses were independent of external influences, and in the great majority of cases arose from the liberation of unconsumed portions of the original poison. He attributes the increase in the proportion of relapses, from 8 per cent to 10 per cent ten or fifteen years since, to the present percentage, indirectly and directly to the new antipyretic treatment. Indirectly, on account of the diminished mortality; directly, on account of the use of hydrotherapy and antipyretic medication which interfere with the natural destruction of the poison in the course of the disease. To combat this result, he had recourse to a secondary disinfectant treatment. To twenty-two convalescents he administered, from the first day with absence of fever, daily doses of from sixty to ninety grains of salicylate of soda, and continued it during ten or twelve days. Of these twenty-two, only one had a relapse, and in this case there was a grave imprudence in diet. Among 93 patients not submitted to this supplementary course there were 27 relapses. A second series of experiments gave 1 relapse out of 29 convalescents of both sexes treated with salicylate of soda, and in this case the exhibition of the medicine was begun by mistake only on the fourth apyretic day; 67 other cases of typhoid offered 15 relapses. To sum up, *Immermann* experimented during two successive years, 1877, 1878, with salicylate of soda upon 51 convalescents from typhoid, among whom the relapses did not exceed 4 per cent, whilst amongst 160 others not so treated, the proportion of relapses amounted to 23.6 per cent.

The statistics of Dr. *Goltdammer* in regard to the treatment by cold baths, in the Bethanien Hospital in Berlin, are the most extensive which have appeared of late years. They cover a period of nine years, from 1868 to 1876 inclusive, during which 2,068 cases in all were treated, with a mortality of 13.2 per cent. They deal with a homogeneous material, subjected to similar general influences, are full, carefully compiled, not made to support a pet theory, and their value is enhanced from their being comparable, to a certain extent, with a series of 2,228 cases during the years 1848 to 1867 inclusive, drawn from the same population, subjected to similar general surroundings, and treated in the same hospital on the expectant plan. In making up the report, the former different use of the terms "typhus" and "febris gastrica" has been taken into account, and the rate of mortality in the early series thus rendered more, rather than less favorable. Moreover, no proof exists that the type of typhoid fever has changed in Berlin of late years. From the

early series of 2,228 cases during the years 1848 to 1867, there were 405 deaths: 18.1 per cent. From the second series, 2,086 cases during the years 1868 to 1876, there were 267 deaths: 13.2 per cent. A diminution in mortality of 5 per cent in favor of the period of cold baths is shown, that is to say, 100 deaths were avoided during the period of cold-water treatment.

The average length of time in the hospital was :

1858-1867 incl. (for 1,086 cases).....	46.1 days.
1868-1876 incl. (for 1,519 cases).....	39.8 days.

Difference, 6.3 days.

There was thus a difference of 5 per cent in mortality, and of 6.3 days of treatment in favor of the period of cold baths. In the preceding series, all cases, the light, the severe, and the hopeless are included. In order to arrive as nearly as possible at an approximative mortality during the period of hydrotherapy, Dr. *Goldammer* deducts from the second series sixty-four deaths, occurring when the patient was brought in too late to use baths, or in a condition already hopeless from existing complications, and thus obtains a mortality of 10.5 per cent for the cases subjected to cold baths. The method he designates as a tolerably severe, but not excessive one. The baths were given at a temperature ranging from 68° to 82° Fahr., generally about 75°, with a duration of from ten to fifteen minutes. Sensitive patients, or those with cardiac weakness, were given baths of 88° Fahr., lowered in the course of fifteen or twenty minutes to 78°.

Temperatures were taken in the axilla every three, and in severe cases every two hours, and whenever 104° was reached a bath was given. Of the patients bathed regularly, the majority had three or four baths daily, the more severe cases five or six, and a very few seven. Baths were omitted in the night. All cases were subjected to treatment in which no counter-indications existed, or in which the course of the disease was too light to justify treatment. As counter-indications are mentioned especially cardiac weakness, also stenosis of the larynx, lobular pneumonia, pleurisy, intestinal hemorrhage, peritonitis. Salicylic acid and quinine were occasionally given in the evening in cases of persistent high temperature. It was not found that the baths were agreeable to the patients, but neither was there much resistance, and they were certainly quieter, slept better, and were less troubled with bed-sores. One case of syncope in the bath occurred, with subsequent fatal result. The records of the last three years, '74, '75, '76, being more full, present some interesting tables which are appended. An increase in the proportion of hemorrhages from the bowel was not observed.

The statistics of Dr. *Goldammer* give no positive information as to the relative frequency of relapse under the expectant and the cold-water treatment, as there were no exact data on this point in the early series of cases; his opinion, however, coincides with that of other observers, being that the proportion of relapses is increased by the baths. By excluding

indiscretions in diet, and fresh infection from without, he concludes that in the great majority of cases the relapse had its origin, whatever might have been the exciting cause, in the same infection which caused the first attack. This view is also somewhat strengthened by the fact that most frequently the mild, short, abortive forms of typhoid, less often the moderately severe, and rarely the severe cases were those followed by relapse.

One case passed through a pretty severe typhoid in this hospital, returned ten weeks later with another severe attack, which was followed by a relapse. This individual, therefore, experienced three clearly marked attacks of typhoid within six months.

The apyretic interval preceding the relapses varied from 3 to 21 days, and averaged 8 to 9 days. Among complications and causes of death for these 783 cases were perforation 13 times; 51 times hemorrhage from the bowels = 6.5% with 21 deaths; 9 times diphtheria with 8 deaths and 7 unsuccessful operations; and 13 times pleurisy with 8 deaths.

MORTALITY ACCORDING TO AGE.

AGE.	NUMBER.	DEATHS.	PER CENT.
4-15 years	51	7	13.7
15-20 "	224	27	12.
21-25 "	274	42	15.3
26-30 "	121	21	17.3
31-40 "	73	18	24.6
41 years and over.....	32	15	47.
	775	130	16.7

MORTALITY ACCORDING TO PERIOD OF ADMISSION.

PERIOD OF ADMISSION.	NUMBER.	DEATHS.	PER CENT.
Unknown.....	33
1 week (1-4 days).....	113	9	7.9
1 week (5-7 days).....	215	23	10.7
2 weeks.....	335	67	20.
3 weeks and later.....	87	31	35.6
	783	130	16.6

The duration of fever in those who recovered before and after entrance into the hospital was:

For men	21.9 days.
For women.....	24 days.
	22.9 days.

RELAPSING FEVER.

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This disease is not indigenous in the United States. Cases were reported by *Clymer* in 1844, and by *Flint* in 1850-51. The malady was confined to recent Irish immigrants and did not spread from them. It was reimported again in 1869, and prevailed to some extent in that and the following years in New York and Philadelphia, where its diffusion was, according to *Flint*, undoubtedly due to a contagium. Since its disappearance in 1870 nothing more has been seen of it in this country. We have been unable to learn of any epidemics of infectious disease answering to the description of bilious typhoid or relapsing fever as having prevailed among the negroes of Philadelphia and North Carolina, and, therefore, cannot share the opinion expressed by Prof. *Hirsch*, that this disease was the cause of a peculiar mortality among those people.

The old term, famine fever, which is now discarded, indicates, not that the symptoms included under the name are caused by want, but that want, overcrowding, and their concomitants favor the development of the disease. The name, however, serves to remind us that the present distress in Ireland, the home of relapsing fever, is very liable to be followed by a general outbreak of this disease at various points, and increasing emigration would in all probability bring it again to our shores.

In 1873 *Obermeier* described a spirobacterium found by him in the blood of those attacked with relapsing fever. This was identified as the spirillum, or more properly the spirochæte of *Ehrenberg*. These two microbions form respectively the fifth and sixth group in *Cohn's* classification of schizomycetes or schizophytes, and compose Tribe IV., that of spirobacteria. As both names have been applied to the microphyte of relapsing fever, they will be used interchangeably in the present article, though spirochæte is the more correct designation. Since this discovery by *Obermeier*, much attention has been given to the etiology and pathology of relapsing fever, materials have been accumulated, and the relations of the microbion to the disease carefully investigated. It cannot be affirmed that positive results have as yet been reached, and many of the observations of those who have had opportunities to make such are very conflicting. This, however, is to be expected in the present transitional state of knowledge upon this and kindred subjects, where the best authorities are working in fields still new, and all are not equally experienced in methods of research.

Some investigators, as *Heidenreich*, *Vandyke Carter*, *Motschutkoffsky*, *Holsti*, *Enke*, believe they have found this parasite invariably, or almost so, at some stage in every case when the blood was examined microscopically; others, as *Riess* and *Lewis*, report a number of cases in which spirilla were not found during any stage of the disease, and again instances in which they were not discoverable during one or more periods of pyrexia. Such conflicting observations naturally make it desirable to suspend judgment as to the precise relation of the microphyte spirochæte to the disease relapsing fever. Whether it is simply attendant upon, or how far it is causative of the disease, we are not quite yet in a position to decide. It is a little premature to shout, "No relapsing fever without spirochætes, and no spirochætes without relapsing fever" (*Unterberger*). Our present knowledge would not justify us in declaring any given case as not one of relapsing fever because spirochætes are not found during the pyretic stage. When spirochætes are found in the blood, however, it is pretty safe to suppose that one has to do with this disease until it is shown, and that has not yet been done, that this parasite exists in the blood under other conditions. It is also necessary to remember that, as other hitherto unobserved agents may perhaps influence the febrile paroxysms, so other conditions of the blood besides temperature, such as amount of water, oxygen, etc., may affect the regular development of the microzyme spirochæte. The spiral bacteria found in various secretions and in water (*Billroth*, *Cohn*, *Ehrenberg*) are similar to our

spirochæte, but careful inspection shows some differences, the spirochæte plicatilis found by *Ehrenberg* in water being somewhat larger, and, what is more reliable, inoculation does not produce specific results.

Both men and monkeys have been successfully inoculated with the spirillar blood of relapsing fever (*Motschutkoffsky*, *Carter*, *Koch*), and this inoculation carried to the second and third stages. Blood in which spirilla could not be found was successful in the hands of one experimenter, but it was taken in the earliest stage of a pyretic onset which soon developed the organism. Other non-spirillar blood did not produce specific results. There is no absolute proof that in these cases it was the spirochætes and not some other accompaniment of the injected blood which transmitted the disease. *Koch* announces that he has found this microphyte in the tissues of an inoculated monkey killed at the height of a relapse, something not previously observed and which we have not seen confirmed, and also that he has successfully cultivated it outside the living body, following the same methods which he pursued with bacillus anthracis of charbon; this had not been done previously except by *Carter*.

The further step, adopted in charbon and which forms the missing link, of reproducing the specific disease from the spores of the microphyte and from the organisms obtained from these, as far as we can learn, has not yet been taken. If *Cohn's* view is correct, that only bacilli form spores, the terms, though not the method, of the step would be altered. Even the successful attainment of this step would not be proof that no other agent caused relapsing fever. There are, however, limits to a rational and healthy skepticism. One thing appears from recent observations, that the spirochæte does not precede the pyretic attacks with sufficient constancy to be relied upon as a warning. To this short summary of the relations of this organism to the disease under consideration we add a brief synopsis of the more important recent investigations bearing on the subject.

All recent writers agree that one attack of relapsing fever does not protect the individual from a second entirely new one. *Litten* states that five fresh reinfections took place among the hospital patients at Breslau during the epidemic of 1872-73, whilst seventeen other patients who had already passed through the disease in 1868 were again attacked in 1872-73.

Dr. J. Motschutkoffsky, of Odessa, had a very favorable opportunity to study relapsing fever upon a rich material in the hospitals of that city between the years 1873-76; and he had the unusual advantage of being permitted to inoculate a number of human beings with the disease. During two years he made over two thousand preparations of the blood. In regard to the spirochæte, his observations led him to the following opinions and results: We do not yet know the origin nor the process of destruction of the spirochæte. It was never found in any of the secretions or excretions, and is not eliminated unchanged from the body. He reports having twice witnessed the process of its dissolution in blood into a finely granular detritus. *Litten* gives twenty-four hours, *Münch*

over twenty-four hours as the length of time it retains its power of motion outside the body. *Heidenreich* saw it move one hundred and thirty days after being taken out of the body and sealed up in a glass tube, and thinks this organism preserves its activity best in a temperature of 58 to 70° Fahr. Our author saw it move thirty-seven days after being subjected to similar conditions. He was unsuccessful in cultivating it, as were also *Weigert*, *Lebert*, and *Heidenreich*, but *Koch* and *Carter* have since succeeded. The addition of equal parts of water to the blood is fatal to the spirochæte, but it also loses its activity more and more the thicker the blood becomes. Its activity is not affected by any internal exhibition of quinine, salicylate of soda, or other agents, and externally only affected by about 1% of quinine. It withstands a low temperature well, but begins to cease moving at anything below 32° Fahr.; it revives, however, in blood which is warmed after having been cooled to as low a point as 15° Fahr.

In regard to the relations of the microbion to the disease, this investigator thinks it impossible to deny the dependence in some way of the process of relapse upon the spirochæte; for he affirms that the appearance of a paroxysm and of spirochæte are nearly synchronous, that the termination of the paroxysm and the disappearance of the parasite are nearly synchronous, that inoculation with apyretic blood is unsuccessful, that occasionally the spirochæte may not be found in the first days of pyrexia, but that they must be present, as this blood is equally effective. During an attack spirochætes are a constant element in blood; during apyrexia, are found exceptionally.

He does not recall a single attack during his investigations, extending over three years, in which spirochætes were not to be found in the blood of the living. This agrees with *Heidenreich*. This latter alone on one occasion discovered this parasite in the blood of a person dying from relapsing fever, and then they were without motion. *Cohn*, *Ehrenberg*, and others have found organisms very similar to spirochæte in the secretions of the mouth, in water, in cysts, etc.; and *Cohn* is disposed to regard these different microphytes as representing merely so many different stages of the same. This is far from being proved. *M.* did not see spirochætes a single time, either during incubation or apyrexia, and noticed no difference in their size at different periods; in number they increase from a few hours after the beginning of the pyrexia up to about a day before the crisis.

He cannot accept *Heidenreich's* theory that the destruction of the microphyte is dependent on the pyrexia, as it does not agree with the facts, but thinks it more probably depends upon the degree of consistency of the blood. Though we cannot stop the disease by any remedies, we may, perhaps, be able to shorten the attacks by withdrawing liquid from the blood.

"Bilious Typhoid" of *Griesinger*, of which *Lebert* makes a separate division, and is rather disposed to regard as a distinct though similar disease in *v. Ziemssen's* 2d edition, 1876, *Motschutkoffsky* regards as

unquestionably nothing more than relapsing fever with an hepatic complication, probably a catarrhal affection of the fine bile ducts. He bases this view upon the following observations: In sixteen cases of undoubted so-called bilious typhoid occurring in Odessa, he found the spirochæte in the blood in all, and the organism taken from the blood of the last eleven cases was subjected to the same tests as those taken from the blood of relapsing fever patients, with the same results. Secondly, an individual inoculated with the blood of a "bilious typhoid" patient developed, after the usual interval, the ordinary relapsing fever uncomplicated by icterus. Thirdly, a patient who was exposed to relapsing fever during convalescence from typhus, developed "bilious typhoid" with spirochætes in the blood; at this time there was no case of "bilious typhoid" in that wing of the hospital, nor had there been, as far as could be learnt by careful investigation, for more than two months; moreover, infection from without was rendered improbable by the length of time the patient had been in the hospital. *Holsti's* report of the epidemic at Helsingfors in 1876-77, in which the mortality was large, in which many cases exhibited marked icterus with other symptoms of bilious typhoid, though still running the usual course of relapsing fever, and showing spirochætes in the blood, is confirmatory of the Odessa observations, and of the identity of the two diseases. *Münch's* experience with autopsies upon those dying from "bilious typhoid" led him to call attention to the coincidence that the liver in many of these cases shows signs of previous pathological processes (cirrhosis, syphilis, etc.).

In the late Berlin epidemic (1879) *Riess* observed one case of "bilious typhoid" with plentiful spirochætes in the blood.

Motschutkoffsky sums up his experiments with the inoculation of spirillar blood briefly as follows: He did not succeed in inoculating animals, although monkeys were tried among others. Only blood was available. Milk, sweat, urine, saliva, and excrement were without effect. Blood was only effective during pyrexia; apyretic blood gave negative results. Blood taken during pyrexia, *e. g.*, the first hour of a commencing attack, was good, whether spirilla could be seen under the microscope or not. Artificial *recurrens* differs in no respect from the natural.

He did not find that reinoculation increased the virulence of the disease. Blood from *recurrens biliosus* produced simply *recurrens*. The period of incubation was from five to eight days. The period of apyrexia about equalled that of incubation.

In 1877, Dr. *H. Vandyke Carter* had an opportunity of observing a large number of cases of fevers in camp and in hospitals in Bombay. Between April and December, about three hundred and fifty cases presented themselves offering all the characteristics of the relapsing fever of Europe; in all these cases, spirilla were found in the blood, and *Carter* has applied the name "spirillum fever" to the disease. The questions raised by *Carter* of the influence of a malarial taint upon this disease are of interest, and will doubtless receive more attention. He regards want, crowding, and contagion as the immediate causes of spirillum fever. He

examined the spirilla carefully, and his results correspond with those obtained in Odessa. He was unable to find spirilla except in the blood; he doubts the identity of similar filaments found in the saliva of fever patients, for they were present during the apyretic interval, and the same were found in his own saliva during health. He observed that the parasite has a natural tendency to seek a liquid medium, and he concludes that the relationship obtaining between the abundance of the parasite and the intensity of the fever is a contingent one, though commonly at particular times the connection between pyrexia and the parasite is close. He did not succeed in cultivating the spirillum outside the human body. Within the last year, 1879, and since the above investigations, Dr. *Carter* has succeeded in communicating "spirillum fever" to monkeys by inoculation with spirillar blood. Forty-four trials were made upon fifty-one of the common small Indian monkeys (*Lemnopithacus Entellus* and *Macacus radiatus*); defibrinated blood was injected subcutaneously. Of thirty-one inoculations made with spirillar blood, twenty-two were successful, and nine failed; all nine inoculations of non-spirillar blood and seven of desiccated blood (mostly infective when fresh) gave negative results; four injections made with saliva of fever patients and a healthy man produced serious, but not specific results. *Carter* found that intensity of infection increases by repetition, and, as in man, the blood at invasion-attacks is not so active as that of relapses, so the monkey's blood, at second remove from man, was found to produce a fatal result in the two cases in which it was used. *Motschulkoﬀsky's* experiments upon men led him to a somewhat different conclusion. *Carter's* investigations will be published more fully, but the main conclusions arrived at are stated as follows: (1), that the spirillum or relapsing fever of man is directly transmissible to a quadrumanous animal; (2), that there occurs a non-febrile infection of the blood prior to "fever;" (3), that, though the blood spirillum was never seen in the monkey without fever ensuing sooner or later, yet the pyrexia is secondary in time, and is susceptible of highly varied manifestations. *Carter* has now succeeded in cultivating the spirochæte outside the body.

Dr. *Koch*, of Wollstein, at whose instigation we believe *Carter* made his experiments, has likewise succeeded in cultivating spirochætes, pursuing the same methods adopted by him with the bacillus anthracis of malignant pustule, and has also succeeded in inoculating monkeys with relapsing fever. One of these was killed during the active period of the disease, and the spirochætes were found in the various organs and in the skin. This result has never been reached with the human body, and should be reaffirmed.

Dr. *Lewis*, of Calcutta, who studied "spirillum fever" in India simultaneously with Dr. *Carter*, in his lately-published results, does not agree with the latter either in his observations or his conclusions. He regards the spirillum merely as a coincidence, and as no more essential to relapsing fever than is famine.

Prof. *F. Cohn*, of Breslau, however, finds serious defects in *Lewis's*

methods of microscopical research, which he thinks quite invalidate the conclusions arrived at, and he is unwilling to regard *Lewis* as an authority. The observations which are published which indicate a positive connection between the parasite and the disease are certainly far more convincing than any negative results we have seen.

Holsti says relapsing fever was first recognized in Finland, in 1865, and was probably introduced from Russia. In 1866 it was accompanied by typhus. In 1867 it began to diminish in Russia, but this was a disastrous year for Finland, on account of a total failure of crops. With famine associated itself an outbreak of relapsing and typhus fevers, of such extent and severity that in 1868 about a third of the population was attacked by one or other of these maladies. In 1869-70 relapsing fever still existed in Helsingfors, but was milder and more rare, after the latter year disappearing altogether. At the beginning of August, 1876, it appeared in Helsingfors afresh at two quite distinct points simultaneously. It was impossible to trace its introduction from without. The epidemic developed rapidly, reached its culminating point in November and December, and diminished slowly, so that the latter half of 1877 presented only a few sporadic cases. There were about 600 cases during this epidemic, and of these *Holsti* saw 437. The disease confined itself almost exclusively to the poorest classes, occupying dirty crowded dwellings. One collection of habitations occupied by the most wretched of the urban population furnished 146 cases of the disease out of 560 inhabitants. The general rate of mortality was large, 7.8%. In 20% of the total there was but one febrile crisis, about the same proportion had two or three crises, occasionally there were four, five, and even six separate attacks; the first attack lasted as an average 6 days; the second, 3 to 6, and the third only 3 days. The average duration of the first intermission was 7.2 days, that of the second 8.4.

The blood was examined for spirilla in forty cases, and the organism was always found. They appeared generally one or two days after the commencement of the fever, and disappeared shortly before its termination. They could not be found during the intermission. This writer states that they were most abundant during the first access of fever. He could not establish any correlation between the intensity of the attack and the abundance of the spirilla in the blood; nor could they be traced in other secretions of the body, as the saliva, sweat, or urine. He reports as little success in cutting short the attacks with salicylate of soda as do other experimenters.

Dr. *Enke's* report of 136 cases which presented themselves in the Hospital at Magdeburg offers testimony of a positive character. He says spirilla were found in every case without exception, in some cases, indeed, only after persistent and repeated examinations. In a few instances where relapsing fever was suspected, it was only after as many as eight preparations of the blood were made that spirilla were found. *Enke's* conclusion is, that spirilla are to be regarded, not simply as an accompaniment, but as a cause of this disease.

Dr. *Riess* took advantage of a mild epidemic of relapsing fever, which developed itself in Berlin early in 1879 and dragged on through the year, to investigate with all possible care the connection between spirochæte and the changes in temperature. His results are based upon 77 cases of undoubted relapsing fever, and emphasize somewhat the difficult nature of this inquiry, and the caution necessary in drawing conclusions. *Riess* regards his cases as typical relapsing fever, but acknowledges that in a considerable number there was a certain irregularity and indistinctness about the relapses. This may explain his results where they vary from those of others. He observed a striking want of congruity between the appearance of spirilla and elevations of temperature.

The spirilla were seldom constant during the height of the relapse, one day spirilla were found and another day none. The length of time elapsing from the beginning of a relapse to the appearance of spirilla varied much; they appeared on an average from a half to one day later than the first rise in temperature; their behavior toward the close of the relapse was equally varied, occasionally not disappearing until some hours after the crisis. Not unfrequently no spirilla at all could be found during an entire pyretic period; out of 96 pyretic periods they were absent 36 times. Again in a few cases no spirilla could be found during any stage of the entire course of the disease.

THE PLAGUE.

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The continued outbreak of epidemics of plague of greater or less magnitude at various points, some of them widely separated, since the one of 1871 in Persian Kurdistan, the last-mentioned in the American translation of *v. Ziemssen's Cyclopædia*, have impressed still more strongly upon the medical world the fact that the plague is still an active disease, especially in the Ottoman and Persian dominions; and the actual appearance of the disease in an epidemic form in south-eastern Russia has shown that the warning given by certain distinguished epidemiologists of a possible danger therefrom, under certain circumstances, to Europe itself was not uncalled for. In 1875, and again in 1876, Mr. *Netten Radcliffe*, in papers read before the "Society of Medical Officers of Health" in England, discussed the prospect of reappearance of plague in that country and in Europe. In 1876, Prof. *A. Hirsch* brought the same subject to the notice of the profession in Germany.

We give a brief summary of the outbreaks of plague succeeding that in Persian Kurdistan in 1871, for which we are indebted largely to these two writers. The plague appeared in Kurdistan in the winter of 1870-71, in a mountainous part of a district which is said to have been free from the disease for forty years; it continued throughout the greater part of 1871, and out of 7,000 people it was found to have affected at least 1,120, of whom 891 or 79.5% died. The next outbreak of plague occurred two years later in the winter of 1873-74, when the disease reappeared in Mesopotamia, on the lower Euphrates in the same district attacked in 1867, but on the opposite side of the river. This outbreak was more severe than the preceding one; the disease prevailed actively during the first six months of 1874, and it is estimated to have caused 4,000 deaths among a population of 80,000, appearing afresh in the

winter of 1874-75, and lasting until the summer of that year. In all these countries, owing to the tendency to concealment to avoid isolation and quarantine, the number of cases and of deaths is more apt to be under- than overestimated. In March of 1874, plague broke out among the highlands of the Assyr district of Arabia, extending to within a short distance of Mecca. In April of this year it again declared itself among the nomadic tribes of the hilly district of Bengali in Tripoli. In 1876 we find plague again devastating the Euphrates valley, during which year it is estimated that not less than 20,000 persons were killed by it in this district. It ravaged Bagdad during this and the following year, carrying off more than 5,000 victims. Already in 1876 it had reached south-west Persia, later in this year appearing in north-western India, and about the same time in northern Persia, in the Province of Azerbaijan and in Astrabad, from which it extended in 1871 to the Province of Ghilan on the Caspian Sea, attacking especially the principal town Resht, which has an active commerce with Astrachan, at the mouth of the Volga; it had been utterly desolated by plague in 1830, having then a population of about 40,000. Before the close of the year 1877 the plague is estimated to have killed about 4,000 people in this town. Cases of the disease are said to have occurred during this year at several places on the Caspian littoral of Caucasia, and it is also said that a number of cases were reported to the medical department of the Russian government as occurring in south-western and north-eastern Russia; but in regard to this the information is not very definite. Quarantine against importation of plague from Persia was not established on the Russian coast until April, 1877. From July to September, 1877, about 150 cases were observed in the town of Astrachan and the surrounding places of a disease which showed a resemblance to a mild form of plague, or was actually considered as such. According to *Hirsch*, the disease manifested itself chiefly by swelling of the lymphatic glands, the tumors being seated for the most part in the submaxillary, and also in the axillary and inguinal glands. A febrile state of variable duration, lasting even fourteen days, preceded the swellings in some cases. These swellings varied much in size; often they resolved, sometimes they suppurated; but with the exception of only one case, where the tumor became septic, and death followed by pyæmia, all these cases terminated favorably. Only a few patients were confined to bed. In no instance whatever were persons coming in contact with these patients infected, a fact which was verified especially in the hospitals; the garrison of the town also escaped entirely. These cases were seen by Dr. *Döppner*, the principal medical officer of the Cossack forces in the Province of Astrachan, and the description quoted corresponds with that given by him of the early cases in the subsequent epidemic at Wetljanka, with that given by Dr. *Cabiadis* of the cases of glandular swellings free from fever which preceded the severe outbreaks of plague at Bagdad and Hillah in 1876-77, and also with the symptoms and course of a few sporadic cases observed in several parts of Russia in 1879, but especially by Prof. *Botkin* in St. Petersburg. These

cases in St. Petersburg gave rise to much discussion and difference of opinion, but Prof. *Botkin* stoutly maintained that they were the result of a mild form of the plague poison, and in this view was supported by others.

This mild type of plague—if such it was—which showed itself in the town of Astrachan in the summer of 1877, was followed in October of 1878 by the appearance of a similar malady at Wetljanka, a small stanitza or Cossack village, situated on the right bank of the river Volga, about one hundred and fifty miles from Astrachan, the inhabitants of which are for the most part devoted to fishing. This mild form rapidly developed into a disease of a malignant and fatal character, exhibiting all the recognized features of the oriental bubonic plague. From its comparative proximity to the termination of an important line of railway, this outbreak attracted the attention and excited the interest, not only of Europe and England, but even of this country, and we shall, therefore, give a somewhat more detailed account of it.

Whether the poison causing this outbreak had an autochthonous origin or was imported, and if imported, how, must remain undecided. The data on which to form an opinion are derived from the reports of Dr. *Döppner*, the surgeon-general of the district, and from those of the commissioners of the different European Governments appointed to serve as an International Commission for the investigation of the plague in Russia. It is not easy to reconcile the sometimes conflicting statements of these authorities; Prof. *Hirsch* and Dr. *Döppner*, for example, differing as to the distance of Wetljanka from Astrachan by fifty-six versts or about thirty-seven miles. Dr. *Döppner* labored under the disadvantage of occupying an official position and having to justify a previous course of inaction; the commissioners, under that of arriving on the scene independently and after the termination of the epidemic. The commissioners, moreover, had to carry on their inquiries by means of interpreters. Dr. *Colvill*, of the British Commission, was disposed to adopt the theory of an "independent origin" of the epidemic—and his previous experience of the disease in the Euphrates Valley lends weight to his opinion—autochthonous in the sense of indigenous, not in that of *de novo*, as explained by his fellow-commissioner Dr. *Payne*.

The most probable origin under the circumstances would seem to have been in importation, either from Astrachan or from some of the fortified towns, as Erzeroum, Kars, Bayazid, in eastern Asiatic Turkey which were besieged and sacked by the Cossack troops. The belief of the great majority of the Commissioners, including Prof. *Hirsch*, the head of the German Commission—a belief shared by the parish priest at Wetljanka, by the local authorities, and by Count *Melikoff*, sent as Governor-General of the Province—was that the poison was imported in packages of goods which reached Erzeroum or Kars from places in Persia or Mesopotamia, already infected, remained unpacked in the bazaars, and falling into the hands of Cossacks were sent by them as booty to Wetljanka.

Prof. *Hirsch* mentions a very clear case of infection with plague by

means of clothing brought in a box from a house where people had died of the disease to another house at a distance, which fell under his own observation.

As far as its situation and sanitary conditions are concerned, Wetljanka seems in no respect to favor the prevalence of zymotic diseases any more than any of the other eighteen stanitzas of this district. The last time plague visited this village was in the year 1807-08; it was then supposed to have been brought by pilgrims from Mecca; it spread along the Volga as far as Sarotov; there were but few cases, and the mortality was small, not above one hundred for the whole district. The following account of the epidemic of 1878-79 is in the main that of Prof. *Hirsch*. It began in October, spread from Wetljanka as a centre, to six or eight villages on both sides of the Volga, and was practically at an end by the first of February.

The progress of its development in Wetljanka may be observed from the table of mortality which is accurate up to the middle of December, at which time the parish priest, who kept the records, succumbed to the disease:

		Deaths.			Deaths.
From Oct. 1 to Oct. 7.....		1	From Nov. 26 to Dec. 2.....		7
“ “ 8 “ 14.....		0	“ Dec. 3 “ 9.....		56
“ “ 15 “ 21.....		2	“ “ 9 “ 16.....		169
“ “ 22 “ 28.....		3	“ “ 17 “ 23.....		54
“ “ 29 to Nov. 4.....		0	“ “ 23 “ 30.....		33
“ Nov. 5 “ 11.....		1	“ “ 31 to Jan. 6.....		19
“ “ 12 “ 18.....		7	“ Jan. 7 “ 14.....		12
“ “ 19 “ 25.....		8			

On the whole there were 373 deaths at Wetljanka during the whole period. By subtracting the average number of deaths that occur (according to an average of several years) from October till January in that place, viz., fourteen, there remained 359 deaths from plague; in other words, in a population of about 1,750 individuals, 20 per cent of the whole perished with the disease. It is very difficult to arrive at the number of cases that recovered. According to very barely trustworthy statements, this was said to have been 81. If those are added to the 359 deaths, we have a morbidity of 440—that is, 25.3 per cent cases of illness amongst the inhabitants, with 82 per cent deaths of the total number attacked.

After Wetljanka, Prischib was first attacked with the disease. Here the number of deaths was 16, which occurred in five houses. Afterwards there was a small outbreak of the disease in Staritzkeje, where 7 individuals succumbed in one house. On the left bank two places had only a few cases, namely, Michailowka, where in one house 4 cases occurred with 3 deaths, and Udatschucie, where in one house 2 individuals died of plague. Selitroucie suffered the most severely; here in four houses 32 individuals succumbed to the disease. It was said that in the steppe two plague-cadavers had been found, and that opposite to Wetljanka, in an island in the Volga, several mortal cases of plague had occurred amongst the inhabitants of Wetljanka who had fled there. It is impossible to

make out whether those statements were well founded or not, but it might be not far from the truth if the total number of deaths from plague in the district were estimated at 450.

About Dec. 18th a cordon was drawn around Wetljanka and toward the beginning of January a general cordon of five thousand soldiers around the whole infected district, with quarantine stations at the north-western and south-eastern extremities on both sides of the river. The quarantine arrangements are reported to have been very imperfect, and the speedy termination of the epidemic is not attributed especially to these dispositions.

SYMPTOMATOLOGY.

The various descriptions of plague which have been given by those having personal experience of it during its greater frequency within the last five years agree in most particulars with the account given by *Liebermeister* in the first edition of *v. Ziemssen's Cyclopædia*. It is greatly to be regretted that the Russian Government did not promptly avail itself of the favorable opportunities afforded by the epidemic at Resht and in the provinces of Astrachan to have the symptomatology and pathology of the disease carefully investigated by competent persons, whose services it could easily have commanded. No post-mortems were made during the Wetljanka epidemic. The following analytical statement of 1826 cases of plague noted by Dr. *Giovanni Cabiadis*, at Hillah, during the epidemic of 1876, is taken from an account of the characters of epidemic plague in Mesopotamia in 1876-77, prepared by Dr. *Dickson*, Physician to the British Embassy at Constantinople, from Dr. *Cabiadis'* notes. Dr. *Cabiadis* occupied an official position under the Persian Government and had every opportunity for observation.

As a rule, an attack of plague lasts from a few hours to four weeks, and Dr. *Cabiadis* considers that one-third of the attacks end fatally. From the accompanying table, however, 1,826 cases of plague seen and registered by him at Hillah, in 1876, the proportion of deaths to that of the attacks is 52.6 per cent. But a great many cases of plague, he observes, are never made known, and would thus diminish very considerably this percentage of its mortality; whilst the deaths, on the other hand, must all be reported in order to obtain the permit of interment.

ANALYTICAL STATEMENT OF 1,826 CASES OF PLAGUE NOTED BY DR. CABIADIS, AT HILLAH, DURING THE EPIDEMIC OF 1876.

From 2 months to 9 years,		277
“ 10 years “ 19 “		617
“ 20 “ “ 29 “		432
“ 30 “ “ 39 “		292
“ 40 “ “ 49 “		123
“ 50 “ “ 59 “		52
“ 60 “ “ 69 “		18
“ 70 “ “ 79 “		11
“ 80 “ “ 89 “		3
An old man of 113		1
Total,		1,826

Sex: Male, 889; Female, 937; total, 1,826.

Result: Recovered, 865; died, 961; total, 1,826.

Manifestations:—

Glandular swellings—in the groin,	710
“ “ axilla,	466
“ “ neck,	98
“ “ several places,	122
	1,396
Carbuncles,	36
Dependent on the nervous centres	{ Coma, 28
	{ Convulsive shake, 9
	{ Petechiæ, 120
	{ Epistaxis, 2
“ “ circulating system	{ Hæmoptysis, 6
	{ Hæmatemesis, 27
	{ Sanguineous diarrhoea, 14
	{ Menorrhagia, 2
“ “ assimilative organs	{ Bilious vomiting, 32
	{ Bilious diarrhoea, 16
	{ Jaundice, 2

Dr. *Cabiadis* does not attempt to deny the contagious properties of plague, but the experience acquired by him during the outbreaks at Hillah (the ancient Babylon) and Bagdad has convinced him that no great risk is incurred in touching a person affected with the plague, provided exposure for any lengthened period to the atmosphere of the apartment occupied by him is avoided. He says that those persons who lived in the same house with a plague-smitten patient, but who avoided touching him or his clothes, through fear of catching the infection, generally did get the plague; whilst those who lived in houses exempt from the malady, but who visited plague-patients and handled them freely, without remaining long in the same room with them, hardly ever caught the disease. *Colvill's* description of the symptoms of plague, as observed by him on the lower Euphrates in 1875, agrees with that of Dr. *Cabiadis* in the main. Both of these observers mention the vomiting and spitting of blood, and hemorrhages from various surfaces as not unusual. This was doubtless the same disease which invaded *Wetljanka*, and in this respect these accounts agree with those of *Döppner*, an eye-witness, and of *Colvill* of the epidemic at that town in 1878-79. *Sommerbrodt*, *Hirsch's* colleague on the German commission, says: It cannot be strictly denied that at a certain period of the epidemic pulmonary symptoms were prominent. Prof. *Hirsch's* account of the symptoms in the *Wetljanka* epidemic differs somewhat from those mentioned, and we quote what he says:

I do not intend to discuss at present the question whether, as some maintain, the epidemic plague known by the name of the “black death” and the so-called “Indian plague” are identical with the Levantine bubonic plague; or whether they differ from each other, as I have assumed and still assume, by definite symptoms which reveal an affection of the lungs. Perhaps at another time I may re-

turn to that question; to-day I have only to say that the lung affection named had not the character of pneumonia, but of pneumorrhagia; and that in the course of the disease in the Astrachan epidemic no such complication either with pneumonia or with bleeding from the lungs could be observed. In a few cases only slight bronchitis seems to have been observed as a complication. Moreover, I will not deny that cases of pneumonia occurred at the time of the epidemic; only I do not believe that they had anything in common with the plague. As in all former plague epidemics, there are three forms to be distinguished, according to the degree of development of the disease. The first form is slight, partly without fever, a sort of ambulatory plague; where without any other grave symptoms, a bubo was formed in the axillary, inguinal, or submaxillary glands, which soon suppurated or resolved, and where the whole process was terminated in that way. Cases of this form seem to have been more frequent, especially in the beginning and towards the end of the epidemic. Of those we saw ourselves a few, as I have just said. But, secondly, there was a series of cases of a graver character, where buboes were formed after an illness of several days, and where the whole organism was deeply affected. Most of the buboes in those cases became purulent, and recovery was only exceptional. A third series of cases was observed in the form of the so-called *pestis siderans*, where no localization of the morbid process in the lymphatic system was formed at all, or the buboes were so small that they escaped the attention of the surgeons and the friends of the patients. Those cases proved fatal without exception, in two or three days.

Under the protection of Prof. *Virchow*, it may not be rash to express a regret that *Hirsch* should have introduced an apparently unnecessary element of confusion into the study of the disease. Plague is probably plague just as typhoid fever is typhoid fever all the world over. It is very evident from the accounts of the various epidemic outbreaks that soil and circumstances modify the plague poison and its manifestations as they do those of typhoid fever, so that some symptoms, and notably buboes and hemorrhages, vary in frequency and severity in different epidemics and at different periods of the same.

A frequent affection of the lungs hardly requires a different name, as "Indian plague," any more than we should now speak of "black death," because the hemorrhages in the skin were unusually large or confluent. The erection of the Pali plague, and the endemic disorder observed in a small district in the Himalayas near Nepaul, into a distinct disease under the name of Indian plague savors somewhat of the "*Schreibtisch*."

According to *Radcliffe*, who follows *Planck*, *Máhámari* (the "great plague," a term also applied to cholera) and locally designated *gola* or *phutkia* (bubo)—the several terms being generally used in the sense of pestilence—seems to have been first noted in Cutch, in 1815, and during the six years 1815–1820 the disease prevailed in parts of Cutch, Kattywar, and Sindh. In 1823 the malady is reported to have appeared among the Himalayas, in the district of Garhwál, a district lying to the north of and contiguous to Kumaun. In 1834–5 it was prevalent in parts of Garhwál. In 1836 the disease broke out at Pali, in Marwar, Rajputana (whence the name Pali plague), and it subsequently prevailed in several parts of the Meywar and Marwar districts. From 1847 to 1853 *Máhámari* seems to have been present in a more or less active form in, or at least rarely absent from Kumaun, and in the last-named year the malady was

also observed in Rhileund, the district lying immediately to the south of Kumaun. In 1859-60 the disease was again active in both Kumaun and Garhwál; also in 1870. From 1870 to the outbreak of 1876-7, in Kumaun, there does not appear to have been any record of an appearance of the disease. In 1876-77 forty-one of the hill villages in the mountainous district of Kumaun, on the southern slopes of the Himalayas, suffered from this malady; in forty of these villages two hundred and ninety-one persons were attacked, of whom two hundred and seventy-seven died. Mr. *C. Planck*, the Sanitary Commissioner for the North-Western provinces and Oudh, inspected the infected district at the time of the prevalence of this outbreak, and has given a detailed account of the results of his inspection. He regards this disease as the same as the Pali plague at Rajputana and contiguous provinces to the westwards—so likewise does *Hirsch*—and, moreover, identifies it with oriental or Levantine bubonic plague. *Planck* says: The symptoms and character of this disease coincide very exactly with the description of plague, with the exception that the appearance of petechiæ (purple spots) on the skin has not been noticed in the Kumaun form of the disease. No appearance of that nature could be seen on the skin of the sick persons examined, and no evidence of such appearance in any case could be obtained. He attributes this absence of visible petechiæ to the fact that the people examined were all dark-skinned. For a more detailed description the reader is referred to p. 48 of "Papers Relating to the Modern History and Recent Progress of Levantine Plague, London, 1879."

The most palpable and evident of all the causes which predispose an individual to an attack of plague during an epidemic Dr. *Cabiadis* considers to be poverty with its accompaniments. In his experience he found that the poor were seldom spared; the wealthy hardly ever attacked; in this respect presenting a contrast to cholera.

TREATMENT.

Recent epidemics have added nothing whatever to our knowledge upon this point; the treatment is purely expectant.

Prophylaxis.—According to *Cabiadis*, the most effectual means for the protection of a community against the propagation of plague are the isolation of the sick, the destruction by fire of their clothes, and the whitewashing with lime and free ventilating of the domiciles in which cases of plague occur. The plague reappeared in only one out of three hundred and fifty houses which had been *whitewashed* after an outbreak of plague in them, whilst its reappearance in houses which merely had been abandoned for a time, after an attack of that disease, but which had not been whitewashed, *was* of frequent occurrence. The other disinfectants tried, such as sulphur fumigations, lighting fires, and throwing a solution of sulphate of iron into the drains, gave no decided results, except when associated with the whitewashing and airing of the infected houses.

v. Pettenkofer recommended sulphuric acid as a disinfectant, as he found it the most reliable means of destroying the bacteria of charbon. *Virchow* thinks dry heat would prove more reliable in disinfecting effects.

All recent writers agree with *Aubert* (*De la Peste ou Typhus d'Orient*, Paris, 1840) that civilization alone has destroyed plague in Europe, and it alone will annihilate the disease in the East.

QUARANTINE AND CORDONS SANITAIRES.—The late epidemic in the province of Astrachan is but little instructive in regard to the value of these measures, for the epidemic was already at an end when these steps were taken. *Hirsch* concludes his remarks upon the Astrachan outbreak as follows:

The principal task of a public sanitary administration must lie, according to my conviction, in a strict supervision of the places which are menaced by the plague, and in a well arranged isolation of the first centres of the disease. If we should have learnt nothing more from that small plague-epidemic than to estimate sufficiently the value of such isolation of infected localities for avoiding further propagation, the victims have not fallen in vain.

The value of quarantine must vary with the conditions under which it is applied. For countries like England and the United States, quarantine against plague would be unnecessary if not impracticable. There are other countries where quarantine might be desirable if a strict one were possible, which the exigencies and temptations of modern commerce render very doubtful.

The rag-trade would be the most probable means by which plague might be introduced into the United States when prevalent in the Levant. New Haven is said to be the only port in this country that receives whole cargoes of rags, and these come for the most part from Egypt. After the outbreak at Wetljanka, measures were taken by the National Board of Health for the disinfection of cargoes from infected ports. The Connecticut State Board of Health, in its second annual report, states that reports from the paper manufactories in the State, where the bales of rags are unpacked, fail to show the communication of any other disease than small-pox by rags, and that by domestic rags in a few instances.

A proper distinction is to be made between medical inspection, disinfection, and cleansing as practised to-day and in this country, and the exercise of measures hitherto implied by the term quarantine, in regard to which we may quote the language of Mr. *John Simon* in his eighth annual report: A quarantine, which is ineffective, is a mere irrational derangement of commerce, and a quarantine, of a kind which insures success, is more easily imagined than realized. Only in proportion as a community lives apart from the great highways and emporia of commerce, or is ready and able to treat its commerce as a subordinate political interest, only in such proportion can quarantine be made effectual for protecting it. In proportion as these circumstances are reversed, it becomes impossible to reduce to practice the paper-plausibilities of quarantine. The conditions which have to be fulfilled are conditions of national seclusion, and the fulfilment of such conditions by England (or the United States) would involve fundamental changes in the most established habits of the country.

