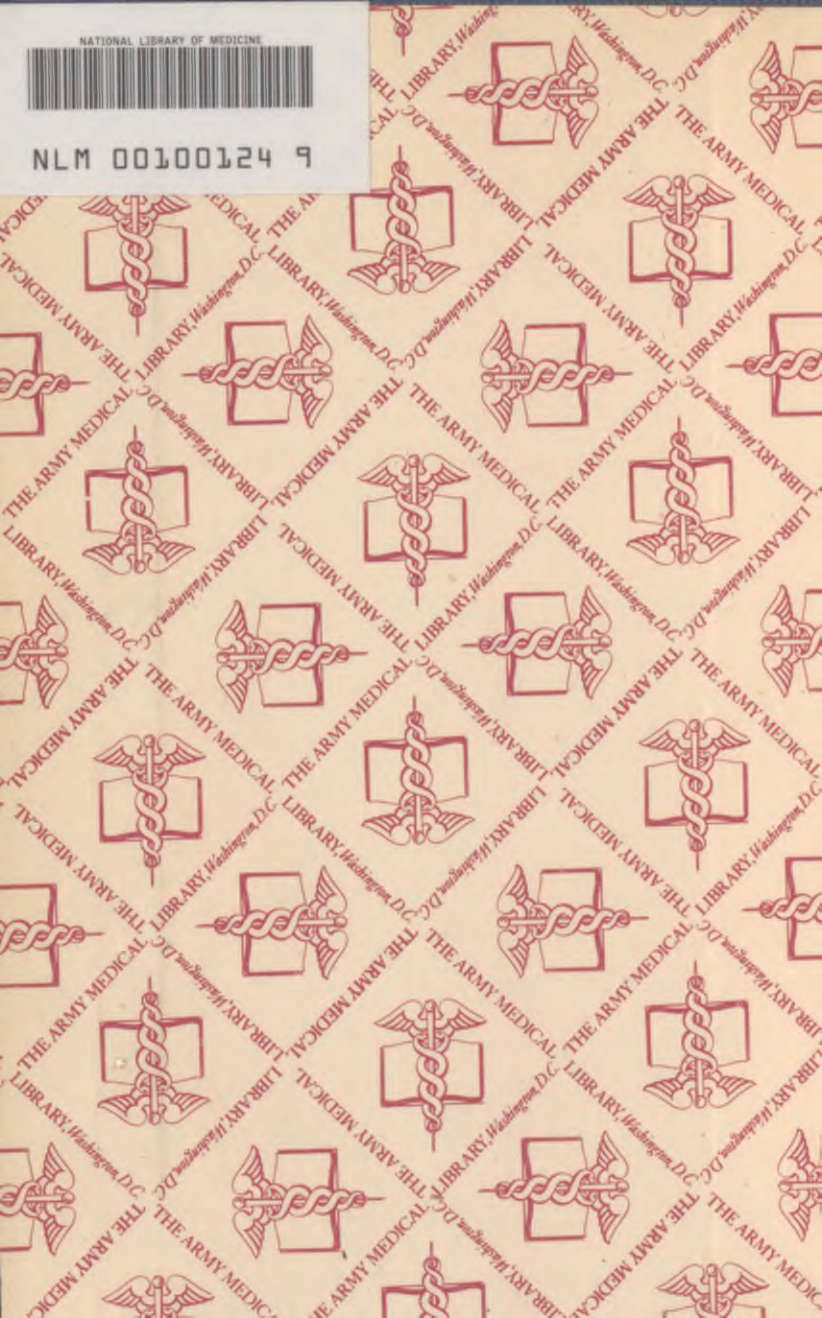

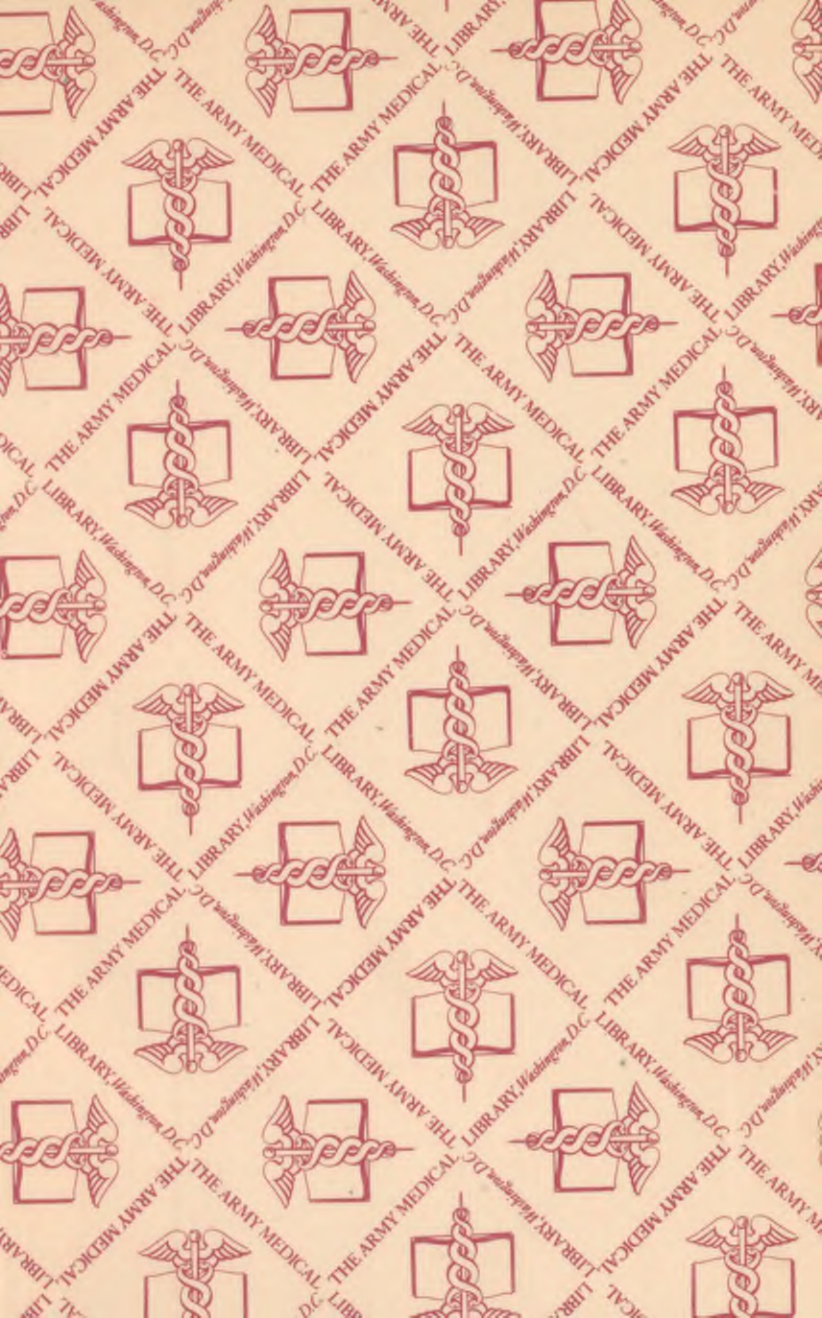
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H. J. Parker

A RIP VAN WINKLE

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IN THE

Practice of Medicine

The Efficacy of Venesection, Cold Water, Starvation and Some Other of the Older Means of Cure in Certain Diseases.

A return swing of the pendulum from the modern plans of exclusively drug treatment, from forced or over-feeding and from "expectancy" (trusting to nature).

CLAYTON, ILLINOIS:
1899.

NOTICE

Whatever the presentation of so small a volume, as a medical treatise, may suggest, the author feels that it will be found sufficiently voluminous, considering the subjects treated of.

For the work considers but three diseases, and is confined principally to their treatment.

Medical books usually abound in the history, etiology, diagnosis and prognosis of diseases, but are short on treatment, while in this volume the rule is reversed.

The author believes that a knowledge of the treatment of disease is the main consideration and the element of real weakness with the practitioner; and here the average volume is characterized by a striking indefiniteness and uncertainty.

Indeed, the average volume on "Practice," while well suited to the needs of the student and as a text book, is of little help to the practitioner so far as the treatment of disease is concerned. It is well worth consulting, of course, but it is not sufficiently clear and voluminous on treatment.

The average volume is again deficient, as it appears to the author, in what may be designated as the philosophy of medicine, or the reason for treatment.

It will tell us that this, that and the other remedy is and has been used, but it says less about the reasons why—the rationale.

The present volume, it is thought (and whether the author's position is regarded as true or erroneous), will be found sufficiently voluminous, and more so than

the average work, on the philosophic aspect of the subjects discussed.

And, therefore, whatever of merit attaches to the author's effort on general grounds, he feels that he will at any rate be credited for giving, at sufficient length, the reasons for the faith within him.

Because more convenient, the author asks the privilege to use the pronoun *I*, interchangable with "the author" and "the writer."

THE AUTHOR.

CHAPTER I.

Prefatory and General Introductory Statement.

"Let me wipe off this honorable dew
That silverly doth progress on thy cheeks."—*Shak.*]

I have chosen as a title for this little book "A Rip Van Winkle in the Practice of Medicine," the reason for which is that many years ago I learned of certain methods of treatment for some diseases herein to be considered, and, after following to some extent and experimenting with other, and what has been called "improved," plans for a time, have been forced back to an "early love," as it were. Not only have I been forced back, by experience and observation, to the adoption of some ancient methods and means of cure, but I am also persuaded to believe that I now stand on grounds that will bear the test of rational and scientific criticism.

As it appears to me, indeed, I have, in a sense, aroused from a slumber of medical insanity into which the profession have been plunged by a too radical spirit of progress which has characterized modern medical thought.

As I view it, the desire to improve and to move forward has led men to cut loose from the past and to lay aside old methods, more because old than because irrational, and thus to make departures that lay claim to merit only because new.

Medicine is what is called an inexact science, by which is meant that it is an ever varying or a progressive or developing science. This is not so much true

with relation to the practice of surgery and obstetrics as in the treatment of general diseases.

As to whether, in this state of inexactness, the profession are advancing or retrograding is a question to be answered by saying that there is general advance, but possibly retrogression in some particulars, and temporarily.

That something approaching chaos reigns, however, in the professional mind is seen in the fact that no two physicians treat the same disease nor the same kind of a case alike. This is more so now under modern rationalism than it was formerly under empiricism; and because, under empiricism (which means according to authority), the physician followed more after rules laid down by others, while under rationalism (which means reasoning out your case), the physician adopts his own methods and is free to do so.

Thus the practice of medicine, or the science of medicine (more correctly, the art of therapeutics), is less exact under rationalism than under empiricism.

Yet most all physicians are empirics—that is, they follow some authority; and when you find a man who ignores all authority, and who is so extreme a rationalist that he consults only himself, you will find very generally the most irrational physician.

Established principles do not change so rapidly, indeed, that one generation can lay aside all that has been learned in the past generations.

A man should be very slow to condemn a method of treating disease simply because it is gray with years.

The fogy is not the one who adheres to old methods, but it is he who follows more in his own routine.

A man's methods may be all new and yet he may be deeper in the rut than the conservative who considers

both the old and the new ; and the greatest of all fogys and cranks is the routinist.

Men speak very glibly about being behind, or up with, the times, but I don't know about this.

As in politics, in religion, and in many other things, perhaps the profession of medicine is getting too fast, and to be too fast is sometimes equal to retrogression. It is a very good idea to go slow, sometimes, and I think I shall be able to point out herein that an occasional halt should be called, in order to survey the ground over which we have passed, as well as to guard carefully our future steps.

I have come to believe, in the first place, that the profession rely too much on drug treatment, or internal medication, and not enough on other means, herein to be considered, and in this very departure will be seen the possibility of exactness; for, when drug treatment is adopted and relied on, an infinite variety of drugs are used, no two cases are treated alike, new remedies are being offered continually, each doctor settles upon the use of his special favorite remedies, and, while each is in a rut of his own creation, there is infinite confusion over drugs and therapeutics grows more and more inexact ; while, if the means and methods herein advocated are correct, it will be seen at a glance that, in the main, the art of healing approaches exactness and, as I claim, becomes more scientific.

For if, for instance, water is better than drugs, we all use the same remedy, instead of a great variety of remedies.

So, too, if blood-letting is better than drugs in a given variety of cases, we have a means of relief, simple in its nature, free from the unknown quantity of variety and from the dangers incident to the use of powerful

drugs which alone could relieve desperate cases; and thus when we are enabled to depart from drug treatment, and so far as able, the art of therapeutics becomes more simple and therefore more exact.

But there is a second fundamental error into which the profession have fallen, as it appears to me, more serious, probably, than that of exclusive drug treatment, and that is "expectancy" (expecting the patient to recover naturally). The expectant theory dominates medical thought in all schools, and I believe it is the greatest foe to science and exactness.

This theory has taken the field so completely that it is at once and always like pulling against the current to advocate and to practice heroic measures, and it is here, more particularly, that my divergence from prevailing methods will meet opposition.

"Expectancy" is based on a scientific principle, expressed in the technical phrase "*vis medicatrix naturae*" (the power of nature to cure), and springs from the common observation that most diseases tend to spontaneous cure, as a wound does to heal naturally; but, like most departures in thought, it goes to extremes in practical application. It is the beginning of what "Christian Science" is the end of, or a scientific principle tending to evolve into a superstition.

"Expectancy" is the real genius that has to so great an extent supplanted "heroism" in the practice of medicine, and there is a gulf between the two which is bridged only by an exact scientific knowledge not yet attained.

"Heroism" was based in the theory that bodily diseases must be combated vigorously, and its most worthy exponent was he who attacked the "*materies morbi*" most energetically; while "expectancy" is based in the

belief that most diseased conditions tend to vanish naturally and therefore only require to be guided by careful and persuasive means.

Generally speaking, practically, expectancy is correct, but it, too, has been carried to irrational extremes and has served as a cloak for ignorance, as an instrument for imposition and as an excuse for the play of superstition.

Its first legitimate offspring was "Homeopathy," its second "Infinitessimalism," and its third "Christian Science" or nothingness, idealism, God.

All of these children of "expectancy" that thrive on the "*vis medicatrix naturae*," have served, and are serving a good purpose by holding in check the radical tendency and spirit of "Heroism," so supposed, but the better opinion probably is that, by opposing some well-established principles, expectancy has converted the *hero into the coward* and the *genius into the ignoramus*. For, in all cases, it says, "you need not attempt much, only trust to nature." When the physician would incline to heroic treatment, it says, "do not interfere too much with nature," and he inclines to conservatism and perhaps to forego a very necessary means of relief.

And, once falling into the embrace of expectancy practically and succeeding on that theory as he will in all mild forms of disease, the physician inclines to negligence and therefore to ignorance. He says, "I can only assist nature a little and, since I can not do much, it is not necessary to know much or to look up information," and thus he scarcely examines the authorities or reads the journals, much less to listen to counsel.

I have met many physicians who have, in rather a boastful spirit, told me that they never consulted the

books, but relied altogether on their own judgement and trusted to nature.

Three years ago a physician of 20 years' practice attended a lady in confinement under my observation, left the placenta in the uterus, returned the next day and, when asked to remove it, said, "O, it will come away in time; nature will attend to that." He returned yet again the second day to find that another physician had been there and removed it, barely in time to save the woman's life.

This extreme case serves to show the general tendency of expectancy, and I could recite numerous cases of a similar nature.

Especially is this disposition to trust to nature and its effects on the professional mind seen in relation to the practice of venesection.

I think I have met in consultation at different times and places more than a hundred physicians. This is a great many, but I believe I have not overstated the number. If I have ever seen a physician bleed a person I can't call it to mind, where I have suggested it they have generally been inclined to oppose it. I don't know of a physician who practices it generally and I know of many who would actually not know how to proceed with the simple operation.

The bad effects of the expectant theory is reflected often from the professional mind to that of the laity. The druggist is led to substitute, in the belief that medicines do but little good, and the housewife is opposed to "strong medicines" and frightened out of her wits at any attempt at heroic treatment.

Once, when I reprimanded a druggist for substituting, he replied, "Why I did'nt think it made much difference. When I have'nt the article prescribed I am in

the habit of using something else that will not hurt, as medicines can't do much any way."

And so it will continue to go so long as mere expectancy holds the fort and until something nearer exactness is arrived at in the treatment of disease.

The diseases to be treated of especially in this work are the three great internal inflammations, Pneumonia, Cerebr spinal Meningitis and Typhoid Fever, and my reasons for offering a treatise on these three ailments are that, after early childhood, more persons die from these affections than from all other acute diseases; that I believe these diseases are generally being treated irrationally, and that in their treatment something near exact and definite methods of treatment and wonderfully successful results may be arrived at.

With meningitis is included, with cerebrospinal meningitis, all those cases of meningitis occurring in typhoid fever, pneumonia, cholera infantum, etc., thus giving us a great many more cases than comes to the notice of physicians in the occasional epidemics of cerebrospinal meningitis alone, and making the mortality from it equal perhaps to that from typhoid fever or pneumonia.

I present the work in the following order and a little differently from the suggestion of my motto, "Facts are before theories."

I rather begin with theory or the fundamental theories, and, after this introductory statement, take up the "bloodletting controversy of 1857-8," supplementing it with *the rationale of phlebotomy*.

From this we pass to the consideration of pneumonia, cerebrospinal meningitis and typhoid fever in the order named, concluding with a supplemental article on *the absurdity of "forced-feeding."*

I also add an appendix with an article on "*some peculiar theorizing*" and on "*the effect on the average medical mind of modern bacteriological research*," which I especially request the reader to not overlook.

THE IMPULSE.

During the first two years of my practice it happened that I was called to treat a great many cases of pneumonia, typhoid fever and cerebrospinal meningitis, and in my own practice and in that of other physicians around me many persons died from these ailments, especially from the latter.

This experience caused me to reflect that these three diseases required special consideration and to think that possibly the profession was a little lame in methods of treatment.

Consequently I began to look up treatment for these ailments, to watch current literature on the subject and to note the treatment adopted by members of the profession here and there, as I came in contact with them, as I read reports from them in the journals and in the public press' and as I would hear, from time to time, from people who had lost friends from these diseases in various parts of the country, or who had noted methods of treatment with results of same.

The most forcible suggestion which has been brought to my mind, in this connection, is that persons dying with the diseases mentioned, do not all die from the disease proper, but from a given symptom or condition that may or may not occur during the progress of these ailments, except that we exclude cerebrospinal meningitis, which consists of one special symptom or condition which causes death.

In typhoid fever, for instance, if the patient succumbs it is generally from bowel hemorrhage or from head symptoms either of which may occur in other diseases or not all in typhoid.

In pneumonia death very generally, or often at any rate, occurs from a head complication or heart paralysis and not from the disease itself.

So that I was impressed more with the desire to know how to prevent and control given symptoms than to cure the disease, and the burden of this work is in the main to point out how to prevent and to relieve these symptoms.

CHAPTER II.

THE BLOODLETTING CONTROVERSY OF 1857 8 AS APPLIED TO INTERNAL INFLAMMATIONS.

When a student of medicine I read with interest the "bloodletting controversy of 1857 8," as reflected by Prof. J. H. Bennett, of Edinburgh, Scotland.

It was mainly from this controversy that sprang the antiphlebotomy movement, resulting in the general abandonment and condemnation of venesection by the profession and the medical authorities.

It is this controversy that the author desires here to reopen, and, having become convinced that the dispute has been decided wrongly, it is proposed to first examine critically the fundamental allegations upon which the antiphlebotomists base their reasoning and their conclusions.

These allegations are:

First—That "the *materies morbi* in the blood can not be reduced by venesection."

Second—That "venesection does not lessen the flow of blood to the inflamed part."

Third—That "inflammation, once established, can not be cut short."

Fourth—That "venesection renders the blood more watery and so changes its composition that the physiological processes are impaired and natural cure retarded."

If these four propositions are true then we may admit that Bennett has succeeded in upsetting the theories of Hyppocrates, Galen, Sydenham and Cullen; if not

true, then we must recognize the ancient sages as belonging yet to the immortelles.

Let us briefly and in order examine them.

1. The ancient writers drew no distinction between eruptive fevers and inflammations, and bled alike for small pox and pneumonia, supposing that, in each case, they accomplished good by decreasing the morbid material in the blood, and, while Bennett justly criticises them here, he falls into a like error of affirming that the "same principles" should guide us in each case. He reasons that in such cases as small pox no good could be expected to follow venesection, so far as influencing the morbid material in the blood is concerned, and concludes that in inflammations we should expect no more from it than in eruptive fevers.

Pneumonia is the one disease in which good is expected to follow venesection, because it decreases the *materies morbi* in the blood, but Mr. Bennett exhibits a serious weakness in not pointing out nor realizing, as it appears, the difference between the nature of the *materies morbi* of pneumonia and that of eruptive fevers. An eruptive fever might require venesection, on account of a complication or a severe symptom, but we would not expect to exert much, if any, influence on the *materies morbi*, by that means, in such a case.

It is true, as Bennett affirms, that Hyppocrates and even Sydenham had a very crude notion of inflammatory processes, and it is doubtless true that they practiced venesection from the force of the observed effects regardless of correct theory, but Bennett's position, that venesection should be abandoned because "the same principles govern" in eruptive fevers and in inflammations, is not tenable and because the same principles do not govern in the two conditions.

The *materies morbi* in eruptive fevers is a specific poison, while the condition in pneumonia is a circulation surcharged with carbonic acid poison and with effete matters not eliminated, (which condition, together with its indications for treatment, will be more fully explained in the next chapter), and Mr. Bennett's first proposition is negatived, in part at any rate, by the fact that the two conditions are vastly different.

2. Mr. Bennett's position that "venesection does not lessen the flow of blood to the inflamed part" is absolutely negatived by the observed facts, and "facts are before theories." This is demonstrated in venesection for meningitis.

I am called to see a case of meningitis or head congestion and, in addition to the severe pain, I observe that the veins in the whites of the eyes are engorged. I bleed the patient and immediately the eyes clear up and the symptoms subside. Here I absolutely see the blood leave the part and Bennett's assertion is disproved.

On this point see also a further explanation in next chapter, but I add here the strange declaration of Bennett that, in all inflammations, "more blood, rather than less, should be sent to the diseased part," (which assertion is perhaps in part responsible for the modern craze for forced-feeding in acute febrile diseases), and this assertion just following his attempt to reason that venesection did no good, because it "does not decrease the flow of blood to the inflamed part"—a direct and palpable contradiction in theories.

For, to affirm that venesection "does not decrease the flow of blood to the inflamed part," is equal to affirming that the flow of blood to the part should be lessened (and that it is lessened is the claim of the phlebotomist); yet he next turns and argues that more blood should

be sent to the diseased part, and thus admitting that, at any rate, venesection could do no harm, since it "does not decrease the flow of blood to the inflamed part."

3. To Bennett's assertion that inflammations can not be cut short and that therefore are to be allowed to run unmolested, every informed physician replies that it is erroneous.

While many diseases are self-limited, the eruptive fevers, for instance, and while many inflammations, let alone, seem to run a definite course, it is nevertheless true that abortive measures succeed in an infinite number of cases. Typhoid fever, until quite recently, has been regarded as a self limited disease and Bennett so classes it, but, under the cold bath and starvation treatment the majority of cases terminate in from fourteen to seventeen days, instead of twenty-eight to thirty-two days.

In threatened meningitis accompanying typhoid fever water-pouring aborts the congestion on the spot, and I have seen the same sypmtom yield to venesection so often in pneumonia that there ceases to be any question in my mind about it.

Congestion and inflammation of the lungs (and, for practical purposes, we need not draw a distinction between the two) abort under venesection in an infinite number of cases, and I have witnessed the same thing in cerebrospinal meningitis.

Indeed in the latter ailment if the patient recovers at all the case aborts or is aborted, because the disease consists of a symptom and condition running regularly and progressively from its inception to a fatal termination. It is not so much a disease *per se*, that, like typhoid fever and pneumonia, generally tends to run a

definite period, if unmolested, but is simply a local inflammation having no definite period of duration.

Numerous cases of pneumonia and pleurisy abort at once under a heavy dose of opium, yet it is hardly a safe remedy in pneumonia so administered. I know one physician, however, who has so treated pneumonia for forty years and his success is so general that he has seen no cause to change his treatment, so he states.

(I am quite sure that I have aborted some cases by the same means, but I repeat here that it is not a safe procedure as a rule. I have seen cases much injured in the attempt. In a case characterized with much congestion about the chest and head, it is, I think, very hazardous, and only quite recently I saw a young man die from opium poisoning in the attempt to abort pneumonia with the drug.)

Venesection is practiced in pneumonia both as an abortive measure and with a view of rendering the disease less severe, and every experienced phlebotomist knows that a timely venesection in this ailment gets the patient on his feet sooner, in the great majority of cases, than any other method of treatment.

Indeed many cases of pneumonia are of a progressive nature; that is, the inflammation begins in a small area of lung tissue and tends to extend to the entire lung, (and in fact to the other lung often,) and, in such cases, venesection is the absolute and reliable abortive measure.

On the proposition that venesection renders the blood more watery and unfit for building up purposes, Mr. Bennett presents a contradiction in argument and I think shows the error of his contention.

It is a fact that venesection makes the blood more watery, and it is a fact that in inflammations the

blood is deficient in water, and Mr. Bennett brings forward the commendable practice of administering alkalies in inflammatory affections with the view of reducing the viscosity of the blood.

He thus admits that the blood is deficient in water and advocates its dilution with medical agents that are supposed to accomplish just what venesection accomplishes.

Mr. Bennett is forced to the position he takes, however, because it is in keeping with his contention that inflammations need active support.

It is here that we may draw a distinction between the nature of, and requirements for, eruptive fevers and inflammations. and it is here again that we may note that Bennett places them in the same category.

It is doubtless true that many diseases that were starved under ancient modes of treatment should hav been supported, but, that acute inflammations do better on a spare than on a full regimen, is getting to be recognized with wonderful rapidity. There is no longer any question but that, in most active inflammations, digestion and assimilation are absolutely suspended and that the disease must be gotten under control before we can think about support, and that getting control consists largely in eliminating effete products with which the system becomes surcharged, often from overeating.

To urge nourishment and support from the start in a case of meningitis, where the stomach is foul and rejects everything; in a robust subject with pneumonia with a torpid liver, or in a case of typhoid fever, when secondary digestion and assimilation are absolutely suspended, is to talk very carelessly and con-

trary to science and common sense.

Yet this is Bennett's logic, and leading lights in the profession today are, from the force of habit, reflecting the same sentiments.

I was inclined for a time to follow the fine-spun theories of Bennett, yet only in a kind of half-hearted way; but more mature reflection and a wider observation, supplemented by the demonstrations of experience, have forced from my mind every one of his fundamental postulates.

Bennett's theoretical conclusion is that internal inflammations should be encouraged to a natural issue by supporting the system rather than to interfere with attempts at abortion, and he gives an illustration to support his theory.

He here starts with a correct assumption, but draws irrational conclusions from it, as I shall attempt to show.

He correctly affirms that in all inflammations it is the local disease that causes the disturbed condition of the circulation, just as a thorn stuck in the foot or any part of the body creates first a local irritation and secondly throbbing, pain and fever. He correctly says that it is the "*vis a fronte*" (cause or power in front) that causes the accelerated circulation, and not the "*vis a tergo*" (or power behind) or the condition of the system or blood that causes the local lesion.

But he makes a very awkward comparison, as it appears to me, in attempting to put the case of an internal inflammation and that of an injury, as from a thorn stuck in the foot, in the same category.

His comparison is good in showing a similar pathological condition as a cause of systemic disturb-

ance, but his deductions as to treatment are erroneous, because the same treatment is not applicable in the two cases.

The thorn must be removed or the parts poulticed to suppuration, but the former is impossible and the latter irrational in the case of internal inflammation. The treatment which would be successful in the case of the thorn injury would be universally fatal in internal inflammations such as typhoid fever, pneumonia or meningitis. It would be nonsensical in these ailments.

Bennett admits, however, that the two cases must be treated differently, the injury by removing the thorn or by encouraging suppuration, and the inflammation by encouraging it to its natural termination and the absorption and elimination, through the emunctories, of the exudation.

But, as he would admit that it would not be rational practice to attempt the removal of the thorn by simply encouraging the natural processes of tissue change, so may we, with equal reason, claim that inflammations cannot always be trusted to nature.

Now when we reflect that the trend of medical thought and practice has followed the lead of Bennett during the past thirty or forty years, and that his theory has been the basis of a reckless or rather negligent expectancy, and if we now perceive that his main theory is wrong, we should be ready and willing to question critically the prevailing methods of treatment for inflammatory affections and to reconsider "the blood-letting controversy."

More especially should we be impelled to this reconsideration, when, after all his theorizing, Mr.

Bennett affirms that he would yet bleed "in some cases."

And again, Bennett opposed antiphlogisticism generally—all heroic tearing down remedies—and, while tartar emetic is one of the most potent antiphlogistics, it seemed to be his chief reliance in pneumonia, so that, after all, he condemns venesection, yet holds slightly to it, and continues to use its chief ally—tartar emetic.

Thus, after all, Bennett, the great leading exponent and propagandist of the theory that acute febrile diseases require support rather than depression in the active stage, has proclaimed one theory and continued to practice on another.

That the profession have followed his theory rather than his practice is seen in the universal discarding of venesection and tartar emetic in late years, and, if any reader doubts the position of the profession here, let him make an effort to find a physician (or any number) who practices venesection, or to find prescriptions filled for tartar emetic, in the treatment of pneumonia; or let him examine the authorities—Quain, Reynolds, Da Costa, Hare, Bartholow and the whole host of modern writers.

Bartholow, however, has swung back to venesection and calomel in the treatment of pneumonia.

The college professors all reflect the theory promulgated by Bennett, and it is thoroughly grounded in the minds of the rank and file of the profession.

While writing this I get the information that some of the college professors on practice have, during the past winter, taught that blood-letting is again gradually coming to the front.

While I have, in a great measure, followed the professional trend, I am frank to say that I did so in a half-hearted way, and more because I lacked the boldness to dissent from current notions entertained by older men around me than because I was convinced by Bennett's reasoning.

On one hand the departure seemed progressive, but on the other hand it seemed too radical a departure from established principles.

That which particularly caused me to doubt the propriety of relegating venesection was the fact that Bennett failed to disprove the main scientific theory on which it is based, namely, that in inflammations the blood becomes viscous (or thicker) and that venesection renders it less so.

In other words, Bennett failed to banish from my mind a fundamental principle of pathology that had been well grounded.

But having now presented, and replied to, the theories on which Bennett bases his departure, and being convinced from experience and observation that they will not hold good in practice, I shall attempt in the next chapter to present the theory upon which venesection is based ; and if I add anything, in either form or substance, to principles long established, they are offered in a spirit of modesty.

CHAPTER III.

THE RATIONALE OF PHLEBOTOMY.

Perhaps the only thing in this chapter that may appear as new is the way it is presented. I have simply chosen a method or course of reasoning which will make my argument easily understood by everyone who presumes to an understanding of the elementary principles of physiological law.

First and foremost the rational practice of medicine depends on a correct understanding of physiology, and, when we follow a line of procedure in harmony with this science, we may expect to meet with the best possible results.

I have come to realize, however, and the longer I live the more thoroughly, that there are few physiologists engaged in the practice of medicine.

The average college graduate is deficient here, and after a physician puts up his sign he seldom devotes time to anything more than practice and the making of money. He reads works yet on practice, surgery and obstetrics and is often compelled to refer to his anatomy, but physiology, the most scientific branch of medical study, he seldom further considers.

I am inclined to the opinion that many of the prevailing methods of treating disease originated in the minds of those who, if they understood physiology, have had but little regard for her laws.

For, as it appears to me, the abandonment of venesection was an ignoring of physiology, and the

methods of treating disease that have taken the place of venesection clear violations of physiological principles; and if this proposition appears true on the presentation of the argument, the author has made out his case in favor of phlebotomy.

When venesection was in vogue authors on "Practice" used to give its rationale, but modern writers have very naturally ignored the subject.

It is only through the practice of a given system or means of cure that we become familiar with its rationale. Having quit the practice of venesection, the profession have ceased to consider its laws. The authorities advise against it, and therefore they do not explain it, and hence we become ignorant of it.

The art having been lost, its rationale has been forgotten, and therefore the necessity for its restatement.

The headings under which the author's theory is presented are:

First—The physical principle involved.
Second—The physico-physiological principle.
Third—The physiological principle.
Fourth—The physiologico-pathological principle.

1. THE PHYSICAL PRINCIPLE, or that where physical laws are taken specially into the account.

Maxim—*The human organism, whether in health or disease, is governed by physical, as well as by physiological and pathological laws.*

There is, in all congestions, what is called a determination of blood to the parts affected, tending to engorgement and inflammation.

The circulation is full and the arteries are beating

plump, *plump*, *plump*, forcibly and often rapidly, against the diseased part, as against an inflamed member when held downward. If, now, we deplete the circulation by means of venesection, the arterioles tap, tap, tap, softly against the diseased parts, as against an inflamed member when held upwards.

To deny that this change from a full to a depleted circulation would lessen determination and congestion is to deny a simple law of physics, hydraulics, or, we may say, of mathematics; for, plainly, if we take away all the blood, determination would cease, and with equal reason may we affirm that the removal of a part of it would have some effect on the force of the circulating current, and thus lessen the determination. The stream would be less full and flow with less force and persistence.

I think that this proposition admits of no denial; yet Bennett has denied it in the assertion that venesection does not lessen the flow of blood to inflamed parts.

And in some forms of congestion there exists a peculiar form of circulation pressure, not altogether the result of an overflowing current, but the result of a change of the current in a sense, or of abnormal distribution. There occurs a dam, as it were, over which the current cannot readily pass, or on account of which it passes more slowly, when the circulation becomes sluggish, requiring more force to throw it, yet not moving smoothly through natural channels.

In pneumonia, for instance, the lung being congested the blood ceases to flow through it readily, when there results a backward pressure upon the

heart and over to the systemic circulation seeking an outlet through venesection.

And something similar, in a sense, occurs when, through overfeeding or a retarded excrementitious process, one or both (and the two are one), the circulation becomes too full and demands artificial relief.

In such cases venesection not only performs a vicarious function of assisting excrementitious organs, but it also acts mechanically by relieving an overburdened heart.

The instant relief afforded by venesection in many cases goes to show that a mere overflowing current requires attention, and not pathological conditions. Take a case of commencing meningitis: In the short space of time required for venesection there could be no change in the pathological condition of the parts, yet the relief is instantaneous.

Clearly, in such a case, it is only physical or mechanical laws that need to be taken into consideration.

In performing venesection in such cases we relieve the part affected of pressure, and we relieve the heart of the burden of throwing an overflowing current.

Phlebotomy seems to weaken the heart for the moment, but it soon begins again to perform its function smoothly and with less burden to perform. Leave the circulation too full and the heart struggles vigorously for a time, but finally yields to an extra burden and we have paralysis of the organ or other serious complications, to which the patient succumbs, even after the primary ailment has abated—a demonstration again from the simplest laws of physics.

Again, when we apply ice to the bowels in typhoid fever, to arrest a hemorrhage or to prevent it, we are stimulating a capillary contraction, the object of which is to drive the blood away from the congested glands; and to deny that venesection would do the same thing, in a measure, if done before the hemorrhage occurs, is to deny a simple law of physics.

How many have asserted that a hemorrhage has often acted well in relieving congested parts? If so, what is the difference between hemorrhage and venesection.

Is it not, in each case, the reduction of the circulation pressure?

If the application of ice tends to check hemorrhage, would it not also tend to prevent it? If venesection would have a similar effect on the circulation, should it not be resorted to at an early stage in robust subjects, in order to prevent hemorrhage from the bowels in typhoid fever (which is difficult to confrol and often fatal), and in order to prevent extensive extravasation in pneumonia and meningitis?

2. THE PHYSICO-PHYSIOLOGICAL PRINCIPLE.

Maxim—*Physical and physiological laws are intimately connected.*

The second principle upon which phlebotomy is based is what may be termed the physico-physiological principle, because in its consideration both physical and physiological laws must be taken into the account.

The great central organ of vitality, and the one which is especially called on to perform extra labor in acute febrile diseases, is the heart; and it is a well-demonstrated fact, recognized by every informed physician, that this organ often gives way (peculiarly so in pneu-

monia) or becomes seriously diseased or burdened in the progress of serious febrile ailments, even when or after all other organs are performing their functions normally and easily and recovery has been expected.

The action of the heart is governed by two well-recognized forces, the *vis a fronte*, or power in front, and the *vis a tergo*, or power from behind, the former dependent upon physiological processes, the latter governed by a law of physics or mechanics.

The living process of metamorphosis, the life process going on in the remote cells of the body, is and constitutes the physiological process of the living organism, and consists in chemical changes whereby the dead or effete products are thrown off into the venous circulation through an excretory process and on through the excrementitious organs—the skin, kidneys and the bowels—and whereby, or the result of which, fresh material is drawn from the arterial circulation. This constitutes the *vis a fronte*, or power in front, which draws upon the heart and gives it strength. It is the physiological pump, as it were—the vacuum which nature abhors and attempts to fill and thus keeps the heart at work. Destroy this process and the heart ceases to act; impede it and the heart becomes enervated and weak.

When the system becomes surcharged, as we say, from overfeeding and from a sluggish condition of the excrementitious organs, this tissue metamorphosis becomes burdened and the *vis a fronte* lessened, resulting in fever and a manifestation of heart weakness. The heart is warning us of an obstruction in front, beating vigorously against that obstruction and asking for either a clearing of the track through the emunctories or a lessening of the load by means of phlebotomy.

To leave the system in *statu quo* here and resort to heart sedatives as a means of relief is about as rational a procedure as would be that of a man who would add his weight to a load already too heavy instead of putting his shoulder to the wheel. Nature might struggle through under the burden, but the number of failures that are seen evidence the irrationality of the plan, to say nothing of its theoretical absurdity.

When we speak of the *vis a tergo* we mean the power of the heart itself to throw the circulating fluid; and, while we may admit that this power from behind is dependent on the *vis a frontæ*, or the physiological cell metamorphosis, which is proven by the fact that the heart becomes weaker or fails as the cell metamorphosis is lessened or suspended, so must it be admitted that there is also an individual heart life force, as proven in the fact that the organ may become paralyzed and cease to act even when the physiological processes in the remote cells of the system are all in perfect order.

The two forces, *vis a frontæ* and *vis a tergo*, are, from the nature of the case, interdependent; and here we see the connection between physical and physiological laws; and thus, while we must always guard and keep in order the *vis a frontæ*, we must also remember that the *vis a tergo* also needs attention.

In many diseased conditions the interruption of the physiological life processes necessarily weakens the *vis a frontæ*, when at once we observe that the heart becomes weaker, yet acting more vigorously. This of itself will finally overcome the heart, even though the burden of a full circulation was partially relieved by venesection; but it cannot be denied that, while the diseased process is at work, the physical burden of the

heart is much greater by virtue of its increased activity alone, and that the extra force necessary to throw a given volume of blood must tend to weaken the heart, just as extra work tends to overcome any part of the muscular system.

3. The physiological principle.

Maxim—*Health depends on normal physiological conditions, and therefore the first object to be sought in the relief of disease is to re-establish those conditions.*

The third principle upon which phlebotomy is based, is that it takes directly from the circulation a portion of the blood which is diseased and surcharged with effete matter that has not been eliminated by the emunctories and the presence of which retards the healthy physiological processes and thus tends to weaken the vital forces.

Its argument is that a depleted circulation, containing less effete matter, is more conducive to health and strength than a too full circulation containing more; to which is added the theory that the nerve centers are shocked and oppressed by a full diseased circulation, while they are relieved and refreshed by a depleted one.

It is a well recognized fact, and one established by scientific research, that, in internal inflammations, the blood is more viscous or fibrinous and deficient in watery elements than in health, and it is another fact similarly established that venesection makes the blood more watery.

It is an established fact again that in pneumonia the blood becomes surcharged with carbonic acid and deficient in oxygen, and common sense and arithmatical law teach us that an abstraction of a portion of the blood leaves less carbonic acid in the circulation.

It matters not how that venesection makes the blood more watery, whether the water is at once absorbed from the tissues or whether exhibited by mouth (for both amount to the same thing, the former following the latter), and that the lungs can supply oxygen more readily and sufficiently to a depleted, than to a full, circulation is readily seen.

By taking on water the blood is washed of its more solid elements through the skin, kidneys and general emunctories, and by taking on proportionally more oxygen through the lungs more poisonous matter is eliminated.

So that venesection tends to re-establish physiological conditions, to encourage normal tissue metamorphosis, and thus to promote health.

That venesection would act somewhat differently in health need not be considered, for it is diseased conditions that we are considering.

Many antiphlebotomists advocate the administration of alkalies in internal inflammations, with the view of lessening the viscosity of the blood and thereby abating the pathological condition—the disease. Thus they are attempting, in a roundabout and uncertain way, to get results such as venesection brings directly or at once; and thus, while they condemn phlebotomy, they are attempting to create a chemical change in the blood similar to that produced by it.

Representing current opinion, generally, though not universally accepted, Prof. Hare condemns the use of aconite, veratrum, antipyretics and blood-letting, on the ground that they only mask the symptoms and give a deceptive promise of relief, while they lower the vital forces and thus lessen the prospects of recovery.

Of the drugs mentioned I accept as true that they only mask the symptoms and retard the real physiological processes (are only poisonous), but that the lowering of the strength of the patient merely is harmful in the acute stage of fevers is not supported by sound reasoning, and that the effect of blood-letting is in any way similar to that of depressing drugs cannot for a moment be admitted.

If the reduction of the strength of the patient tends to prevent or lessen the chances of recovery, how is it that the patient becomes convalescent and starts on a sure road to health, often, only after being reduced to a mere skeleton from the ravages of disease?

The physiological processes proceed just the same in the weak as in the strong subject, and often better, because in the overfed the system is surcharged with foul matter, while in the reduced subject often the system is hungry, as it were, and the vital processes acting vigorously.

Likewise, when the blood is surcharged with deleterious products an abstraction of a part of it leaves less effete matter to be eliminated by the excrementitious processes—less work for the system to perform—and hence blood-letting conduces to strength by taking off the strain, and the sooner enabling the physiological processes to proceed naturally and free from the burden of effete products.

To those who have learned the evils of the modern and prevailing habit of over and forced feeding in fevers, and who have come to realize the wonderfully rational and successful plan of starvation (excepting water), I desire to put the question: Should we not, from a scientific standpoint, expect similar results from starvation

and venesection? Starvation decreases blood; so does venesection.

And this is all primarily that venesection does.

The claim that venesection is injurious by reducing the strength is equal to the assumption that patients with little blood naturally would succumb to fever more generally than full-blooded ones, while the reverse is the rule.

In typhoid fever, pneumonia or meningitis it is in the plethoric subject where we meet danger.

Must not the patient be reduced at any rate before convalescence? Yes. Then what form of reduction of strength is the quicker and the more easily overcome?

Every physiologist knows that persons soon rally from the loss of blood in any reasonable amount, whether in sickness or in health. Every physiologist knows that the system can not so easily rally, or is likely not to rally, while under the influence of poisonous drugs and especially heart depressants. Every physiologist knows that the system can not rally while it is burdened with a surcharged circulation and with effete products that require to be eliminated.

What physician has not wondered at that peculiar condition that often supervenes in the later stage of pneumonia, where, although the patient seems so strong generally and where he coughs strong and expectorates easily, there is yet a heart depression which carries him off?

And what physician does not know, without wonder, that, however much reduced a patient may be from long continued sickness or from mere loss of blood, he will yet survive and regain his strength rapidly?

What is the difference between the two forms of weakness—that from venesection and that from heart sedation with drug depressants or heart paralysis from the retention in the circulation of effete matter, both of which latter are virtually the same? The former is a simple natural weakness, from which the patient soon rallies, because the physiological processes are proceeding with less interruption and burden. The latter is a diseased or artificial weakness, caused by a suspension of the physiological processes, and therefore necessarily followed by dissolution.

Take a case of severe pneumonia from the start. We treat it on a mild expectant plan or on heart sedatives, and, as it seems, are getting along nicely until about the seventh or eighth day, when we expect a favorable crisis, but instead we have an unfavorable one; the temperature rises a little, the heart becomes weak and, although the lung has cleared up and the patient seems strong, and although we have hedged and yet continue to hedge against the condition with ammonia, quinine, digitalis, strychnia or stimulants, the termination is often fatal. It is because the system, and especially the heart, has been overtaxed in an effort to eliminate effete matter which might have been removed, to a great extent, at the outset by the timely use of the lancet.

It is not known just what the effect of poisonous drugs, such as aconite and veratrum, is on the physiological processes, but reason teaches me that it is the same as that produced by accumulation of effete matter. At any rate they mask without eliminating, and the vital forces give way the same under the continued presence of effete matter and under the use of drug depressants. The general admission is that depressing drugs

weaken the heart and really retard tissue metamorphosis. If so, they not only mask, but they prevent the system from throwing off effete matter, and thus add fuel to fire.

It is only eliminants that could possibly assist the natural processes, and as the greatest eliminants are venesection, which removes surcharged blood directly, and calomel, which eliminates indirectly, without masking or suppressing physiological tissue change, it follows that, from a scientific point of view, the old treatment of antiphlogisticism is the more rational system, and that the modern system of drug depression is to be condemned as unscientific and irrational.

Be it remembered that drug depression by means of heart sedatives has taken the place of venesection and is the prevailing plan of treatment for acute febrile diseases, but Prof. Hare makes a great slip of the pen when he confounds them as similar in their effects on the human organism.

Drug depression and venesection, I repeat, have little in common. The former acts upon the nerve centers and from there the circulation and physiological processes are depressed. The latter acts in a manner mechanically and chemically, and, by lessening the pressure of the circulation and abstracting a portion of the surcharged blood only reduces physical strength, but at the same time takes determination away from diseased parts and leaves the physiological process, not only normal, but better conditioned to proceed. Aconite, antipyrene, and such powerful drugs, mask poison and deceive, depress vital power and retard tissue metamorphosis, while venesection is free from all these. When you take acetanelid you congeal the blood in the capillaries, but the loss of blood by epistaxis, a wound

or venesection only invigorates healthy tissue action by creating a circulation hunger.

4. THE PHYSIOLOGICO PATHOLOGICAL PRINCIPLE.

Maxim—*Physiological and Pathological Processes Have Nothing in Common.*

If the foregoing maxim is true, then the antiphlebotomists, Bennett and his coadjutors, and all who have followed the same lead, present their case on the anomolous and absurb premises of attempting to harmonize two antagonistic principles. For they contend that, when a pathological condition is set up, it cannot be cut short, but may, and should be, pushed to its natural issue by the physiological process.

If, for instance, an extravasation of liquor sanguinis occurs or begins to occur, as, say, in pleurisy (or it is at any rate the logic of their theory), that no attempt is to be made toward checking it independently of physiological tissue change, or that it is best checked or cured through physiological processes.

It is well agreed that an exudation once poured out must be absorbed by physiological action, but that this action is the reversal of the process now going on is, I think, clearly established.

The pathological condition is an exasmotic one, the pouring out of liquor sanguinus from the capillaries, while the physiological one is an endasmotic process, whereby the exudation is absorbed back into the capillary circulation and on through the excretory channels. Just where the former process stops the latter one begins.

That some inflammations seem to run a definite course, when let alone, is true, but it is equally true that

the natural termination of others is the destruction of life, and thus, while it might be good policy to only encourage the former one in its career, such a course would, at any rate, be reprehensible in the latter.

Besides, the genius has not yet arrived who could, in advance, distinguish between the two.

It may be reasonably contended that pneumonia and typhoid fever, if let alone, have something near a definite period to run, and then tend to spontaneous cure; but the tendency in pleurisy and cerebrospinal meningitis is an uninterrupted progression, and I am quite sure that many cases of typhoid fever and pneumonia also assume this progressive form.

And, whatever the tendency, in this respect, to encourage physiological processes only, would be but equal to letting the pathological ones alone.

While the physiological processes are the reverse of pathological ones, the former does not counteract the latter, but only correct the result. The pathological process pours out, the physiological process takes up. Both may proceed and do proceed together, nature always repairing, but, to affirm that, by encouraging physiological tissue change, we push a pathological process to its natural issue, is to make the diseased and the healthy processes identical.

After all is said, the position of the antiphlebotomists is simply to let inflammations alone, that the diseased process is a natural one tending to self cure.

But, as experience and reason have taught us that physiological processes may be artificially suspended, so has experience (if nothing more) taught us that pathological processes are subject to therapeutic art.

And now, as we proceed, let the reader reflect that, if I have offered a correct rationale, then venesection is the proper treatment for internal inflammations and prevailing methods wrong, because other and prevailing treatments do not fulfill the indications and can therefore be no more than attempting to perform a given work with tools other than those most suitable for the purpose.

Besides, the prevailing methods of drug treatment, for the most part, have no settled and definite rationale. They could not have, because they are so various, and it may be noted that authors who recommend them say little in the way of reasoning out the principles upon which they act as related to physiological laws.

Phlebotomy as practiced in the olden times was perhaps justly condemned, because it was carried to extremes.

But modern medical thought and practice has gone to the other extreme. From bleeding for everything we have gone to bleeding for nothing, from bleeding to extremes and oft repeating we have gone to bleeding not at all.

It remains for the genius of the present and the future to get on rational grounds between these extremes, and, when that is accomplished, we may expect to have done much in the direction of substantial medical progress.

A HABIT OF PROFESSIONAL NEGLIGENCE, OR PERHAPS TIMIDITY, OR STUPIDITY, OR INDIFFERENCE, OR CRIMINAL CARELESSNESS, OR WHAT NOT.

The most radical opponents of phlebotomy must admit that it has yet a place in therapeutics, even though that place be small. In an urgent case of puerperal convulsions, for instance, nothing else can take its place.

I remember some thirty years ago, before the St. Louis Medical Society, Prof. Pallan condemned in severe terms the growing habit, with the members of the profession, of going without a lance in the pocket.

He cited a case where three passing doctors were called in to see a lady who needed immediate bleeding, and not one of them was prepared to do it.

Venesection is of course an emergency remedy, and such above all are the remedies which the physician should know how to use. Most cases of sickness get well as readily without, as with a doctor, especially all the milder cases of pneumonia, and here we have the doctor making bills when he renders no aid and absolutely unprepared to render assistance should it be necessary.

The greatest recommendation for a physician should be that he knows what to do in emergencies. When one needs a doctor surely is when he needs him badly.

I believe, however, that it has been timidity and a false pride, to a certain extent, that have caused the profession to get out of the habit of performing venesection, and, getting out of the habit, they dispense with it often when they know that it should be resorted to.

And especially are we slow to hurt a delicate patient and to besmear, with blood, the persons, the linen and the tapestry of the exquisite—the elite of our patrons.

It is said that when Napoleon once called a physician to attend his wife, knowing the timidity of the generality of men when dealing with the aristocratic and the great, said to him, "Take hold and treat her as you would a peasant."

The doctor who has practiced some years knows how much there is in the sentence quoted.

Or it may be, after all, that supine laziness and indifference on the part of members of the profession was and has been the initiative cause of the antiphlebotomy movement. I have observed that lethargic, careless and indifferent men seldom resort to heroic measures requiring work, unless forced to them.

Next the sloth breeds the coward, and the coward the ignoramus. The lazy man does not want to practice blood-letting. When he gets out of the habit he becomes timid about it; timidity causes him to forego the practice, and he then forgets all he may have known about it and becomes ignorant of one of the greatest arts in therapeutics.

And, naturally, when men do not want to do a thing, or do not know how to do it, they conjure up all manner of theories against it. These theories, supported and encouraged by a persistence of the disposition which hatched them, finally become standard and authoritative, are passed along the channel of medical education and dogmatically asserted in the face of and against science, history and experience. And thus phlebotomy has become almost a lost art.

The object of this work is to revive it and to assign it to its proper place in the healing art.

Another thing that has operated against the practice of venesection is an honest reliance on the expectant theory, on the belief that nature is first and the physician only second in the therapeutic effort.

Still another element to be considered is the commercial spirit of the times. Innumerable remedies are discovered, invented and manufactured and thrown on the market, where they are eagerly sought by a profession equally as gullible as the general public.

The physician who grabs at every new remedy and puts it in the place of long-established means of relief has no reason to laugh at a public that are gulled by venders of patent nostrums. One is duped by subsidized medical journals; the other by a less culpable public press.

And now, after all we have said on the subject of venesection, who is the best authority—the man who has practiced it or the man who has only theorized on it?

"Facts are before theories," and when such great minds as Hypocrates, Galen, in the ancient world, and Sydenham, Cullen, Paget, Andral, etc., following the reformation, and down to the present century—men of wide experience in the practice of the art and of wide general learning, and men as capable to judge of facts as the greatest men of today—when they have asserted so positively the merits of venesection; and when the dissenters, such as Bennett, condemn it in theory and hold to it in practice; and when we know that modern authors who have opposed it are not men who have practiced it, but who speak only on theory; and when we see such men as Bartholow admit his former error and proclaim that venesection cannot be abandoned, may we not conclude that the subject demands our most earnest consideration?

CHAPTER IV.

PNEUMONIA.

I shall present the subject of pneumonia in my own way and according to my experience and observation. No attempt will be made at minute differential diagnosis, nor will I devote any time to the histological aspect of the disease nor to the various chemical blood changes that science reveals in pneumonia, but handle it in a way that the average physician, and indeed, the laity, may understand me.

Pneumonia is met in mild or severe form, according to the severity of the winter, or according to the number of succeeding cold snaps. In mild winters and in the early part of the season it is generally of a mild form, while, during severe winters and toward spring, it is more severe.

Not always so, but as a general rule.

In this first fact there is something that should put the doctor on his guard, for it is a tricky and deceptive disease, and often, a case which at outset seems mild, may turn out to be severe, and mild and expectant treatment, which will do for mild cases, will be found inadequate for severe ones.

Another distinction to be drawn in cases, and that should put the physician on his guard, is that the severity of the first symptoms does not determine the gravity of the case.

Bartholow says that pneumonia comes on suddenly with a rigor, or preceded by a day or two of malaise,

followed by a rigor—one variety coming on suddenly, as after getting too warm and cooling off suddenly, the other covering two or three days in its initial approach.

In my experience, the variety coming on slowly may consume a few days or a great many in its approach. A person may have a cold for a month, or, through exposure for a long period, may get gradually sore about the chest, and finally, after some weeks, take down with pneumonia, and, so insidiously may the case pass from a mere indisposition to severe pneumonia, that the primary requirements of treatment will be lost sight of. Rather than approach suddenly and with severe symptoms, these cases develop into deep-seated and widely-spread pneumonia, without any one of the symptoms that usually warn us of danger.

As it seems to me, the difference between these two variety of cases is so great that they reflect two distinct diseases, and I am sure that they often require a different treatment in some respects. In one variety there is a sudden attack upon the lung, usually on the outer surface or involving the pleura, and thus characterized with much pain. In the other variety the lung becomes sore and slowly congested and the exudation begins in the center more generally and extends perhaps rapidly without any pain and with less cough than in the other variety. The nerves seem to be obtunded in these cases, and a dull heavy pain develops slowly in place of a sudden and more severe pain peculiar to the other variety.

(It is useless to draw a distinction between pneumonia proper and pleuropneumonia, because the average physician can not differentiate and the treatment need not be much different—must treat symptoms anyway as they develop.)

A distinction usually drawn between these two varieties is that the first is unmasked, the second, masked pneumonia. It is a good differential designation and, in remembering it, we should remember that the masked enemy is the one to be the most feared, as much here as anywhere else, for it is the enemy that robs you (if not well on guard) of your patients, your mental equipoise and your reputation. It will steal upon you unawares, it will get a deeper hold than you think it has, even after you detect it, and it will laugh at the remedies you are likely to apply for a case seemingly so mild.

In the winter of 1869-70, and again a year or two later, I treated a great deal of pneumonia of a severe type, and, while I was inclined to follow the tide that had then well set in against venesection, I adhered very strongly to calomel, the blister and other of the antiphlogistic methods. Indeed I pushed calomel so strongly that many of my patients were severely salivated.

So that, while I bled only when forced to do so, the other heroic means resorted to perhaps compensated for it.

I, however, resorted to venesection in some of the more desperate cases and, to consider the treatment generally adopted by me, I may say that it was decidedly antiphlogistic. I also and always expected to get control of the disease before I thought about supporting the patient. In fine, I tore down from start to finish, for the first few days at any rate, and met with good success so long as I followed this line.

At the same time I practiced in a community where there existed a strong prejudice in favor of heroic treatment, especially blood-letting. Often, when called to

see a case of pneumonia, the patient would insist on venesection, even when I was not disposed to resort to it.

Making some inquiry as to the cause of this prejudice I was told by an old citizen that some years previous there had been a severe epidemic of pneumonia and that in that epidemic a Dr. ——— had bled his patients and they recovered, while those that were not bled died.

There was at this time especially a growing prejudice in the professional mind against venesection, growing out of the controversy of ten years previous. The antiphlebotomists had won in this controversy, for the time, and it seemed that the profession rather gladly seized a theory so much calculated to relieve them from the inconvenience of blood-letting.

Then came along, of course, and in proper order, various theories as to what should take the place of venesection and numerous drug sedatives, depressents and antipyretics clamored for recognition; and, following the general current of modern thought, more perhaps from habit and imitation than from real scientific enquiry, I drifted away from ancient moorings.

But occasionally the ghost of antiphlogisticism, resurrected by experience and observation, would rise before me. I would see the drug treatment fail in that class of cases where formerly venesection was regarded as the "sheet anchor," and occasionally I would see or hear of venesection coming to the rescue successfully in similar cases. I would see, as it seemed, aconite and veretrum paralyze the heart, antipyretics mask the symptoms, only to deceive, and I would see cases pulled through only with herculean efforts, that, under venesection, would have aborted in a few days, according to the pretentions of the ancient school of thought, until I

came to seriously question the virtue of prevailing modes of treatment and to doubt the rationalism of the departure from older notions.

The expectant plan of treatment has, however, been my reliance generally, until more recently, or, I may say, more so after my first few years of practice and until up to the present time.

The prevailing plan of heart sedation with aconite, veratrum as primary treatment, I have not adopted, except that I have used veratrum quite frequently. I have always regarded aconite as too powerful a heart depressent to be used as a medicine at all, and, after careful investigation and repeated experimentation, have come to the same conclusion with reference to veratrum.

All along, during thirty years of observation, it has been noted that certain epidemics of pneumonia have been exceedingly fatal, but the writer has noticed that, in the desert of destruction, an occasional oasis has appeared, and in that oasis has been found the lancet, calomel in heroic doses and general antiphlogisticism in timely use.

I have observed that in the general run of cases and in most seasons, the expectant treatment has been successful, but that in severe cases and severe epidemics it fails utterly. Indeed, the difference in cases is so great that one, who has been successful with expectant or with any of the modern plans of treatment, wakes up as though he had met a new disorder, when he comes in contact with a severe form of the disease.

And one may run along for fifteen or twenty years, treat numerous cases and never encounter the disease in its more severe form.

For this very reason many physicians abandon heroic treatment, rely on the expectant plan, and are very much inclined to ridicule, and look with contempt on venesection and on antiphlogisticism generally, until they encounter one of these severe epidemics, when they lose most all their cases and are driven to mysterious explanations for causes of a mortality exceeding that of the balmiest days of phlebotomy.

Then they have learned something from experience, if they are perceptive, but they say little about it.

Right here it may be remarked that, in his onslaught on venesection, the experiments and statistical collections of Bennett were confined to a period of only a few years, and that one of his strongest supporters had practiced but eleven years and treated but seventeen cases of pneumonia. An experience covering so short a period as that to which Bennett's investigations were confined is entirely unreliable. The author treated something near fifty cases, mostly on the expectant plan, from 1884 to 1899, without a single fatality.

During this period I bled very few cases and even those might have gotten along without it. I really had none of the more severe variety of cases, and from this experience alone could have given strong endorsement to Mr. Bennett's side of the controversy and to expectancy, if not indeed to the prevailing plan of heart sedation, but I had learned something before that had created a suspicion, and something since that has confirmed the correctness of my suspicion, that Bennett's observations did not extend over sufficient grounds.

Furthermore, I treated cases in 1869-70 and again in 1884 on heroic measures, excepting venesection, and succeeded, but in 1899 treated apparently the same kind of cases on the same plan with universal failure and some similar cases by timely venesection with magical success.

During this epidemic of severe pneumonia in the winter of 1899, and when many died of the disease, which was called by the doctors "typhoid-pneumonia," as an excuse for a heavy mortality, of course, an old citizen said to me: "Doctor, I remember that fifty years ago, when I was a boy in Indiana there was one winter a great deal of "typhoid and pneumonia" (as he put it) and when they were bled freely they generally pulled through, but if they were not bled they died."

And such exactly was my experience and observation during the winter of 1899, in my own practice and in that of numerous physicians with whom I came in contact, as well as in the general current of news from all over the country.

"LaGrip" has been very fatal, we hear, in different parts of the country and during certain winters, and this means very generally the pulmonary complication of "Grip" pneumonia.

Let the reader reflect that influenza itself, without a complication, never terminates fatally. I have never seen the "grip" kill any one, but during epidemics of that disorder, it is very convenient for the physicianf who loses a case of pneumonia, to call it a severe case of "grip," since he is more likely to be excused for losing a case of the new scourge than one of pneumonia, a regularly occuring disease with which we are all familiar and should therefore know better how to treat; and hence

the press terms with accounts of fatalities from "grip," when, if the matter is carefully looked up, it will be found that there has been a fatal epidemic of pneumonia, in which venesection has not been generally resorted to.

And the author is not the only physician that has been awakened as from a Rip Van Winkle slumber and been thoroughly impressed with the idea that venesection is the sheet anchor in certain forms of pneumonia.

We hear the returning wave of phlebotomy from all quarters in both the professional and in the public mind.

While writing this work, which was prepared during the month of May, '99, (excepting a few corrections added since) I questioned a medical student from Chicago as to the position taken there by his lecturer on practice on this subject.

He replied: "They died like sheep with rots in Chicago this winter from pneumonia.

"All our cadavers were pneumonic subjects, and our professor on practice says that we are gradually coming back to venesection as the most important consideration in the treatment of pneumonia."

And now a little reasoning right here.

I assert, as a fact that cannot be controverted, that reasonable venesection will injure no case of pneumonia, and ask the question: Had we not better keep on safe grounds?

I know from experience that it will save many cases that will not be relieved by any other known remedy.

In two cases where I considered venesection the sheet anchor, one died under the cold-pack treatment, the other barely pulled through under it. No impression of consequence was produced by the pack treatment, and because of the heavily surcharged condition

of the circulation. In the less severe case a terrible sweating crisis probably served the eliminative requirements.

And how do I know that venesection will save any case? Because of repeated successes from it in cases just such as have resisted all other means of relief.

Heroic treatment was abandoned on the ground, principally, that it was too depressing; yet staring us in the face is the disproof of this notion, and in the fact that the more robust the subject the more severe the disease.

It is notorious that our dangerous and fatal cases of pneumonia are among the plethoric, while in more delicate subjects the disease is scarcely to be feared at all.

On the theory that antiphlogisticism is wrong treatment in this disease, it ought to appear that strong and robust subjects withstand it better than those of delicate constitutions, while the reverse is the rule.

Our experience and observation on this point is a common-sense demonstration which no theory can upset.

But the modern fad of forced feeding generally has caused the profession to depart from many well-established principles, and in no disease do we see the error of the supporting craze more than in pneumonia, if only we pause and reflect.

"Facts are before theories," and the fact that pneumonia is more dangerous and fatal in full-blooded than in thin-blooded subjects is a clear demonstration that antiphlogisticism is right and the supporting plan wrong n this disease.

The modern and prevailing notion that the patient must be supported in the primary stages of acute dis-

eases and when the vital organs are in a state of congestion has been completely banished from my mind, both by experience and observation and by reason. I never could grasp it, and am now thoroughly convinced that it is erroneous, and indeed as absurd as would be the argument for the turning of a breeze rather than a stream of water onto a conflagration with the view of extinguishing it.

But the question will arise right here: Is it not true that pneumonia is also dangerous in the persons of drunkards and those with a broken down constitution, as well as in plethoric and robust subjects, and, therefore, is not the supporting plan better adapted to such cases than the antiphlogistic one?

I reply that the supporting plan, or more correctly the stimulating plan, should in such cases supplement antiphlogisticism, and at the proper time.

I have reasoned in another chapter that the venesection and calomel treatment is supporting, because it relieves the heart and the sooner gets the system in a condition where the physiological processes may proceed more naturally, and that, though the patient is for the moment reduced in mere strength, he is better conditioned to survive.

The same question may be put to those who follow the prevailing plan of heart sedation and, as I think, with greater force of logic.

The patient is sick and in danger at any rate in pneumonia and certain cases need stimulants more than others, but the vital question is, what should be the treatment aside from considerations of bodily nourishment or support?

Which would more likely injure a case, if treatment is to be viewed from this standpoint, venesection and calomel or aconite and veratrum?

And why is it that those who prefer the latter to the former seem not to think so much about the dangers of heart failure under the modern plan of heart sedation?

In those cases that require support the danger is especially from heart weakness, and of all treatment the most irrational, it is to administer heart depressants for the relief of heart weakness.

I reason that in pneumonia the heart is overburdened and that the conditions existing are the same as those produced by medical heart sedation, the prevailing method which has supplanted venesection and calomel, and I reason that, while the modern and prevailing plan of primary treatment masks the symptoms and paralyzes the heart, venesection and calomel relieves the strain on the circulation and thus gives endurance to the heart power.

I therefore argue that, in pneumonia of the drunkard, venesection is the sheet anchor in the first stage, to be supplemented with reasonable stimulation.

And I would give stimulants as stimulants, and not as food.

Likewise would I argue in those cases where, from broken down or debilitated constitutions, the circulation and the heart are weak.

In any case of bleeding, in disease or surgical operations, stimulants counteract the shock of the operation and, on that principle, I would stimulate, yet not desist from an operation necessary alike in the weak as in the strong.

There is a disposition of late to hedge in on the modern plan of heart sedation with strychnia, a powerful heart tonic.

This is one remedy which I have thoroughly tested and found that, when the heart once begins to fail, strychnia will not revitalize it.

Those who use strychnia use it as a secondary remedy, and correctly so, but it is not thought of as a remedy in the first stage and, if used in first stage, would not counteract the depressing effects of a surcharged and poisoned circulation.

When that condition supervenes known as a "failure of the vital forces," which is heart paralysis, or which produces heart paralysis, the patient has turned the corner. The heart continues to act for some days, but, in spite of heart stimulants, it gradually and surely fails.

But what will you do with Bennett's statistics? is asked.

Among other similar hospital reports Bennett gives the following from Dr. Dietl of Vienna:

	VENESECTION.	TARTAR EMETIC.	EXPECTANT.
Cured - -	68	84	175
Died - - -	17	22	14
No. of Cases	85	106	189
Deaths -	1 in 5	1 in 5.22	1 in 13½

The explanation of this is very simple.

It would be a very careless or a foolish physician who would treat all cases alike, and surely it was the milder cases that were treated on the expectant plan and the more severe ones by venesection or tartar emetic.

The mild cases ought all recover, barring an accident, and one death in 13½ cases under the diet and expectant plan is therefore a high rate of mortality and goes to prove what I have claimed all along, namely, that many cases are more severe than they seem to be.

A reasonable venesection could no more injure a mild case than could a nose bleed or bleeding from an accident in a well person, and thus I reason that, had the more severe or robust of the patients treated on the expectant plan been bled, the rate of mortality under it would have been far less than one in 13½.

On the other hand, it was surely the more severe cases that were treated on the more heroic plan, and, when we select the worst cases and encounter a mortality rate of no more than one in five, we have made a good record.

The fact is that the treatment here was on the antiphlogistic plan, which is to bleed such cases as seemed to require it, and the mortality rate taken altogether, just as cases are treated in general practice under the antiphlogistic plan, is 53 in 380, or about 1 in 7.

Now consider that in bad epidemics of pneumonia, since heroic treatment has been abandoned, the mortality rate is alarming under the prevailing plans of heart sedation and expectancy, and consider again that we are better able to differentiate between cases now than formerly, and that those who favor heroic measures resort to them more rationally and in connection with other improved methods, and we should expect a much reduced rate of mortality.

Besides, as previously stated, Bennett's statistics

were confined to too short a period of time to be of any real value.

RULES GOVERNING THE PRACTICE OF PHLEBOTOMY.

Before proceeding to consider some rules to be observed in the practice of venesection, let me caution the reader against the thought that I am making any special effort to point out only special cases that require the remedy, for I have quite thoroughly resolved to make the practice general and to urge it as very generally the safest remedy in pneumonia.

Besides, if one pauses in an attempt to make up his mind as to whether or no he will bleed, he is apt, in this day of expectancy, to yield to the negative side of the question. And my experience is that there is something about the disease that deceives us in the negative direction, and, if we make a mistake here, it will be in deciding not to bleed. The patient might get along without it, but he will also get along as well with it in all cases and better in many cases.

It is best to make a general rule to bleed every case. Let this be the state of the mind, and yield the other way only when convinced that the case would be injured by it.

Then you are in a safe mental condition on the subject.

It is, however, essential that something be said in the way of explaining in what variety of cases venesection is most urgently needed, the time to bleed, the amount to be taken, the kind of subjects, age, etc., and, as the author views the subject from his own experience and from the experience of men who have practiced the art extensively, we will now consider some of the more important rules.

The best time to bleed is at the early stage, during the first twenty-four hours if possible, but we should not neglect the proceedure simply because a little late.

The kind of cases that require bleeding is the next consideration.

All strong and full-blooded patients should be bled, and we should never neglect it simply because the patient is a little delicate. We are not going to kill any one by reasonable bleeding, and as a rule the patient is not bled enough.

Do not wait to find a red-faced, 200-pounder, but bleed all colors and conditions if other symptoms, to be named directly, indicate it, regardless of age, sex, weight or temperament, and you will be on the safer ground.

The amount of blood to be taken should be proportional to weight generally; 20 ounces in subjects weighing 150 pounds and 24 ounces for 180 pounds would be good average bleeding in an ordinary case where the symptoms are not urgent, but not enough in an urgent case. Where breathing is oppressed, or where pain is great, or where there is cyanosis, or where there appears to be considerable congestion about the chest or head, one or both, it will be found necessary to take an ounce of blood for every five pounds of weight. This will relax the patient completely and the symptoms will subside.

To one who has practiced venesection to any extent it is amusiug to note the modesty of even great authors on this subject.

Bartholow (in his text book on the practice of medicine) recommends bleeding an adult from two to eight ounces. Any experienced phlebotomist would only laugh at this exhibition of modesty. The professor is a

great author, but on this special subject is not informed. This may be seen on reading his article on pneumonia, for here he tells us that he has been an opponent to phlebotomy, but that a wider experience had changed his mind. He thus evidently had not practiced venesection to any great extent, and thus could not be expected to speak authoritatively. With a 1 before his 2 and a 2 before his eight (12 to 28 ounces) he would have come nearer the bull's-eye.

Some have adopted the rule to bleed until the patient breaks out in a sweat, but I have seen the perspiration pour from the excitement of the preparation for the operation, and the patient will not be relieved by perspiration alone.

Others bleed until the patient turns white about the face or faints, but this, too, may result from excitement.

The best general rule as regards the amount of blood to be taken is according to the weight of the patient, varying it, of course, according to circumstances.

Next to this general rule is that the urgent symptoms, on account of which venesection is resorted to, must be relieved. Bleed according to weight, and then on gradually and carefully or repeatedly until symptoms subside is the best rule; until pain subsides, if pain has been present; until he breathes easier, if for dyspnoea; or until pulse grows weaker, where it has been strong.

The cases where blood-letting is indispensable, almost, or that require it more urgently, are:

First, when pain is severe in side or head, one or both. Bleed until relieved.

Second, where lung seems to be congested, with oppressive breathing and lividity of face or lips. Bleed for results.

Third, where there is a dizzy feeling of head, or dusky, diffused redness of face.

While I have stated the three most urgent variety of cases that need venesection most urgently, I hope the practitioner will not be misled here. If he waits to find these kind of cases he will fail to bleed in many cases where it should be done. I mention them only as most urgent, and all symptoms may be in abeyance in a very bad case. Do not be deceived.

Some say bleed only when the temperature is high; but I have seen cases apparently mild with temperature not over 102 F at any time, yet terminate fatally on expectant plan of treatment. Temperature is no guide. And just so with other symptoms. They all may be masked, and so much so that the diagnosis is uncertain, and in all masked cases venesection is especially indicated. Do not wait for diagnosis, but bleed, and then you can better diagnose.

Look out for masked pneumonia. It is not always masked so much by the physical manifestations as by other prevailing ailments. The physician is often masked, rather than the case, or as much so.

For instance, suppose that typhoid fever prevails in the neighborhood or in the family where a case of pneumonia occurs, and suppose that winter cholera prevails and that the patient has had it. Then suppose that the patient has felt languid and sluggish for some days, when you are called and find the patient with a high fever, no urgent pneumonic symptoms and everybody expecting typhoid. You may consider the fever rather high for the first day of illness, but we are not always called on the first day of fever.

I was slipped up on in just such a case. I put it on

treatment for typhoid, cold bathing principally, and the patient continued to grow worse without a chest symptom externally manifested. The case choked down from lung engorgement and heart failure.

I might have diagnosed this special case by the early high morning temperature, a bilious tongue, a dizzy feeling of the head and clouded mind, which symptoms were present, or possibly by a close chest inspection.

Do not be deterred because the patient is of small frame, because tall and slender, nor because of sex or complexion.

Bleed for results and for relief, regardless of all things, except age; and only the very young and the very old need be exempted.

I cannot understand, indeed, why even the young should be exempted, and I have seen very old persons relieved by venesection.

Reynolds, in his "System of Medicine," cites Andral and Frank as authorities for the position that "no age nor period of the disease contraindicates venesection."

This citation is very important, because it is from authorities that speak from experience.

More than likely these authors are about the only ones who have tested venesection in children, and they are the only ones who have a right to speak, therefore.

What can these modern authors (who have never bled children, and yet who cry out against it,) know about it? Absolutely nothing. They are only voicing a universal prejudice.

Suppose that a child with pneumonia should have a severe nosebleed (followed by apparent relief, as I have seen), or should cut its foot and bleed freely, would you imagine that your case is at once hopeless?

Yet such is the logical inference from the seeming great concern felt by some authors who advise us by no means to bleed a child, yet who never bleed anyone themselves.

What is the reason that a child should not be bled, if an older person should be?

The only reason the author has for exempting children is that of inconvenience.

After much observation and experience the author has come to the conclusion that, if the physician attempts to draw fine distinctions between cases to determine whether or not he should bleed a patient in pneumonia, he will too frequently yield to the negative side of the question because of the deceptive character of the ailment, and that, since any case may be bled once in reasonable quantity with absolute impunity, the safest plan is to perform venesection in every case, except patients too young for the proceedure.

Another consideration is whether a single or repeated venesection should be resorted to.

Except in special cases of extreme congestion, and even here generally, only a single and sufficient venesection should be resorted to, and that in as early a stage as possible, repeated once during the next twenty-four hours if case is not relieved.

What is wanted is a decided impression at the start.

Under the old plan venesection was not only the sheet anchor, but it was pushed to extremes and repeated often, and, in Bennett's criticism, he points out that patients were often bled more than four pints during the progress of the case, which together with

other depressing methods was followed by a mortality of about one in seven.

The results may be expected to be quite different when we bleed only once and early to get an early impression on the disease and at the same time hedge in with opium, ergot and other remedies to then equalize the circulation and prevent further extravasation.

This is a most important point and experience and reason has convinced me that early single, yet sufficient bleeding, followed by remedies calculated to quiet and equalize the circulation, will give wonderfully good results as compared to the older method.

If the case is seen later, of course bleed once and proceed the same way, otherwise if venesection is at all indicated, or if it has been indicated and the symptoms have not materially subsided, which is to be determined, not by the apparent strength of the patient, but by the pulse and temperature, or more particularly the temperature.

The case is not under control until the temperature is declining.

OTHER ANTIPHLOGISTIC REMEDIES IN PNEUMONIA.

The next remedy in importance in heroism is calomel in large doses—to adults ten or more grains at first dose and repeated in smaller doses every three hours until good results are obtained.

Calomel not only removes pressure from lungs by depleting a congested liver but arouses the secretions and stimulates the emunctories generally, thus removing effete products from the circulation and reducing fever. It is thus the most potent febrifuge. It does

not mask the symptoms and at the same time leave the poisons in the blood, but it removes the poisons, when the symptoms subside naturally. While it makes the patient feel weak at the time, the physiological processes are left more free to go on, unimpeded by the pressure of effete matter, and the heart is left strong.

If you do not bleed, push the calomel treatment in first stage and give tartar emetic also. If you do bleed, administer it rather freely anyway.

Calomel treatment is especially adapted to the treatment of small children and is here generally sufficient in the line of heroism; but in the adult it is often not a sufficient antiphlogistic alone and had better be supplemented with tartar emetic when venesection is not resorted to.

Tartar emetic, unless in very small doses and in severe cases, is not suitable for young children.

Calomel is the greatest of all medicinal derivatives. It takes determination away from the head, as well as from the lungs, and thus prevents the development of brain symptoms that so often arise in the course of pneumonia and that not infrequently carry off the patient.

Next in importance is tartar emetic in one-third grain doses every three hours during first two or three days of illness or until symptoms abate.

This is a very powerful remedy and belongs to the antiphlogistic class, and, while Bennett has so strongly opposed antiphlogisticism, this is his remedy.

If any remedy is calculated to take the place of venesection it is tartar emetic, for it weakens the pa-

tient, reduces the fever and softens the pulse as no other medicine will, and is free from danger used rationally and in adults or any but small children.

In condemning venesection on account of its depressing nature, and prescribing tartar emetic in large doses, it seems that Bennett preaches a theory contrary to that on which he practices; but he succeeded well with the remedy.

Tartar emetic needs to be given but a few days and should be discontinued soon as fever declines. Two, three or four days is long enough to keep a patient under tartar emetic.

The blister is of less importance than venesection, tartar emetic and calomel. It is really only a secondary remedy. It is a useful counter-irritant to relieve pain in first stage and later on is supposed to encourage resolution. It is also valuable in the head complication applied to back of head, but this complication seldom arises after venesection and a course of calomel, which should be borne in mind always.

A most valuable febrifuge and one that is free from danger is muriate of ammonia in 30 to 60 grain doses every three hours. It may be given in place of tartar emetic in debiliated subjects.

THE NEW AND MOST NATURAL ANTIPHLOGISTIC AND NON-MEDICAL ANTIPYRETIC. "CHEST CHILLING."

And now we must consider the greatest antiphlogistic method of treatment for pneumonia, cold water or "chest chilling."

Though new to the most of us, the plan has been sufficiently tested in the hospitals to demonstrate its virtues, and under it the mortality rate has been greatly reduced.

But it must be noted that the comparison made is between it and the prevailing methods, excluding venesection.

It must be remembered that chest chilling is a general continuous treatment, while venesection, as now advocated, is not as a general treatment but a treatment for primary stage.

The method failed in some cases and it is barely possible that venesection would have saved the cases that were lost.

It may reasonably be objected that, however successful the cold water plan may prove generally, since pneumonia is a disease that manifests itself so variously, no general plan can fully meet the indications of all cases, and that therefore venesection, calomel, etc., may yet be the proper remedies for special cases.

The advocates of venesection and calomel may consistently hail the water treatment with gladness, therefore, not because they see in it an enemy, but because it is a friend that also antagonizes prevailing general treatment.

I think that it may be said in truth of the water treatment that it is superior to expectancy, because it is a vigorous do something plan, that it is far superior to drug depression and drug heart sedation, because free from poisonous elements that interfere with physiological processes, that it is far superior to medical antipyretics, because it is a natural febrifuge —all of which is proven by the lessened mortali y following it—but that there is nothing in it negating the utility of venesection, calomel, etc., in the special class of cases where these remedies are indicated.

Let it be urged and adopted as a general treatment to supplant the prevailing methods, but let it not "claim the earth," for under it or under any other general plan there will be met cases where venesection and calomel or a course of tartar emetic cannot be safely ignored.

While the water plan does not need my endorsement to give it merit, I am glad to say that I have tested it sufficiently to believe that it is far superior to the old plan of hot fomentations, *except in cases of pleuritic complications.*

THE ADVANTAGES OF HEROIC MEASURES IN PNEUMONIA, ESPECIALLY VENESECTION AND FULL DOSES OF CALOMEL.

Venesection and large doses of calomel are called "heroic" treatment, while treatment with deadly poisons such as aconite and coal tar derivatives is considered mild.

The reverse is certainly true.

Indeed the argument for venesection and calomel is that they are harmless in the hands of any but a fool.

Again the so called "heroic" treatment is simple and exact. We know exactly what to expect from venesection and calomel, not so with prevailing drug treatment.

With venesection and calomel we get just what we go after, and when these means are resorted to in time and intelligently, with the use of appropriate adjuvants, recovery is almost certain, in even the desperate cases, while when they are not resorted to all bad cases are in danger and often prove fatal.

True a great majority of cases may be treated successfully with other means and many cases require no attention at all, but the safe plan, and especially to obviate the head symptoms with which so many die, is to resort, at an early stage, to the means that are a sure bar against serious complications.

It is only in an occasional season that we meet with many of the more severe forms of pneumonia, and many physicians, who have practiced only a few years, imagine that the heroic treatment has had its day. I experienced an epidemic of severe pneumonia about thirty years ago, again about ten years later, again in 1884 and not again until the winter of 1898-9.

Bartholow, in his last text book and in an article on pneumonia, says that while he has been inclined to oppose venesection and calomel, "a wider experience has convinced him of his error," or words to this effect, and he gives first consideration to these remedies in the order named and as the most important means of relief for this disease.

Osler, in his new work on practice, recommends venesection in pneumonia, but I believe in no other internal inflammations. He favors it for plethoric subjects but does not urge it strongly as a general treatment.

Freedom from dangerous effects is one of the strongest arguments for the use of a means of cure. When we bleed a patient and give calomel we can go home and sleep easily. Not so with aconite, chloral, opium and poisonous drugs generally, the effects of which must be watched with the greatest care.

Nor will the nurse make any serious mistake when venesection, calomel and the blister are the remedies;

but I have known many a poor sufferer to be laid out with poisonous drugs.

What is venesection more than a nosebleed or the loss of blood from an accident? And all of us know that mercury is a favorite remedy for tubercular and lung symptoms.

The loss of blood simply as a means of relief in a single instance cannot tend to produce chronic lung trouble, and mercury tends to prevent lung disease; but a continued irritation from the pressure of congestion and a torpid liver is surely more likely to produce chronic lung disease.

But the greatest argument for venesection and calomel treatment is that it aborts many cases and renders them all mild, and the patient is soon on his feet, while prolonged illness and dangerous sequalæ are more apt to follow the modern prevailing treatment.

One of the greatest errors, both professional and popular, is that the older method of heroism is more depressing and wearing on the system than the drug treatment of to-day—the prevailing plan.

Bartholow says that, according to the old practitioners and to Sir James Paget, the patient very soon recovers from the effects of venesection; that the depression from it is only temporary. He calls attention to this in reply to the argument that venesection is too depressing a remedy; and we all know, on a moment's reflection, that a single blood-letting is soon recovered from.

It depresses for the moment; but I have learned that this is just what is required. Pneumonia is a disease that must be controlled and gotten under subjection on the start. If this is not done it will simply run its natural course, the milder cases recovering and the more

severe ones dying, whatever the palliating means of relief resorted to.

Therefore, both as regards the dangers from the immediate attack and from possible serious sequalæ, the antiphlogistic plan of treatment is superior to the prevailing one.

HEAD SYMPTOMS IN PNEUMONIA.

Something remains to be said about the treatment of head symptoms in pneumonia, and especially because they are various and to be relieved by different means.

And, firstly, they are not likely to arise after venesection and a course of calomel.

These symptoms manifest themselves in about four varieties.

The first is an active congestion, with severe pain occurring in the early stage of congestive cases.

Venesection is the sheet anchor in such cases.

The second variety begins with a dizzy, full feeling about the head in the early stage, but no pain, and soon develops into muttering delirium of a quiet form.

The brain is simply clouded by the presence of surcharged blood, but there is no meningitis nor inflammatory symptoms. The patient will talk and pick at the bed clothes in a half-sleep condition, and occasionally he will open the eyes and stare at a given point. It is very much like the head symptoms of typhoid fever, that often occur at about the same stage–first few days—but the similarity is oniy in some of the outward appearances.

In typhoid the eyes are a little injected. Not so in pneumonia; the eye will not stare as in pneumonia, and the delirium is more consistent. In typhoid, if you

wake the patient at all, he is momentarily clear, while in pneumonia he will talk both rationally and irrationally at the same time.

In typhoid the condition is meningeal irritation; in pneumonia, clouding of the brain by deoxygenized blood. The former is generally relieved by water-pouring (over head); the latter is injured by this means, but yields to venesection and a course of calomel, one or both.

A blister might relieve it in typhoid, but not in pneumonia.

The third variety is a wild delirium, with a dilated pupil. It is meningeal irritation, and I have relieved many such with a blister to back of head and neck. Venesection and water-pouring will also relieve it.

The fourth variety, occurring later, is similar to the third, but approaches insidiously and is preceded or accompanied by a slight rise of temperature. The first objective symptom is often a remark a little out of place, yet the patient declares he is better.

As this condition progresses the pulse grows a little faster and weaker, the patient becomes restless and obstinate, looks out at the window or around as if looking for something, spits in the wrong place, takes hold of things and holds fast, laughs and curses alternately and is difficult to please or to manage, continually claiming that he is better, if asked how he feels.

Of course this condition is seldom, if ever, relieved. It does not occur if the case is controlled during first stage.

There may be some meningeal irritation here, but it is about the way all cases terminate when not relieved.

The genearal cause is carbonic acid poisoning and heart failure, and, being the regular sequel in cases not

relieved, it is not to be relieved short of treatment adapted to the primary stage.

I am not fortunate enough to have relieved a case so far advanced, but have known venesection to relieve cases that had advanced nearly so far, and since no other remedies will relieve, and since it is a matter of kill or cure, I shall bleed the next case where I see the condition approaching; that is, should I not be called until as late a stage.

I have not so far resorted to venesection in advanced stages of the disease, and the profession universally condemn it, here more especially; but, as I have changed my mind and my plan on so many other points in connection with the treatment of pneumonia, I shall carry the lance until the patient is either dead or convalescent.

As elsewhere referred to, Andral and Frank say that it is never too late to bleed in pneumonia, and they spoke from experience.

A DISTINCTION.

The proper distinction must be drawn between primary and secondary treatment in pneumonia, and more particularly in this than in many other ailments.

Primary treatment is what I have been considering mainly, and by that is meant the chief reliance in the first stage.

Expectorants, sedatives and anodynes, tonics, stimulants and remedies for special symptoms and complications are to be considered alike under all methods of primary treatment.

The expectant plan of treatment, followed by so many, consists generally in the use of secondary remedies only.

As to primary treatment, the distinction between the old and the modern (or prevailing) plans is that the former relies on venesection, tartar emetic and colomel; the latter on aconite, veratrum and various heart sedatives to control the active stage of the disease, and what, in the main, is to be decided in this investigation is to determine the superior plan.

Both these plans are really heroic, but the modern one has been so interwoven with "expectancy" and with special fads that one is liable not to draw the proper distinction.

A second distinction is to be drawn, of course, as to whether primary or secondary treatment is required in each individual case, or whether "heroism" or "expectancy" is to prevail.

The conclusion of the author is that secondary, or "expectant," treatment is better adapted to infants and small children (excepting that they should have calomel freely), but that heroic treatment is necessary in most all other subjects, and that as between the plans of heroism, ancient or modern, the former is awarded the palm by the crucial test of experience and by every consideration of scientific enquiry.

And let it be added that the cold water treatment, whether considered primary or secondary, is adapted alike to young and old, and, if it continues to prove as efficacious as it so far has, there will perhaps be less dispute in the future over drugs at any rate.

If the water treatment is to be relied on, I am satisfied, from experiments made with it, that it must be pushed vigorously. By applying a large towel around the chest, wrung out of cold water, and repeated every five or ten minutes, I have seen the most happy effect.

It soon begins to feel comfortable and the fever begins to fall magically.

It will not work half way or reluctantly used. In two children, 9 and 11 years old, where the plan was followed fearlessly it worked like a charm; in two others where the parents were afraid to apply it, I was compelled to change off to warm poultice.

In the case of a lady 56 years old, with double masked pneumonia and progressing insidiously, I started in with the cold pack instead of venesection. The patient was not suffering much pain—pulse, 104; temperature, never over 102. Applied cold towel, and in 24 hours fever dropped to 101. Family objected to treatment and I permitted them to substitute warm poultice, because the fever was no higher all along. In twenty-four hours fever was back to 102, where it remained until eighth day, when it went to 103; pulse 120, and the patient sank from heart paralysis.

This patient had taken a good course of calomel during first two days.

It was in a rather enfeebled constitution, yet not apparently so, and seemed to run so smoothly that I did not resort to very heroic means in the first stage.

I took it to be a case for the cold pack or chest chilling plan, and, after watching the progress of the case, I am satisfied that it would have proven a success but for the interference of a nurse who knew more than I did.

I shall adopt it in lieu of the hot poultice, as a regular secondary remedy, but shall not forget my lance and other primary remedies in cases where heroic measures are indicated. And I repeat

that pneumonia is a most deceptive disease and your case is generally worse than you think it is.

RECAPITULATION OF SOME IMPORTANT POINTS IN CONNECTION WITH PNEUMONIA AND ITS TREATMENT.

1. It is the most deceptive of all diseases, the main deception being that, in adults especially, the case is often more grave than the physician at first imagines. Make no rash promises.

There is no regular connection between the outward symptoms and the gravity of the case.

3. The danger from heart failure or paralysis is greater in pneumonia than in any other acute febrile disease.

Threatened heart failure is harder to overcome or meet than the same symptom in other diseases; venesection is the most certain safeguard against it, and, when it is threatened in later stage, no remedy will stay it, save perhaps venesection.

Heart sedatives and depressants, such as aconite and antipyrene, mask the symptoms without relieving the pathological conditions, weaken the heart, the organ most liable to fail, and are the most irrational remedies that can be thought of in the treutment of pneumonia.

5. Though a high temperature is ominous of evil in pneumonia, a case with low temperature is not free from danger. A case is not under control until the temperature begins to decline, a fall of temperature from drug treatment is only deceptive, and a rise of temperature in latter stage of the disease means certain dissolution, except at crisis, when it should quickly decline again. The rise of temperature sud-

denly at crisis should have been preceded by a considerable decline, otherwise it portends evil and not good.

6. A high pulse rate is more dangerous in pneumonia than in typhoid fever, and a weak heart will not yield to stimulants in the former as it will in the latter ailment.

7. Stimulants are contraindicated in early stage of pneumonia unless supplemented by venesection. In drunkards both should go together.

8. Venesection, timely and in reason performed, will injure no case, whatever the strength, size, complexion, constitutional condition or complication—no more so than a nosebleed.

9. Venesection is the most certain abortive measure known in the treatment of pneumonia, and attempted abortion should be the first consideration.

10. As a treatment venesection is exact and free from the unknown quantity of variety and from the dangers incident to drug depression and heart sedation.

11. Venesection is the only remedy that, with any degree of certainty, will relieve the condition known as carbonic acid poisoning.

12. Cold water is the most rational external application in pneumonia, but it will not take the place of venesection and calomel in cases demanding immediate relief on account of congestion or where the blood is surcharged with carbonic acid and effete products; nor will it relieve the head symptoms caused by the same condition of the blood.

13. The breathing of cold air, though not argued in our general treatise, is of great utility in pneumonia, and in infants will alone relieve cases seemingly hopeless.

14. Be on guard against "masked" pneumonia, and bleed whether diagnosis is certain or not.

CHAPTER V.

CEREBRO-SPINAL MENINGITIS.

Twenty-eight years ago I practiced through an epidemic of cerebro-spinal meningitis and lost a great majority of my cases, as did also other physicians about me with whom I came in contact.

The treatment adopted was exclusively drug, but of course in great variety. I do not remember that a single case was treated with cold water or ice or that venesection was ever resorted to. I do not think these means were as much as mentioned.

In conversation with an old citizen some time afterwards, and in reference to the scourge, he said: "Doctor, I remember that old Dr. —— used to bleed everybody when they had a pain in the head. I don't know if they had this disease, but people often had bad head trouble and they would bleed them."

"Did it do any good?"

"Why, yes, of course it did. It most always cured them, if it wasn't too late."

I practiced a great many years before I experienced another epidemic of this disease, but in the meantime, and during a practice of thirty years, I took observations from every available source with the view of procuring data on the subject. In treating various diseases and cases one will often hear people refer to similar cases that they have seen, and we all know how prone they are to inform us of the treatment

given by "old Dr. ——," especially in cases where the old doctor was the family physician of the narrator. From this source the susceptible physician gains some very useful information, and it serves a telescope, as it were, through which he is enabled to peer far into the past.

More frequently do we hear, too, (and more eagerly told,) of cases where the patients were given up to die and how some neighbor or friend happened to think of something never dreamed of by the physician, and how the patients then recovered.

And just such cases do occur, but, if looked into closely, it will be found that the old lady or neighbor, who violated the doctor's instructions, procured the information from some physician who had used the same means successfully in time past.

I will relate a few instances.

A gentleman once told me, for instance, of two cases of cerebro-spinal meningitis that were given up to die by two good doctors, whereupon the father of the children put ice around them and they got well. Another told me of a case of the same disease in the person of his brother, where the doctor gave him up, but he began applying ice to his back and head and he recovered, to the surprise of the doctor, yet he remained an imbecile. (Well authenticated).

Another well authenticated case coming through a similar source and occuring fifty years ago was a case of "bad fever in the head," occuring in the winter time and where the attending physician put the patient in a spring wagon and took him half a mile to a creek where there was a waterfall, under which he

held the patient's head for some time, and by this means cured him.

But more especially and more frequently, and going usually further back, have I heard older persons refer to the old habit of bleeding for head troubles and express surprise that the doctors had "quit it." Time and time again have I heard older persons, when referring to some one who had died of meningitis, say, "I believe he would have gotten well if they had bled him."

From experience I came to learn that when head symptoms develop in typhoid fever, water poured over the head early would relieve the symptom invariably and absolutely. I had also learned that venesection would do the same thing in typhoid, pneumonia or any other ailment.

I next reflected that cerebro-spinal meningitis (the active form) is really only a rapid and powerful determination of blood to the head, the same thing that we have, in a milder form, in other ailments, as in typhoid fever, and concluded that what would relieve one would relieve another form of the same disorder.

Therefore when cerebro-spinal meningitis appeared in 1898 9 I resolved that if a case fell under my care I would resort to heroic measure.

The opportunity came, and I will present the clinic exactly as it ran.

One clinic does not establish much, generally, but in the case here presented and the means resorted to, together with the fact that I have relieved more than a hundred cases of meningitis and brain fever occuring in the course of other diseases by the same means, I believe

I have much more than I would have in an isolated case under drug treatment.

It is more particularly in drug treatment of disease that we hold to the rule that "one swallow does not make a summer," and this because we do not know whether nature or the remedy is responsible for results.

I hold it to be vastly different in the use of appliances where one can see and trace so clearly the effects of the treatment. It is here more like it is in surgery, where we can see at once that ligation of a vessel arrests hemorrhage. We see the symptoms come and go as we apply or withhold the remedy—water—and we see the blood leave the injected eye and the symptoms vanish when we perform venesection.

So that, all things considered, I present the clinic with implicit confidence in the means of relief resorted to, with such happy results.

A TYPICAL CASE OF CEREBRO-SPINAL MENINGITIS, WITH COMMENTS.

W. T. Robust, farmer, 20 years old, weight 150 pounds. Had been afflicted with aching, or soreness, in back of neck and shoulders for some weeks, and had headache two days previous to attack, but said nothing about latter symptom and was eating his meals heartily. Ate a very little supper, however, on Wednesday evening, and said he did not feel well—ached all over—and lay down on lounge in room next to dining room.

His father finished supper in a few minutes, and on going in next room to ask son how he felt, found him unconscious and pulling his hair.

I saw patient two hours later and found him seemingly semi-conscious and screaming out occasionally,

with pain in head. Pulse 40 to 80, beating slow a few beats and then faster a few beats.

The veins of the whites of the eyes were injected; tongue foul.

I got the patient's head over a tub of ice water at edge of bed and began to pour it over his head with pint cup, letting it fall from an elevation of from two to twelve inches directly on base of brain.

Patient dropped to sleep in a few minutes, but I continued the water pouring forty-five minutes, when he lay back on bed much relieved, and pulse was beating seventy per minute.

"Where is your pain?" "In head."

He occasionally screamed "Auch!"

"What hurts you?" "My head." "Where and how?"

Placing hand at base of brain he said, "Right here is the center and it feels like needles shooting out once in a while from here up into my head and right down my backbone."

Administered good dose of calomel and Dovers powders at once and began giving mixture of bromide and chloral. Gave two more doses of calomel during next twelve hours and considerable chloral mixture, but owing to sickness of stomach had to discontinue all medicine per stomach, but administered morphine hypodermically in moderate doses.

Continued to pour water over his head at intervals and as pain returned, and patient seemed considerably relieved and slept twice two hours at time on Thursday night.

On Friday at noon the treatment did not seem to be controlling the case sufficiently, eyes yet injected

and patient suffering intensely at times. Intervals of relief were shorter.

I now opened vein of arm and took full thirty ounces of blood—pint cup twice full, nearly brimming—when patient fainted away.

The eyes cleared up at once, patient complained of being very weak and said everything looked dark.

He seemed exhausted, but never complained of pain in head afterward.

After this, ice water tried once to head seemed to hurt, and I used a little tepid water at times.

The calomel, assisted by injections, had acted before blood letting.

Stomach remained very irritable, paining and burning with vomiting, but yielded to creosote and charcoal, soda and water.

On Saturday morning at 8 o'clock patient became entirely rational and asked what had been the matter.

He had been conscious at times, but in only a dreamy way, and now said he remembered nothing.

I kept him under a little opium for a few days to keep nerves quiet and, with a light diet, (not forced on but withheld,) gradually increased, recovery was uninterrupted, mind remaining clear.

It may be noted particularly the effect of water and of pain in head upon the nervous system.

At one time, after water had been poured over the head for nearly an hour at a stretch, the patient took a severe chill, rubbing his hands together and his teeth chattering violently.

I took this to be a shock from the water, as it perhaps was. But at another time, after water was applied only a few minutes, the same thing occurred

and we stopped water. This time, however, the eyes remained injected and he screamed out twice in rapid succession with "Oh, my head!" whereupon I applied the water again, and thoroughly, with good results.

Another time, when using water not so cold, he said, "Colder," when ice was added to his comfort.

In fact his actions or suggestions were my guide in the application of water, both as to temperature and as to duration. If not cold enough he indicated it; if not applied long enough he would remain restless, while when the effect was sufficient he either went to sleep or would ask to be allowed to lay back on his pillow and rest.

OBSERVATIONS.

The first thing that will arise here in the mind of the critic is that I have presented but one case and, contrary to all precedent, offer it as basis for a disquisition.

But I reply that I have offered a hundred cases.

Remember that it is a symptom we are considering, and not a disease; and if the physician would treat symptoms more, and differentiate between diseases and vary treatment less, he would meet with better success often.

When we have determination of blood to the head, it matters not what the disease is, the indications for treatment are the same, unmistakably so, and on the same principle that we give anodynes for pain, whatever the ailment.

Besides, and more forcibly true, active rushing cerebro-spinal meningitis is the same thing in all cases, sporadic or epidemic, and whether in the disease specially named as such or in the course of other diseases where

the symptom manifests itself; and, if a given means of relief acts so instantaneously, and under close observation such as satisfies us that the relief is from the remedy, then we have a demonstration that the remedy relieves the symptom.

Why is opium lauded so highly as a remedy for cerebro-spinal meningitis? Simply because it relieves pain and acts just the same in all cases.

Of course, we have all known for a long time that opium relieves pain, and hence we do not hesitate in giving it. Well, so have I known for a long time that cold water and venesection relieve head congestions absolutely and more universally, permanently and quickly than any remedy yet discovered, not excepting opium nor the anesthetics. The authorities know this, especially of venesection, and refer to the "wonderful effect" (as remarked by Hare) of this ancient means of relief.

Why the authorities so generally recognize this means of relief (venesection especially) and yet condemn the practice can be explained only on the assumption of a *habit of thought* growing out of the "blood-letting controversy" and the modern craze for support and forced feeding; but the position is based on neither experience nor rationalism, as it appears to me.

"Facts are before theories," and the facts are that venesection relieves head congestions and, though it weakens for the moment, does not lessen real vitality nor the powers of endurance.

For passive congestion, or after the active stage, the blister is wonderfully efficacious, especially in the head symptoms of typhoid fever, pneumonia and cholera in-

fantum, with strong stimulation in desperate cases of the latter ailment.

To the question, "How much water and how much blood-letting?" I reply: "Just enough to absolutely control the symptom;" and, as the relief of the symptom is the cure of the disease, the *fact* constitutes the rationalism of the treatment.

In that variety of cerebro-spinal meningitis, occuring in epidemics of the disease, where the head symptoms are more or less masked and approach more slowly, in which cases there appear low typhoid symptoms, occuring generally in older subjects and characterized by a sallow skin and frequently by a little yellowness of whites of eyes, vigorous stimulation should supplement phlebotomy and cold water. We have here more of that typhoid, or typhus, condition which calls for strong alcoholic stimulants.

CONTRA.

The first medical journal I picked up after treating the case I have presented contained an editorial by the editor, in which he condemned venesection in cerebro-spinal meningitis and advocated as principal treatment, and as that on which to rely, "expectancy" and a supporting diet from the start.

Of course old "expectancy" comes to the front when there is really no remedy to be had, and of course it is next to impossible for a member of the profession to say much about the treatment of any disease without advocating support. I verily believe that most, or some, of our medical colleges would unhesitatingly grant diplomas to their students if, on

examination, they but readily answer "expectancy" and "support" in reply to all questions.

If you adopt the expectant plan in this disease, you may "expect" the patient to die, and I note that the writer just referred to gives the mortality rate at from 30 to 75 per cent.

And pray, what could we expect from feeding? The patient would die before a meal could be digested, and besides the stomach is exceedingly irritable in this disease and generally rejects everything.

Talk about feeding a patient stricken with violent meningitis and that will be beyond hope in twenty-four to thirty-six hours, unless relieved! The laity should know more than this. It's the height of absurdity.

No disease that comes on with a rush should be fed, and in fact food should be withheld until you get control of the symptoms.

Verily the profession has gone mad on feeding.

Generally the system is surcharged with bilious or effete matter, often as a result of overdieting, and elimination, rather than feeding, is indicated.

In his remarks against venesection the same writer urges that "even wet cupping has been followed by bad results."

True enough, any remedy that falls short of meeting the requirements of the case would be followed by bad results, and what should we expect from a little cupping in a case that demanded the abstraction of two pints of blood or more?

That we cannot bleed young children is no argument against venesection in those cases where it can be performed.

In children I would wet cup at once and thoroughly, but it is a very delicate proceedure (child's play) in an adult, or even a youth, and will fall short.

In small children the disease generally acts very rapidly, and I have been called two miles, and on first manifestation of the disease, to only find the subject dead on my arrival.

In children I would administer a potion of opium and of whisky, administer a little chloroform carefully and abstract blood from the nape of the neck freely with wet cups, then pour water over the head gently and continuously, keeping up opium and whisky carefully and withdrawing chloroform as quickly as possible. Of course fine and careful work is demanded here and only the physician personally can do it.

The argument that venesection is too depressing a remedy to be resorted to in cerebro-spinal meningitis, as claimed by some eminent authorities, is not based in scientific truth, as I view it. On the other hand I claim that blood letting in this disease and in pneumonia is supporting, by relieving the blood pressure from a local congestion in the former and by doing the same thing and by also removing a part of the surcharged blood in the latter.

Only reflect a moment here!

A person is strong, healthy and eating heartily to-day, and to-morrow is attacked with violent head symptoms. What is there about him or within him that tends to depress but the local congestion? What is there to contraindicate venesection but the local congestion, which can be and is instantaneously relieved by it? Bleed to relaxation and what have we left? A patient that feels weak but whose system goes

on performing its functions quietly and undisturbed by the local congestion.

At the same time, while we are thus relieving the local congestion, put the patient under the supporting and quieting influence of opium, the great and only medical specific for this disease, and no fears need be entertained in the direction of depression.

What has frightened men into the belief that venesection is depressing is that, while they have half way performed it, the disease has killed the patient.

The rule should be to bleed until you get results—relaxation—and please reflect that you can bleed spasm and pain clear out of a man in any case, and this is what is needed.

A little bleeding and a little water would be followed by bad results also, but enough brings returns better than 75 per cent mortality.

I have found that timidity in the use of the lancet and cold water has led many men to abandon remedies that, properly used, would cause a change of opinion.

When I encounter the disease again, if ever, I shall bleed on the spot, (except in small children,) repeat if necessary, and expect to be compelled to use the water less heroically.

The author's *resumme* on cerebro-spinal meningitis is that it is a disease characterized by one special condition—determination of blood to the meninges—and that the condition is counteracted and relieved by a definite and exact treatment—venesection and cold water, supplemented with opium, if necessary, and with stimulants in cases assuming a typhoid form.

No modern author that I know of recommends venesection in cerebro-spinal meningitis. Osler, the latest

work out, says that venesection is not indicated, but I think he has simply caught up the cry against it without much thought on the subject. He, however, recommends the application of ice and cold water. He also falls into the common error (as it appears to me) of support in the way of "nutritious diet."

CHAPTER VI.

TYPHOID FEVER.

About thirty-five years ago, and just before I commenced the study of medicine, I was in attendance at a high school which was broken up by an epidemic of typhoid fever, and I and other members of the family were stricken.

The physician that attended our family treated many cases in the community without losing one, while another physician in the same community, who treated many of the cases from the same school, lost many of his patients.

I remember the treatment of the successful physician to have been, in part, immediate checking of the bowels, ice to head and bowels, and starvation—prohibiting sweet milk emphatically.

I remember that some of the patients of the unsuccessful physician died of hemorrhage from the bowels.

I did not realize at the time that the result in each case depended on treatment, but supposed it to have been merely accidental or because some cases were more severe than others. On this point I have changed my opinion, as will appear as we proceed.

The memory of this epidemic almost passed out of my mind as I went on in the study and practice of medicine, until experience and observation brought it clearly back.

In the college lectures and text books I learned more of the medical remedies for typhoid fever than of diet

and other non-medical means that are of so much value in the treatment of the disease, and when I began practice I relied a little too much on the former and not quite enough on the latter.

And, as it so transpired, the members of the fraternity with whom I came in contact during my first years of practice, had no special plan of treatment for this disease—treated it variously and, as I now believe, relied also too much on medicines and not enough on systematic dieting and external appliances.

Practicing in a malarial district, too, as I was, the people expected the doctor to break up a fever very quickly, and the physicians were much in the habit of administering anti-malarial remedies, even when and after a diagnosis of typhoid; in fact, typhoid and malaria were frequently associated.

I, however, very early adopted the expectant plan in this disease and met with fair success. I was careful about diet, checked the bowels moderately, gave turpentine emulsion and no strong medicines, and in this course have been reasonably successful through many years.

However, I have been to some extent and at times pulled away from old methods by the modern craze for feeding, by the claims of some writers that the bowels should move daily in the course of typhoid fever, and by the many reputed specifics offered from time to time for the cure of a disease that is not cured with medicines.

The silly and chaotic state of the medical, or professional, mind in recent times on the management of typhoid fever is seen in the quinine craze of only about twenty years ago, originating in Germany, and consisting in an attempt to abort typhoid fever with a single dose of quinine containing from 75 to 100 grains.

The mortality following this plan caused it to be soon abandoned; yet I presume it was only the knock-down argument that abated the craze, for, as it seems to me, only a half witted man would grab at such a straw.

More recently have been offered such specifics as iodine and carbolic acid, arsenite of copper, sulpho-carbolate of zinc, etc., as bowel disinfectants, various antipyretics and remedies to meet all variety of cases and conditions, while none of the inventors seem ever to reflect that no antiseptic can counteract the evil effects of a careless diet, and that no artificial antipyretic can take the place of water.

Few there are who reflect that antisepsis, either internal or external, is not a remedy, *per se*, in typhoid fever; that at most it is but of trifling importance. As pure air is better than foul, and clean garments better than dirty, so antiseptic precautions and remedies are preferable to filth and carelessness; but, unless your case is managed properly otherwise, they are of no value. This is proven by the fact that typhoid patients will recover in illy ventilated apartments, with bad sanitary regulations and with never a change of linen, if treated otherwise properly, while with wrong treatment otherwise the mortality will be great, notwithstanding the patients may have pure air, the most scientific sanitary surroundings, a daily change of linen and any form of internal antiseptic medication.

I will treat typhoid fever in the hovels of the poor, in crowded tenements and with two patients to the bed and let them drink the same water from which they contracted the disease; I will bathe them in water, not to cleanse them but to cool the fever and refresh the nerve centers, check the bowels on the

spot and cut off the diet, except water orange or currant juice and whisky in particular cases, (see further on) and cure my cases; while you may treat the aristocracy, have the advantage of all modern antisepsis and, if you will let the bowels run, use cold water sparingly and feed your patients on milk, you will make a bad reputation as a physician and be of great pecuniary benefit to the undertaker.

Antiseptics and specifics such as are in vogue are not remedies for typhoid fever. The disease consists of an inflammation and its consequences. The cure is the suppression of the inflammation primarily and in counteracting its consequences incidentally—which see as we proceed.

It seems to me that the profession have of late years gone mad on "specifics" and disinfectants in typhoid fever and, in consequence, are neglecting established and, as it appears to me, more rational treatment.

Sulpho-carbolate of zinc, carbolized iodine, arsenite of copper, etc., are offered as specifics and on the theory of antiseptics, of course.

Experience has shown them to be worthless, and the slightest reflection teaches us why.

They cannot quiet the inflamed parts as does opium and are not to be compared with turpentine as remedies for tympanites.

Their only possible efficacy is as disinfectants and, when we reflect that the lining of the bowel can not be washed off and done up like an external sore or wound, we see at once the uselessness of disinfectants.

But there is one specific which acts on a different principle, and experience teaches us that it is superior to all others as a disinfectant. This is nitrate of silver.

Let us reason out its mode of action.

What we have is a condition of inflammation of the lining of the bowel, and what is required is to dry up this condition.

Nitrate of silver, on mixing with the fluid contents of the bowel, and whatever the character of these contents, foul or otherwise, forms an astringent and stimulating wash on the same principle that it is used in conjunctivitis.

Prof. William Repper, of Philadelphia, speaks in the highest terms of nitrate of silver—having treated 100 cases successfully with the remedy.

The proper dose for an adult is one-sixth to one-fourth grain, made into pill with gum accasia, every six hours, made fresh every day, and continued throughout the course of the fever.

The profession have, from some cause, also gone mad on support and feeding in acute febrile diseases. While reflection and science teach us that overeating is the prolific source of many acute diseases, yet when it has produced its result, when the cup begins to run over as it were, when the system is revolting against the oppression of overfeeding, the profession cry "Add fuel to the fire."

Nowhere is this species of modern medical insanity more clearly seen than in the management of typhoid fever. Physicians are feeding typhoid fever patients, when food only ferments and acts as an irritant to the diseased parts, and then attempting to counteract the

injury by means of antiseptic specifics, just as they apply warm poultices over burning surfaces and organs and expect to correct the result of such folly by the administration of poisonous antipyretics.

But the climax of medical insanity is reached in the modern craze of forced milk feeding, which bounds beyond ignorance, and can, in reason, be regarded as nothing short of criminal carelessness.

I have known nurses to administer, under directions from the attending physician, half a gallon, a gallon and over a gallon of sweet milk to a typhoid patient in twenty-four hours, and I have heard both allopath and homeopath actually boast of the quantity of milk they had succeeded in forcing down the unwilling palate of a patient, only to form irritating curds and gases along the tract of a diseased bowel.

It has transpired during the past fifteen years that I have been called in consultation with many physicians in typhoid fever cases (perhaps fifty cases), and I believe that every case that died, except one or two, had drank sweet milk freely, had tympanites and died from bowel hemorrhage. (One or two died from meningitis, and where no attempt was made to keep the head cool.)

I have seen the same thing in some penal institutions and hospitals that I have visited; have heard of it here and there all over the country and have every reason to believe that the habit of dieting on milk in typhoid fever is quite general.

We see it mentioned approvingly in the public press and recommended in that trashy class of literature known as "household physicians."

I think that the carelessness with which milk is exhibited in typhoid grows out of its general endorsement as a proper article of diet in this disease. The doctor is asked if the patient can drink milk, and he replies, "Oh, yes, milk is regarded as the proper diet in typhoid fever," and it is given "ad libitum." The average doctor is, however, responsible, for he usually prescribes it outright and in excessive quantities.

It is astonishing how little the physician reflects on this matter of milk diet. I am quite sure that I have known them to prescribe milk for a patient very sick and feverish, in quantities exceeding what a well and hungry man could consume and remain in health.

Only think of it—more than a gallon in twenty-four hours!

A quart or less, however, might kill as certainly.

Milk taken into the stomach must first be curded before digested, and it undergoes only the first process in typhoid fever stomachs, excepting the more watery portions that may be absorbed.

In the healthy subject even milk is known to be a feverish diet, and this because it is, in the ordinary form, difficult to digest. It has an old and true reputation of being a "feverish diet."

Some friend of the milk diet has demonstrated that, under its administration in typhoid fever, the bacilli at once decreased in number, and of course anything that destroys the germ morbi must be considered great.

But this proposition is easily disposed of. Milk is constipating and anything which checks the diarrhœa checks the multiplication of bacilli. It is, however, a dangerous astringent, and we have better

remedies for the diarrhœa than a mass of irritating curds. While milk binds the bowels it injures in other ways, and I have known hemorrhage to result from its exhibition after convalescence had about set in, just as I have seen relapses follow the eating of some hard substance or the administration of a dose of castor oil.

Another result of the milk diet and of careless dieting generally is a condition of chronic inflammation, or induration, of the bowel glands, following which may occur perforation or peculiar nervous manifestations, with extreme asthenia — generally fatal.

I have seen such cases where the nervous manifestation has been treated for remittent fever, but where post-mortem examination revealed only a condition of chronic inflammation of Preyer's glands. The lining of the bowel had gotten in a condition similar to that of the conjunctiva, commonly called "granulated lids."

I take the following from the Therapeutic Gazette:

Dietetics in Typhoid Fever—Dr. Bishop believes (North Carolina Medical Journal) that excessive temperature and many other disagreeable symptoms that arise in the course of a case of this fever are as much due to overfeeding as to any other one thing that can be mentioned. He says that Fothergill graphically describes the condition of the alimentary canal of typhoid patients in these words:

"There is also a brown, chapped tongue, well retracted, the brown fur consisting of an accumulation of dead epithelial scales, significant of the condition of the whole intestinal canal, with brown sordes on

the teeth of similar origin, accompanied by the formation of crusts on the lips."

"Now, here we have an alimentary canal in a dry, parched condition, all the secretions held in abeyance, the dead epithelial scales making it hard and glossy (or at least the parts that are open to inspection). The sensibility of the nerve endings are obtunded, especially those of the special senses of taste and smell, the power of absorption is greatly lessened. Brunner's glands and Peyer's patches are swollen and tender. Now we give milk for nourishment, but does it nourish? He can't help thinking that it does not. What does it do? It passes through a buccal cavity devoid of salivary secretions, enters a stomach whose flow of gastric juice has been materially lessened, if not altogether stopped. After it has remained here for a longer or shorter period, almost wholly unacted upon by the gastric secretions, herefore virtually as an offending body, it is probably passed or washed into the intestines by the next drink of water or milk. Well, as this is kept up steadily every two hours by day, and, as described in one hospital, every three hours by night, we can easily imagine why the bowels become full of gas, and the abdomen tense, with a tympanitic resonance. The curds become hard and abundant, irritate the tender glands of the intestines and cause pain in the stomach in its feeble efforts to digest them.

"Now, assuming that these conditions are all true, what must be the result when milk is given, as much as the patient can or will drink, as done in one case mentioned, and six ounces, mentioned in another case, every two hours?

"Tympanites, constipation or diarrhœa, high fever, delirium and coma, all of which are to be treated according to the most improved methods."

And the patient dies, I add, generally of bowel hemorrhage.

Bartholow (Text Book on Practice) falls in with the procession and recommends milk diet, but he admits that it has been greatly abused.

Reynolds (Reynolds' System of Medicine) says that if milk is exhibited it should be given in small quantities at a time and with seltzer or soda. He seems to have realized that danger might result from a careless use of the article.

Excepting an indispensible article, all medicines and articles of diet fraught with danger should be prohibited.

Osler, the great diagnostician, recommends three pints of milk in twenty-four hours, but he loses six per cent of his cases and mentions the fact that he meets with cases where bowel hemorrhage occurs.

In Hare's medical therapeutics we read: "In the dietetic management of typhoid fever the tendency at the present day is toward over-feeding, and especially with milk."

The author then goes on to quote from Sir William Jenner, who condemns the careless administration of milk, and to urge the free exhibition of pure water, a deficiency of which is found to exist in the blood of typhoid fever patients.

It has been proven to a demonstration that milk, in any considerable quantity, taken into the stomach of typhoid fever patients, forms into offending curds and produces disastrous consequences.

It so transpires that the last two cases of typhoid fever that I have seen, and in consultation with two neighboring physicians, were fed on milk freely. I saw both in third week of sickness and found the bowels tympanitic. One died of hemorrhage from the bowels, the other from severe chill and nervous shock, the result, as I thought, of an irritated condition of the bowels. I have seen these results so often that when I am called to see a case of typhoid now and find tympanites, I say, "been drinking milk?" and receive an invariable repiy, "yes."

On guarding a lady against the exhibition of milk in a case I was treating recently, she turned to her daughter and remarked, "You know how much milk they said they gave John?" "A gallon a day," she added, referring to a relative in a distant state. Said I, "he died, did he?" "O yes," she replied.

The practice of forced feeding and the resort to milk diet doubtless grew out of the old theory that typhoid fever is a self-limited disease in duration, that patients affected with it become necessarily much reduced, and that therefore a probably fatal termination from asthenia is best guarded against by timely support. But, even admitting the theory of self-limitation, (which has been exploded,) the error of the conclusion drawn from it, that the patient should be supported in the early stage of the disease, is seen in the persistence with which he holds up, even after repeated relapses, and the rapid convalescence from an extremely asthenic condition.

Experience teaches, indeed, that typhoid patients carefully dieted seldom, if ever, die from mere asthenia and that forced feeding often prolongs the case and reduces the patient.

I call to mind one particular case, where two relapses occurred (caused by the careless exhibition of cathartics), and in which, practically, no nourishment was taken for seventy-three days excepting whisky and water and the patient recovered as readily and as nicely as though convalescence had continued when it first set in on the twenty-eighth day and previous to the first relapse.

I have known numerous cases to relapse from careless dieting, and forced feeding in the first stage is, in my opinion, not to be distinguished from careless dieting.

On the other hand, I see case after case abort under a starvation regime and case after case run a mild course on a water diet alone.

Experience, observation and reason have forced upon my mind the conclusion that starvation in the first stage, or during the febrile excitement, conduces to strength later on, hastens recovery and more surely accomplishes what irrational forced feeding seeks to do.

In the meantime the average physician seldom thinks of keeping the head cool nor of relieving it of congestion by the old established means of water pouring or ice, or the lance in desperate cases, but relies solely on medical depressants that generally only mask the symptoms.

The prevailing plan of treatment for typhoid fever, therefore, is to feed and encourage, rather than to abate it, and to then turn and mask it into silence and death.

The result of this treatment is a mortality rate of from 25 to 40 per cent in hospitals and under eminent

care, and as I believe as high as 50 or 60 per cent among the less informed and less experienced members of the profession.

In an address before a body of physicians in New York City, Dr. Simon Baruch reminded his hearers that the records of the Board of Health, New York, (1876-1885,) shows 7,712 cases of typhoid fever, with 3,184 deaths, or a mortality of 41 per cent, and he expressed the opinion that the mortality was equally as high in private practice in that city.

Not only in the great hospitals and in private practice do we witness the lamentable results of the prevailing plan of treatment, but in otherwise well regulated penal institutions and in the army and navy we see an alarming mortality from typhoid fever, resulting from doubtless the same cause, exclusively drug treatment and forced feeding.

The hospital ship "Relief" left Porto Rico with 167 typhoid fever cases on board. Fourteen died on the voyage of only a few days, to which add a reasonably probable number of fatalities that must have occurred after the ship's arrival, would raise the mortality to a high per cent.

In Gen. Miles' annual report he gives 6,616 deaths from all causes, 2,774 of which were from typhoid fever.

The annual death rate in this country from typhoid fever, according to Prof. V. C. Vaughn of the University of Michigan, is 50,000.

Fifty thousand lives annually from a treatment that is more like "kicking a man when he is down," as Dr. Page says, than like rationalism.

Now let us consider the antiphlogistic method of treatment, consisting in the cold bath or pack, free use of cold water externally and internally and a restricted diet or starvation as a general treatment, and even venesection in special cases where early and violent head symptoms supervene or in very plethoric subjects, and incidentally a stimulating treatment in a great variety of cases and checking of the bowels in all cases.

I shall begin with a consideration of the general plan of hydrotherapy, since, when it is practiced vigorously at the onset of the disease, special treatments are seldom required.

The plan generally is as old as time itself, but it has of course undergone various modifications and, as now advocated and practiced, is said to have been in use by a limited number of physicians at a very remote period.

The revival (if such it be) of radical hydrotherapy may be attributed to Brand of Germany, and is termed the "Brand method." It consists of the cold bath treatment exclusively, and in referring to it little is said by the authorities as to the system of dieting adopted or of the medical remedies used in connection with it. Those who advocate it, however, and who have adopted it, so far as I have learned, recommend the free exhibition of water and give statistics showing that under it the disease either aborts or runs a shorter or milder course. Hence not much is said about dieting or medicines. Hare gives the following results from the method:

Jurgensen, (Tubingen) 217 cases, 1 death.

Vogl, (Munich) 221 cases, 6 deaths.

Military Hospital, (Strasland) 257 cases, 1 death.
Military Hospital, (Stettin) 186 cases, 3 deaths.
Brand, (Private Practice) 342 cases, 1 death.

In all 1223 cases 12 deaths—1 per cent. Says Hare: "Not one of these twelve deaths occurred in a case that came under treatment before the fifth day."

Dr. J. C. Wilson treated sixty-four cases by this method at the German Hospital, Philadelphia, without a single death; Dr. Peabody eleven cases at Bellevue Hospital with same resnlt, and wherever it has been resorted to comes the same report. The results from it are so gratifying and absolutely convincing that Hare concludes: "We are guilty of criminal neglect if we permit prejudice, difficulty of execution or any obstacle to deter us from adopting it."

Dr. Baruch, in his address previously referred to, lays great stress on the importance of the bath treatment, and Osler says that it is about the only thing necessary in the treatment of typhoid fever.

The method consists in immersing the patient in water at 65 degrees F. for fifteen minutes, and repeating it every three hours so long as the temperature remains at 103 degrees F.; or the patient may be placed in a tub and the water poured over head and shoulders.

But much more remains to be said about the cold water treatment.

Often serious head symptoms develop early in the progress of the disease, and when the temperature is not over 102. It is characterized by muttering delirium, *subsultus tendinum* and injection of the whites of the eyes. The pulse and general strength may be good.

This is relieved by pouring cold water over the head, held over the side of the bed, and repeated at short intervals, or as necessary. It is superior to ice.

The patient should be encouraged to drink freely of pure, cold water, preferably of soft water. The continued febrile condition deprives the blood of water (as demonstrated by scientific research), and thus its free exhibition restores normal conditions and invigorates, refreshes and strengthens the patient.

It is often necessary to apply cold cloths or ice to the bowels to prevent or arrest hemorrhage in cases that have been badly managed in the start, and even though the temperature be not high.

In private practice the wet sheet pack may be used instead of the bath, and with equally good results.

I have of late years resorted to the more liberal use of cold water in typhoid fever, and with the most gratifying results. And I have come in contact with no practitioner who has tried it and failed to find it the most successful of all methods. There is no longer any question about it.

I am not prepared to say what method of dieting was resorted to by Brand and others in the cases treated on the bath plan, but I presume that, as the treatment was expected to abort the disease or to cut it short, as it did, it was not considered necessary to pay much attention to nourishment, and that, therefore, the patients were not forced-fed. And I note that Dr. C. F. Fage, of Boston, an enthusiastic advocate of the cold bath treatment, is a most radical opponent to forced feeding. In a pamphlet on the subject he advocates absolute starvation, except the free exhibition of cold soft water.

But in private practice we come in contact with a great variety of conditions not met with in hospital experience, and hence other things are to be considered.

And an equal importance to anything else is the whisky treatment, which, though seldom required in cases well managed from the start, is nevertheless of incalculable importance in some cases and conditions.

Indeed we occasionally meet with a case in which we have at the outset a weak heart, a very high fever and a dry tongue, with perhaps *subsultus tendinum* (twitching of the muscles) noticed when we take hold of the hand and which denotes great prostration.

Here stimulation is urgently called for, according to Reynolds, and I have seen its charming results in a sufficient number of cases to satisfy my mind on the subject.

As suggested by the same author, too, stimulants are a valuable adjunct to the cold bath method in this variety of cases.

Remember that it is when we have both the rapid pulse and the high temperature that stimulants are indicated; for we have delirium and *subsultus* from head symptoms when the temperature is not high which does not denote general weakness, and we may have a rapid and weak pulse in case of hemorrhage when the temperature may be at or below normal and when stimulants are contraindicated, or we may have an infantile pulse without high temperature toward convalescence, or head symptoms with low temperature at the same stage, when stimulants are not specially indicated.

Again, a sudden rise of temperature often precedes a hemorrhage, when also stimulants are contraindicated.

The cases requiring stimulants are where the weak

heart with rapid pulse accompanies a persistent and continued high temperature.

An early dry tongue generally indicates stimulation.

The following extract from an article on the subject, by C. Binz, in "Quain's Dictionary of Medicine," may be read with interest:

PHYSIOLOGICAL ACTION.—Alcohol is a powerful antiseptic, probably from the fact that it is capable, even when diluted, of preventing the development of septic germs, such as vibrios and bacteria, as well as of paralyzing the activity of those already formed. In small quantity, and slightly diluted with water, alcohol promotes the functional activity of the stomach, the heart and the brain; whilst a like quantity largely diluted exerts but a limited influence upon these organs; if, however, the dose of alcohol be often repeated, it is readily assimilated, and becoming diffused throughout the system, undergoes combustion within the tissues of the body, imparts warmth to them, and yields vital force for the performance of their various functions.

Simultaneously with this consumption of alcohol, the body of the consumer is often observed to gain in fat—a circumstance due to simple accumulation of fat furnished by the food remaining unburned in the tissues, because the more combustible alcohol furnishes the warmth required, leaving no necessity for the adipose hydro-carbon to be used for that purpose. Three and one-half fluid ounces of alcohol per day is sufficient to supply about one-third of the whole amount of warmth requisite for the human body during twenty-four hours. * * * Doses somewhat larger, but still sufficiently moderate not to cause intoxication, act, for the most part, in the same way; but, as an additional effect,

they produce a distinct decrease of temperature in the blood, lasting half an hour or more. As far as the matter has hitherto been explained, this latter effect depends upon a directly depressing influence exerted by alcohol on the working cells of the body, and upon a temporary paralysis of the vaso-motor nerves. The latter is followed, of course, by dilatation of the superficial vessels, particularly those of the head, in consequence of which a larger surface of blood is exposed, and the loss of heat by irradiation into the air is increased, the temperature of the circulating fluid being thus lowered; whilst, the combustion being carried on by the cells being retarded, the generation of heat from this source is diminished."

And, under the heading of "Therapeutic Application," the same writer says:

"There can be no doubt but that a healthy organism, supplied with sufficient food, is capable of performing all its regular functions without requiring any specially combustible material for the generation of heat and the development of vital force. But the case assumes a different aspect when, in sickness, it transpires that, while the metamorphosis of tissue goes on with its usual activity, or with increased energy, as happens in many diseases, the stomach refusing to accept or digest ordinary food, fails to supply material to compensate for this waste. Here it is, then, that a material which can be most readily assimilated by the system, and which, by its superior combustibility, spares the sacrifice of animal tissue, is especially called for; and such a material we have in alcohol. Small, but oft repeated, doses of alcohol, largely diluted with water, are generally well tolerated by the weakest stomach; and, thus given, the absorption and oxidation of the spirit goes on without

difficulty or effort on the part of the patient's system. * * * In this sense alcohol is a food; for we must regard as food not only the building material, but all substances which, by their combustion in its tissues, afford warmth to the animal organism, and, by so doing, contribute toward the production of vital force and keep up the powers or endurance. As fever patients can tolerate large quantities of alcohol without showing any sign of intoxication, it is allowable, and sometimes even necessary, to rise in the scale of doses beyond the limits ordinarily prescribed."

In an article on alcoholism, in same work, John Curnow, M. D., says, in relation to the contraction of habit from the use of alcohol in sickness: "In the experience of the writer the exhibition of large doses in fevers and acute affections has never done this—indeed, in several instances, a great dislike to stimulants has been produced."

In same work W. H. Broadbent, M. D., in an article on typhoid fever, says, "In a large proportion of cases no alcohol is necessary from first to last; it is scarcely required in the early stages of the disease, except perhaps in drunkards; and at no period should it be given as a matter of routine, or merely because the case is one of fever, but only to meet certain definite indications. These are mainly evidences of weakness of the heart—frequent, weak and fluttering pulse, and weakness or absence of the first sound of the heart. When, as is usually the case, the tongue is also dry and the teeth and lips are covered and the mouth lined with sordes, the indications for the use of stimulants are unmistakable. The effect should be watched; when alcohol does good the pulse becomes

less frequent and of better strength and volume, and the temperature is usually lowered, an important indication also is that the odor of spirits is not detected in the breath. When very *high temperature* and other unfavorable prognostic symptoms set in very early, stimulants may be given without waiting for the conditions above mentioned. Alcohol is again often required as an adjunct to the treatment of fever by the cold bath. The safest form of stimulant is brandy or whisky; the quantity needed will vary greatly in different cases; in some two or three ounces in twenty-four hours will be sufficient, in others ten or twelve ounces will be required."

In Reynolds' System of Medicine we find the following rules laid down for the administration of alcohol or whisky in typhoid fever:

"Stimulants, in any considerable quantity, are not ("generally") needed in the early period of the disease. When required they should be well diluted. A few ounces of wine, in the form of wine whey, or dry port mixed with an equal quantity of water, may be given. Effervescent wines must of course be avoided.

"If the heart's action be weak, or the patient tends to lapse into the typhus state, brandy may be freely given, carefully avoiding excess.

"The following general rules may be observed in the administration of alcoholic stimulants in this disease:

As long as the pulse remains under 120, and retains moderate force, six or eight ounces of wine, or four ounces of brandy, given within twenty-four hours, will be sufficient. When the pulse ranges between 120 and 130 and is small we may double these

quantities; and if the heart does not respond to the stimulant after twelve hours thrice the original amount may be given.

"The bulk and force of the pulse must be our chief guides; and, if these notably fail from day to day, we must daily increase the quantity of the stimulant until the patient is supplied with as much as half an ounce every half hour, always diluted.

"When there is much hectic, and the pulse is small and sharp, strong stimulants often appear to increase the irritability of the system, and in such a case we should give them sparingly and in the early part of the day, trusting to a small dose of quinine, with or without opium, according to circumstances, in the evening."

It has been my fortune to meet with cases and conditions in my private practice where I have seen demonstrated the efficacy of stimulants according to the principles and rules laid down by the authors here quoted.

In one case of a young lady, aged 20, I gave a thorough test.

I diagnosed typhoid fever, yet she had not the usual symptoms well marked. I had eight cases of typhoid fever in the same block, and four members of the family of which this young lady was a member were stricken with what I called typhoid fever, yet it acted something like sporadic relapsing fever, or a union of typhoid and intermittent fevers—"typho-malarial," if there be such a special ailment.

It was in a family poorly fed, and all four of the cases relapsed on about the fifteenth day, two of them relapsed twice each and none of them from any known

cause. It was not in a malarial district and there were no malarial troubles in the neighborhood, nor did any of the cases present any clearly marked symptoms of intermittent fever, nor were they treated with quinine, excepting the one whose case I am specially considering, to whom I administered twenty or more grains of quinine on about the second day of illness on account of peculiar cramping spells and chilly sensations with a temperature of 106 F. There were two succeeding spells of this kind on I think the second or third days of illness, yet no clearly marked intermission nor bilious symptoms peculiar to intermittent, remittent nor relapsing fevers, and no bowel symptoms peculiar to typhoid. The head remained clear throughout in all four of the cases, until in the third week in the case of the one in which the whisky test was made.

Be the disease whatever it may have been, the patient on about the eighteenth day of illness, and after a relapse, presented the symptoms that called for heroic stimulation, according to the foregoing authorities, and it is at any rate certain definite symptoms and not the disease *per se* that call for stimulants.

The pulse was weak and ran 145 per minute; the temperature was 105; the tongue parched and seemingly paralyzed; she was mumbling, and reaching out with her hands; her eyes were half closed and she was gradually growing worse and weaker and was supposed to be dying.

I began to pour down whisky and water at the rate of sixteen ounces of good rye whisky in twenty-four hours,

At the end of twenty-four hours she seemed to be holding a little better than her own; at the end of forty-eight hours there was improvement. I reduced the amount to eight ounces during the next twenty-four hours, when she grew worse again. I increased the amount to sixteen ounces again during the next twenty-four hours, when decided improvement appeared. Here the disease seemed to be broken, as it were, and I gradually reduced the amount of stimulants, and stopped them in a few days.

It was twelve months before she regained her usual strength.

Of course, this was not a typical case; but remember that stimulants are called for to meet certain definite conditions and symptoms, whatever the disease. In a typical case of typhoid, treated on the plan of hydrotherapy and starvation during first stage, we might also have an early condition which would call for stimulants, but not so late as the third week. By this time the patient should be convalescent, barring mismanagement and accident.

In the extreme asthenia of protracted cases, and where ordinary articles of diet are not tolerated, three or four ounces of whisky per day serve a good purpose, but heroic stimulation comes in to control urgent symptoms, and is required for only a few days.

A Prof. Richardson, of a London hospital, has excluded alcoholic stimulants from the institution and furnished more favorable statistics of cures generally, as compared with other institutions, where alcohol was used; but this proves nothing in the consideration of a special subject such as we are handling. We are considering special conditions in a special disease, while the

report of Prof. Richardson was based on the treatment of general diseases, and he may not have had a special case such as I have here presented.

In attempting to prove anything in the treatment of disease, individual cases must be considered; and if stimulants are to be bodily banished from the physicians' armamentarium, I ask: "What should I do in a case similar to the one I have presented?"

I want to add here that for a similar condition in pneumonia—high temperature and pulse, with delirium and *subsultus*—I would not expect relief from stimulants.

It is because that in typhoid fever the condition is caused by bacteria, or basillus, blood infection, for which alcohol is a specific, as it is for snake-bite or septicmæic poison, while a similar condition in pneumonia is occasioned by a deficiency of oxygen in the blood, or carbonic acid poison, for which alcohol is not a specific, and which may be relieved, if taken in time, by venesection, calomel or other eliminants. In the one case the poison may be killed, or neutralized; in the other it must be eliminated.

The specific indications for alcoholic stimulation in typhoid fever are, as I believe, basillus infection, manifested or reflected by the symptoms that have been described, and extreme asthenia in protracted cases.

While it is probably true, as affirmed by Binz, that alcohol holds up the patient in protracted cases by supplying artificial heat, I believe that it is called for more frequently to counteract the effects of bacteria absorption, or as an antiseptic, than to supply heat and strength or as a food; for, while it serves the food purpose in cases much reduced, it is often at an early stage that it

is more urgently called for, and when these urgent symptoms are overcome the patient may then go on with but little, if any, stimulation, though many days of gradually falling fever.

To recapitulate, or to explain more clearly a little here, it will be observed that I hold to two theories as to the use of stimulants in typhoid fever—first, as an antiseptic, or to counteract the effects of germ absorption; and, second, as an artificial support, or to supply artificial heat when the vital forces are waning.

It may be a question as to whether alcohol is a food; and all well-informed persons know that its continual consumption tends to weaken and undermine the constitution. But many drugs that are useful in disease are injurious in health, and many that are proper for a time would be poisonous if continued indefinitely.

For practical purposes in disease, I care not whether alcohol be considered a food or otherwise. "Facts are before theories," and when, in repeated trials and experiments, it is demonstrated to my mind that in certain special cases alcohol will do what no other known remedy will do, and bring results for good that cannot be had without it, I shall continue to give to it a place in therapeutics.

On this question of alcohol the professional mind is tending to take a drift exactly similar to that taken after the blood-letting controversy of 1857-8 and to do away with it entirely.

When the physician used to bleed for everything and in all stages of disease, many cases were injured, rather than benefited, by venesection; but in the revolution of thought against heroic treatment it has been almost forgotten that, as the best authorities assert, it is

yet the sheet anchor in a limited number of cases and in certain conditions. Notwithstanding an extremist occasionally states that its days are entirely past, on reflection he admits its occasional utility.

So now we occasionally note a radical statement, such as uttered by S. N. Davis, that "alcohol is of no value in any case, and absolutely injurious in all cases;" but at the same time we find such men totally unprepared to meet the arguments of such authors as Reynolds and Binz, or to tell us how to dispense with it or what we may safely substitute for it in cases such as I have cited, in cases where habitual topers are prostrated with certain maladies, in certain cases of diphtheria, and especially in certain septisæmic conditions, where it has been proven to be the sheet anchor.

I believe that alcohol, like venesection, has been abused, and that it should be used with greater care and less generally than it hitherto has been, but that it has no place in rational therapeutics is a dogmatism unsupported by science and contrary to the accumulated experience of the profession.

Not many months ago Prof. Davis was asked a few pointed questions on his anti-alcohol position, and in a leading medical journal (Medical Brief, of St. Louis), but the profession have not been enlightened with a reply.

Prof. Davis' statement was a bare assertion—a dogmatism—supported by no argument.

Toward convalescence we often have, in typhoid cases, a dull condition of the brain, which may be called passive congestion, and similar to what appears in many cases of cholera infantum.

The patient is crazy, but not wild.

This condition, in both of the diseases named, has always been relieved, in my hands, by a fly blister to the back of the head.

For diarrhœa and bowel tenderness I have used tannin, opium and turpentine, with cold applications, with universal success.

As nourishment in typhoid fever I rely on pure water, which supplies the deficiency of that article which exists in typhoid, coffee as a mild tonic, orange or currant juice to supply carbo-hydrates sufficiently in ordinary cases, and whisky to also supply carbo hydrates and to counteract bacteria poison or extreme asthenia in desperate cases.

Ordinary food, and especially milk, cannot be digested, and, while a little might be absorbed, the more solid elements remain as only irritants, and destroy lives innumerable.

We get more carbo-hydrates, or real support, from orange or currant juice than from any quantity of milk, and fruit juices need not be given in any great quantity.

Apple, peach or pear juice contains malic acid, and is often too much inclined to start the bowels to moving. Otherwise with the berry tribe.

I begin the treatment of typhoid fever with the cold bath or cold applications sufficiently to control fever, especially in head and bowels; and, to refresh the patient, I cut off the diet at once, except as herein previously indicated, which, as ordinarily viewed, amounts to starvation; check the bowels on the spot, and expect a mild run of the disease and an early convalescence.

With this treatment typhoid fever ceases to be a self-limited disease and is shorn of all its horrors.

The treatment is not new; it is as old as written medical history, and is only being revived, modified and improved.

It is simple as simplicity itself, and under it the patient needs little professional attention. I have treated cases without ever seeing them.

The patient cannot digest food, stop eating; he is hot, cool him off with the most natural means; if he has diarrhœa, control it with laudanum and tannin, as you would any diarrhœa, and, if weak and exhausted, give a little whisky. This course, pursued the earlier the better, will obviate serious complications and guarantee a speedy recovery.

But the physician will be called to see cases of typhoid in various stages of the disease, cases out of the ordinary in manifestations of symptoms, and cases that have been badly handled or neglected. These may be called emergency cases and require careful handling.

First here may be mentioned cases with early and severe head symptoms. Often these cases are so urgent that, but for the history and other symptoms, they may be mistaken for meningitis, but happily, as the same means of relief are suitable for both, differential diagnosis need not worry us.

They are relieved at once by venesection and water pouring, one or both, as indicated and as the judgment of the physician may determine. The safest course is heroic and thorough treatment. Water pouring may be tried for a few minutes, and if relief comes, its continuance at intervals may, together with general bathing, be sufficient. In robust subjects,

however, venesection may be necessary, and there is positively nothing to be feared from it.

As a rule drug treatment will not relieve them, nor will the application of ice take the part of water pouring.

The cases in which we have a weak heart, high fever and a parched tongue early in the course of the disease and that require stimulation, have been referred to previously. I need only add here that, when the head symptom is also present in these cases, the treatment for head symptoms may supplement stimulation, and that both act admirably together where indicated.

Occasionally, however, even at a late stage, the head symptom may be violent and require heroic treatment. It should be relieved regardless of the apparent strength of the patient, and I desire to emphasize this point, for here is where timidity causes the physician to shrink from his duty. Heroic measures will not kill the patient, but continued head irritation surely will.

Violent head symptoms will often supervent in conjunction with increased or severe diarrhœa. That is, diarrhœa and head symptoms will occur simultaneously.

Both must be controlled at the same time, for, while in many diseases the head is relieved by the revulsive effects of cathartics, it is not true in typhoid fever that diarrhœa relieves brain irritation. The reverse is the rule.

Serious head symptoms, as also serious nervous depression will often follow in cases that are, by mistake or otherwise, treated on the quinine plan during

the first stage. Boost the patient up on quinine during early stage and it will collapse later on.

But the class of cases that excite our pity and that at the same time require the most delicate handling are those that have been medicated and dieted to their injury.

It may seem presumptious in one physician to condemn as wrong the acts of another in the treatment of disease, and it is generally so, and I speak here only after having been convinced beyond all controversy.

I know, if I know anything about medicine, that continued purgation and free feeding in typhoid fever produces always alarming bowel lesions.

Every physician ought to know this, and it has been known before the writer hereof was born, yet it is the great number of such cases that I have seen in consultation that have demonstrated the truth to my mind.

In such cases we find, usually about the third week of illness, a slight or considerable tympanitic condition of the bowels. The patient is bright, temperature not high often and convalescence seems to be approaching, when suddenly the temperature rises a little and a hemorrhage follows about twenty-four hours later, and the patient collapses in a day or two, if not sooner.

If the case is seen before the hemorrhage occurs, ice applied to the bowels and laudanum internally may prevent it and the patient may yet recover, after a hemorrhage, under very careful management. But cold to the bowels must be kept up, with laudanum and turpentine rather freely internally.

The application of warm poultices to the bowels in such cases, as practiced by some, invites more blood to the inflamed glands, extends the inflammation and hurries off the patient. Dr. Page, in ridicule of this practice, asks: "Would you expect to cool a red-hot poker by placing it in a bed of coals?"

The condition here is a chronic inflammation and induration of the glands of the diseased bowel, resulting from the presence and irritation of indigested food, gases or milk curds, and it results generally in hemorrhage, perforation or exhaustion.

While typhoid subjects do not usually die from mere asthenia, they may do so in this variety of cases, and because the condition is a continued bar against feeding, when the patient should have been by this time in a condition to take nourishment, and it is also a bar against the use of stimulants, which might keep up the fires of life in a very protracted case and in the absence of bowel congestion.

But it sometimes occurs in these cases that this condition of the bowel causes a peculiar nervous manifestation simulating intermittent or remittent fever, when the physician, misjudging his case, resorts to quinine, only to see the symptoms continue and the patient's time shortened.

The attempt to thus control symptoms, here, that are not understood, only serves to prevent the patient from dying of asthenia, to say the least, and because it hastens a fatal issue.

If it clearly appears that the tympanites is caused by the presence of milk curds, a dose of oil or calomel may be of benefit. I have seen three or four cases in young

children where calomel brought relief in such cases, and the treatment is endorsed by the best authorities.

If the condition is simply chronic inflammation of the bowel lining, nitrate of silver is the remedy—one-fifth grain in pill four times a day, prepared fresh every day.

I would, however, caution the reader here against the notion, entertained by some, that tympanites is generally to be relieved by cathartics.

On the other hand, cathartics, or even laxatives, only aggravate the trouble, barring the presence of curds. There is no case, except where sweet milk has been drank carelessly, where the bowels should be moved for the relief of tympanites, and a continuation of the usual diarrhœa is almost sure to be followed by tympanites.

In the early stage of the disease, and generally, tympanites is caused by inflammation, aggravated by cathartics, but prevented by the timely use of astringents, cold applications and opium.

The belief that tympanites can be relieved by removing the accumulated secretions, and on the theory that they act as matter offending to the inflamed glands, is one of the greatest errors made in the treatment of typhoid fever.

So long as the pathological condition remains the secretions will re-form as often as removed, and to permit or to force the bowels to move continually only opens a dam which continues to fill with increasing rapidity and to the exhaustion of the patient.

The pathological condition must be dried up by means of astringents and remedies that induce repose, when secretions previously poured out will take care of themselves.

SUPPLEMENTAL.

ABSURDITY OF FORCED FEEDING IN ACUTE DISEASES.

"Instances of longevity are chiefly among the abstemious. —Arbuthnot.

A prevailing hobby endorsed and adopted by the medical profession generally, allopath, homeœpath and all schools, is that, in all ailments the patient must be supported and, whether nature protests against it or morbidly demands it, every ailment is crammed and stuffed from start to finish. From the old maxim of "starve fevers" and from the old habits of "fasting and prayer," as means of cure, the pendulum seems to have swung to the extreme in the other direction, and devils are expelled by forced feeding. There is scarcely an article written for the medical journals on the general treatment of any disease, or a disquisition for the text books, that does not teem with the demand for support, whatever the ailment or conditions of the organs of digestion and assimilation.

To "support" seems to fill in at every point where rational medication cannot be conceived of and physicians seem really to vie with each other as to how much they can say in favor of it, and to boast of how much nourishment they can crowd upon a system often unprepared for its reception. I see leading editorials in medical journals urging support in cerebro-spinal meningitis, when, in this disease, usually the stomach revolts at everything in the shape of food, and when

the system is surcharged with effete products that have accumulated from overfeeding and lack of exercise, often, and in pneumonia when clearing of the alimentary tract is generally most urgently demanded and when the organs of digestion are out of order, and I have met in practice and heard of hundreds of physicians who force from half a gallon to a gallon of sweet milk into the stomach of typhoid patients during twenty-four hours, when secondary digestion is absolutely suspended, resulting in the accumulation within the bowels of curds, undigested products and gases, and producing hemorrhage and destructive inflammation. Now everywhere it is recognized and remarked about that overeating is the prolific cause of most acute ailments, yet no sooner has overeating produced the disease than that the physician urges support—"*samilia samilibus curantur*," or "the hair of the dog cures the bite."

What do you want to feed typhoid fever for, when the patient has no appetite and when digestion is absolutely impossible? The abstemious are never bilious; and when, in pneumonia, the liver needs unlocking and the stomach is foul, what do you want to feed for?

In meningitis, when the patient must be relieved instantly, if at all, why talk about support? How much time have you here to build up the system? An irritable stomach is characteristic of meningitis, and the patient vomits food, medicine and everything else. Going to nourish this patient, are you? It will die while you are trying it.

Of all the absurdities ever conceived by men pretending to scientific attainments it is that of feeding in acute febrile diseases, when the organs of digestion are

not at work, when assimilation is impossible, and when the entire system is in a strain, attempting really to throw off a surplus of dead products resulting from overfeeding.

Jesus Christ was a better physician than many modern savants.

Speaking of one possessed of a "devil" (which was probably a case of epilepsy, or at any rate some form of bodily ailment,) he said, "This kind are driven out by fasting and prayer," or a similar statement.

He had at least learned the value of abstemiousness as a factor conducive to health, and to this extent I have become thoroughly converted to christianity.

Stop forced feeding in acute febrile diseases, and the days of your patients "shall be longer upon the earth."

APPENDIX.

SOME PECULIAR THEORIZING.

While it may seem that the author has been a little dogmatic in much that is contained in this work, I believe that it will be admitted that his dogmatism has been generally on the side of experience and against that form of theory that is unsupported by experience.

Theory, however plausible, may be wrong; however ridiculous, it may be right.

Medicine is an inexact science, and, year by year, theories come and go. Often a theory which we rely on today is thrown to the dogs tomorrow, and, unless a theory has been long established and well supported by experience, it is, for practical purposes, of no more worth, often, than the grossest superstition. I have seen the most fine spun theory of the educated savant fail where the common sense of an uneducated housewife has succeeded.

Don't turn the deaf ear to a seeming ignoramus who, with contradictory or absurd theory, presents a fact, for beneath his reasoning there is often a fact which, though he knows, he cannot explain. He has seen something or felt something or instinctively perceived that which perhaps no theorist can explain and, therefore, has often no solid reason to gainsay. For instance, an uneducated man said to me that "doctors ought to bleed more for head troubles." He

said he had spells of aching of the eyes, and believed bleeding would help them.

I asked why he thought so and he gave me a fact and his theory for it. He said that when his wife was confined she had spasms and at the same time was flooding, and the doctor bled her in the arm, whereupon both the spasms and the hemorrhage ceased. His theory was that in the hemorrhage the blood did not come from her head, but when she was bled from the outside veins above the waist the blood came from her head and cured her, and, being cured, of course the uterine hemorrhage ceased.

Now there may, after all, be something in this theory, though none of us can explain it. But the fact was that the operation, based on the theory which he said he got from the doctor, was followed by success, and the same procedure is urged by the authorities in such cases.

The author relieved a woman once under similar circumstances.

I held the uterus down, however, with my hand while I bled her.

Another man said, "Back in Kentucky they used to bleed everybody once a year in the spring and you never heard of much bad sickness there."

I asked him how he accounted for it.

"Why," said he, "it takes out the bad blood and purifies the system."

Now, while this man could give no rational explanation as to why and how the blood became bad, nor how that vensection "purified" it, I believe there is a correct theory for the procedure he related. I believe that overeating in some way surcharges the

blood with poisonous elements that tend to accumulate beyond the powers of the emunctories to eliminate them and that, often, venesection is highly beneficial as a depurent.

It goes without saying that reasonable abstemiousness is the greatest preventive means against many ailments, and I believe that overeating makes the blood too rich or too thick, or surcharges it, and venesection cures these conditions that abstemiousness tends to prevent.

The excuse or reason generally offered by the many people who believe in venesection and who wish to be bled, is, "I believe I have too much blood," or, "I believe my blood is too thick."

There is not much theorizing here, but I believe they are telling a great truth.

Overeating makes too much blood, as well as too much flesh often, and, while a course of diet commenced in time would do about as much or more than venesection, people are not apt to regulate their diet, and besides are often in need of relief before they have thought of it.

The physician often laughs in derision at the man who wants to be bled because his blood "is too thick," but it is the full-blooded who seek the remedy and who experience relief from it, and it is a scientific fact that venesection makes the blood more watery.

So that, often, the patient knows more than the physician, however little he may know about medicine generally.

I have known of persons who had been bled to their relief beg and plead for a venesection at the hands of the physician, only to be refused and to die

from what ought to be held as "criminal carelessness." Physicians, as a rule, know a great deal less than they are given credit for.

Thousands of people outside the profession yet hold to venesection traditionally, and to that tradition the subject often links a form of instinct which makes him really wiser than the most logical theorist.

It is a wise physician who can give to tradition and instinct the proper weight in the practice of the healing art, but it does not require a very profound mind to know from obssrvation that the physician who runs wild on theory often knows less of practical worth in some things than the ordinary housewife.

The extremes of absurdity to which mere theory may carry the medical mind is seen in the conclusion of the recent "tuberculosis congress" held at Berlin, that phthisis pulmonalis is "not hereditary."

Any one but a very learned physician would be regarded as a fool in making this assertion, and perhaps the great lights who promulgated this wonderful evolution of theoretical insanity did not reflect that no layman (and few physicians) on top of dirt will more than ridicule the conclusion.

THE EFFECT ON THE AVERAGE MEDICAL MIND OF MODERN BACTERIOLOGICAL RESEARCH.

I have somehow been peculiarly impressed with the effects on the practice of medicine of recent bacteriological research and discoveries, whether as practitioners, we should be more of a bacteriologist than a physiologist and pathologist or vice versa.

Should we attempt to run down the basillii and the bacteria, or is it better to simply treat an inflamma-

tion on common sense and well established principles?

This subject has a more particular bearing on the treatment of typhoid fever.

This line of investigation has done, and is doing, much in the way of explaining the cause and nature of disease, but perhaps less of a helpful nature in suggesting improved methods of treatment.

The nature of disease is not changed by these investigations and, whatever the discoveries, methods of treatment that have proven successful before, will not be abandoned after them. In treating internal inflammations, it does me no good to know that each has its special microbe as cause or effect. I have simply an inflammation, and if certain remedies discovered previously to the discovery of the bacillus, have proven successful as means of relief, we should adhere to them, notwithstanding none of them are microbe killers nor disinfectants.

Numerous specifics have been offered for the treatment of typhoid fever on the antiseptic theory, but they have proven to be of little value compared to cold water and starvation or to opium as an antiseptic. Their effect is absolutely nil in the absence of the means of relief herein suggested. Theoretically they should be alone sufficient, practically experience has proven them next to worthless.

Time and time again I have met physicians who seemed to have swallowed the bacteriological dictionary, who were filling the stomach and intestines with antiseptics and germicides as the only means of cure, but who were, at the same time, neglecting the use of water and the older remedies for relief, paying no attention to diet and letting the bowels run, and losing most all their patients.

Of course, they were "very bad cases to start with" and "the doctor was very faithful."

During the past year I saw two cases of typhoid fever with two neighboring physicians, both of which patients died.

The physicians were both bright and good general practitioners.

On consultation with the first one I was confronted with a germicide and "specific" vocabularly almost beyond my comprehension and a shelf of pellets, tablets, triturates and powders unknown to me as remedies in this disease; but the diet had not been sufficiently guarded nor any attention paid to a violent meningeal complication, and the patient died early in the battle with specifics.

In the other case, the physician permitted the bowels to run "any number" of times a day, actually gave castor oil every day to keep them moving, allowed the patient to eat and drink most anything he wanted, and, when tympanites developed from the free consumption of sweet milk and potatoes, and when fever raised and the case turned for the worse in the third week, my worthy collegue explained that there had occured "a new and extensive bacteria invasion," which he had however failed to prevent by the administration of the germicides, antiseptics and "triple phosphates" such as I never heard of.

The patient died from bowel hemorrhage.

Now both of these physicians were trying to learn something and deserve credit for it, but they had been absolutely hypnotized by bacteriological theorizing and germicidal fads.

Neither had even thought of the use of cold water or ice nor of the old reliable remedies for typhoid fever,

and of course venesection and water-pouring which (one or both) would have relieved the case with early violent head symptoms (it being a robust subject too) were never dreamed of.

In the attempt to run down bacteria does not the physician often forget that the diseased germs may originate and multiply within the bowel or diseased part, as well as to come from without in the first instance? And, if so, will not reflection teach him that continued irritation of the inflamed part, by means of ingesta, may multiply germs faster than he can kill them with germicides and antiseptics?

We may destroy the germs successfully, yet, after convalescence is established, the eating of a piece of apple or a dose of oil, by irritating the tender recently-healed glands, may induce a new inflammation and another regular course of fever, thus demonstrating that, by irritation, the causative germs have originated entirely from within.

I do not believe that the bacteria, once absorbed, can be eliminated by artificial means, but that, once entering into the blood, they must be consumed by internal thermogenesis, of which process fever is the outward manifestation. And, if this is true, how absurd the attempt to control the fever by means of antipyretics, rather than to first subdue the local inflammation, when the germs cease to multiply and the fever abates naturally.

I think that Prof. Cantini, of Naples, has correctly stated the case in the thought that "fever (in typhoid) is the expression of organic reaction against microbic infection" and that the "microbes are destroyed by fever combustion."

And from this we must infer that, so long as germs multiply, the fever must continue, and that, therefore,

instead of trying to kill germs we should rather attempt to prevent their multiplication by suppressing the local inflammation.

I repeat, therefore, should the physician not be more of a physiologist and pathologist than a bacteriologist?

Physiology teaches me that, when digestion and assimilation are suspended, food acts as only an irritant.

Pathology teaches me that in typhoid fever the blood is deficient in water, from which I infer that, in the process of microbe combustion, the watery elements are consumed and that the re-supply of these elements constitutes rational scientific treatment.

Pathology teaches me again, and as we all understand, that the more extensive the internal inflammation the more the danger of systemic infection.

This we knew before we knew anything about microbes and when we designated the danger as a form of septicæmic, rather than a "microbic infection."

What has bacteriology taught us?

Simply that things are called by different names, not that we have gotton any real practical information.

It gives us an insight as to the character of the "*materies morbi*" a more scientific conception, and, while it thus brings more science to our support, it does not add so much to what physiology and pathology has taught us.

Certainly bacteriology is but a handle to pathology and its researches should be encouraged, but the thing I am trying to impress is that the practitioner too often relies on only the handle and gets his cup sometimes upside down.

And we may reflect that, while the successful treatment of typhoid fever herein set forth has been sustained by bacteriological research, it was evolved independently of the microscope.

(THE END).

A "RIP VAN WINKLE"

IN THE

Practice of Medicine

The Resurrection of a Hero, and the Revival of a Lost Art

BY

H. J. PARKER, M. D.

CLAYTON, ILLINOIS.
1899.

"Facts are Before Theories."—*Aristotle.*


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