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A

TREATISE ON FEVER:

ITS

CAUSE, PHENOMENA, AND TREATMENT.

WITH

AN APPENDIX,

CONTAINING

VIEWS ON SOME FEMALE DISEASES, SOME DISEASES OF CHILDREN, ETC.

BY

REZIN THOMPSON, M.D.,

NASHVILLE, TENN.

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PREFACE.

THE author feels profoundly thankful to the all-wise and beneficent Disposer of events, and very grateful to an intelligent and liberal profession, that the present edition of this work will appear under much more favorable auspices than did the first. Then the author was quite unknown; and coming forward, as he did, with views on some subjects at variance with received opinions, and proposing to do some things which the profession had decided could not be done; by remedies, too, which had been always known, and supposed to possess but little power over diseased action, and therefore wanting the fascination which ordinarily attends the introduction of new remedies—purporting to accomplish by the use of third-rate remedies what others had failed to accomplish with the most potent articles of the materia medica—it was but natural to suppose that he would be considered a visionary dreamer, and his work cast

aside, as not meriting the attention necessary to ascertain whether he was dealing in facts or fancy. Having a very vivid conception of these things, it required the exercise of a greater amount of moral courage than any other act of the author's life to sustain him in his resolution to bring out his work at all; but feeling that he had a mission to perform, he resolved on doing his duty; and now he has the high satisfaction of knowing that he has been the means of doing much good; and that his little work, though unattractive in its appearance, and containing many errors in typography, has been highly appreciated by many of the most enlightened of the profession, and a second edition now earnestly called for.

The author returns his sincere thanks to his brethren in the profession who have so nobly come forward with their approbation to sustain him in his work; and although some who have written to him have not had the entire success in the use of his plan of treatment which others have realized, he highly estimates their candor in reporting, and hopes to be able in this edition to point out the secret of their partial failure.

The arrangement of the present edition will be found somewhat different from the first: the author had it in his mind to leave out the chapters upon

the circulation, but has been advised to retain them, and add a condensed view of the nervous system; because it is thought that a review of these things will do the old practitioner no harm, and that, as many persons not connected with the profession may be induced to read this work, a plain, comprehensible view of the circulation and the nervous system would not only be interesting, but of real advantage; for it is now well understood that the more correct information the people have upon the truths of medical science, the better they are prepared to appreciate "honorable medicine," in contradistinction to pretence and quackery. Other alterations in the arrangement of the work have been made, grouping together such parts as had a particular relation to each other, so as to present a more systematic treatise.

In carrying out this idea, dysentery and cholera infantum have been assumed to be febrile diseases, (which they really are,) and are accordingly classed with particular fevers; and what is said of female diseases, and diseases of children, etc., has been thrown into an Appendix, so as to make it suitable to simplify the title of the work, it now being "Thompson on Fever, with an Appendix." This arrangement was not determined on until the work was partly stereotyped, and hence the old title of

"Thompson on Fever, Dysentery, etc.," occasionally occurs in the first part of the work.

Inquiry has been made by several why the author quotes so much more freely from Fordyce upon the subject of fever than from any other author; the answer is, that in Fordyce the author finds nearly all that has since his time been said upon the subject, and he preferred the "pure wine undiluted." In the first edition the author said that this is a *transition period*, when physicians were disposed to abandon the extra debilitating treatment, and adopt a milder and safer plan of treatment, many inclining to the use of remedies of a more or less decided stimulating character. Since then this tendency has become more decidedly manifest, even the late writers of France and England having modified their treatment materially, almost abandoning the antiphlogistic treatment in fevers and inflammatory diseases; so that the author's plan is now "in harmony with the tendency of the age," and will the more readily get an impartial hearing.

Upon a very careful review, the author has not found it necessary to change or modify any of his views upon the science of diseased action; yet his further observations, and the suggestions of others, have enabled him to clear up many difficulties which had presented themselves to him, and to the

minds of others, with regard to the phenomena which disease occasionally presents in its progress, and the variations of treatment which may become necessary under different circumstances. And although he is fully aware that no system of practice, however simplified, can ever be presented to the public which will enable the people to become their own physicians, and supersede the necessity of the services of the regular professional educated physician, and, consequently, has made no attempt to operate outside of the profession, yet as many planters and others have procured his book, and have managed their own cases of sickness with very good success, he has in this edition tried to present such rules as will enable them to do so more understandingly.

Physicians need have no apprehensions that a reduction of practice will follow a free introduction of this work among their patrons; on the contrary, the better informed a community is upon the subject of medicine, the readier it is to avail itself of scientific skill: it is only the quack and the nostrum-vender that need to dread the light. True, families who have this work may often ward off threatened attacks of sickness, and even manage uncomplicated cases, without the aid of a physician, but in all grave cases the doctor will still be sent for: the

author's best patrons are those best posted as to his peculiar mode of treatment. He would, therefore, advise every one to study his book carefully, so that he may be able to do something understandingly in the absence of his physician, and be better prepared to appreciate his skill when called on.

The Introduction to the first edition is retained entire, because it shows the author's feelings at the time, and is a good *exposé* of the general objects and aims of the work.

Notices and experience of others may be found at the end of the book.

Dedication.

To W. K. BOWLING, M.D.:

Influenced by a desire to manifest an appreciation of the high reputation which your merits and attainments have won for you in our Profession, and the noble position you have ever maintained in defence of "honorable Medicine," as well as by the many personal and professional courtesies received at your hands, the Author respectfully inscribes to you this work. And while not expecting his views and opinions to escape criticism, yet, if characterized by that spirit of candor and amenity for which you are distinguished, he will be satisfied, though some of his ideas and theories be regarded rather as the productions of fancy than developments of scientific truths.

Prof. W. K. Bowling:

Dear Sir: For the reasons given in the above dedication, and for the further one, that to you, more than any one else, the author is indebted for the favorable reception of the first edition of this work—for you not only kindly apologized for the unworkmanlike manner in which the book was gotten up, and for the many typographical errors which remained in spite of the author's best efforts to the contrary, but, looking alone to the real merit of the production, you drew attention to such features in it as you thought were worthy of commendation; and though you neither embraced his theory of Fever, nor endorsed his plan of treatment, yet for years past you have, in your Lectures, set your face as a flint against the murderous practice of excessive bleeding and purging in Southern diseases, and especially against the use of mercurials in Typhoid Fever; and in this way hundreds of minds had been prepared to give an impartial hearing to doctrines going still farther in the same direction — he therefore respectfully dedicates to you the present edition, hoping that your life may long be spared to further on the cause of *honorable medicine.*

INTRODUCTION.

In this age of multiplication of books, when the only reason thought necessary for bringing out a new one is that the author can write something which others will read, it may be thought superfluous that any apology should be offered for presenting another work to the reading public. But as the writer of the following pages is not courting literary fame, nor does he design to fix up the wares of his imagination for exchange for money, still less would he pander to a depraved taste for reward; having no party to defend, no clique to uphold, and, at present, advocating no great moral reform, it may be proper enough that he should in advance tell why he has written at all. If we consider the human family with regard to their wants, their nature, their capabilities, and their destiny, we would naturally come to the conclusion that their *eternal* interest would claim their first and greatest attention; next to which, as essential to enjoyment of comfort in this life, that the preservation of health should rank in importance. The third interest in rank would be the laying by means for

securing physical comforts; and, lastly, the present gratification of the wants of their mental or animal nature. But observation abundantly proves that generally this order is reversed; *present* gratification being considered *first*, and *eternal* interest *last;* and the mode of preserving health, and the means for removing disease, so utterly uncared for, except when suffering comes, that did not men study them as a means of making money, they would be forgotten from the earth, and man in this respect would speedily relapse into barbarism, having no science to direct him and no means at hand by which to relieve the misfortunes of the flesh except such as nature furnishes ready prepared and at the place. Every individual of sane intellect knows that he must some time die, and that he will probably be sick more or less before that event; but these are merely entertained as abstract facts and probabilities, without having the least effect upon present conduct; consequently, there is not one in a hundred but would spend a dollar for something rich and rare to gratify the animal appetite, a theatrical performance to minister to the gratification of the passions, a new work of fiction to satisfy a morbid desire for mere excitement, rather than expend it for a knowledge of means for preventing or curing a disease, which he may never suffer, or for preventing premature death, when he already believes that dreaded event a long way off.

The author has not, therefore, written his book for the purpose of pleasing or entertaining, or any

way directly benefiting the people in general, because his subject, being the best mode of overcoming disease—which they do not intend to have, and do not intend to think about until it comes—is one which they will not consider, and therefore they will probably never read his book.

But there is a body of men whose business it especially is to attend to these matters, who make their living (often a poor one) by attending to them, and it is for them that these pages are written—written because the author believed they would offer facilities for contending successfully with at least some of the more formidable difficulties they have to grapple with in the warfare they are waging with human diseases. But, notwithstanding the author has addressed himself particularly to the medical profession, yet he has endeavored to make his reasoning so plain, and has encumbered his language so little with technicalities, that any intelligent person can read it understandingly, and he hopes with pleasure and advantage. Having been assiduously engaged for a period of about thirty years in the practice of this profession, and having made *diseased action* his constant study during that period, ever striving to trace the signs or symptoms of disease to the particular departure from healthy action from which they arise, among the many things which he has found to be obscure, the author had his attention particularly drawn to the mystery which has ever rested upon what is called the "febrile movement." The nature of the particular

condition of the system which gives rise to the group of symptoms which the profession as well as others call fever, is confessedly not well understood. The author read much that has been written upon this subject, both by the ancients and moderns, much of which he considered absurd, much contradictory, much visionary, and nothing entirely satisfactory. In fact, many of our most talented and popular writers have honestly acknowledged that they did not understand the subject. Dr. Fordyce, whose treatise on fevers has formed the source from which all other writers have drawn their description and most of their ideas of this disease for the last half century, says: "Every man who has read the various descriptions of fever which have been given by authors, ancient and modern, of one country or of another, becomes immediately sensible that neither its causes, rise, progress, nor termination, are thoroughly known or perfectly described; and of this he will be more fully persuaded if he has frequently had occasion to see the disease. The history of fever, therefore, is by no means thoroughly understood. That its treatment is not, appears clearly, as the practitioners of different countries who attend patients in this disease, nay of the same county, district, and even town, although of great learning in medicine, employ very different modes of cures."

Again he says: "In short, fever is a disease the whole of the appearances of which have been in no ways accounted for."

Again he adds: "These sensible appearances in fever seem so very independent upon one another, that they can only be considered as symptoms of some alteration of the system which has not hitherto been investigated." The ingenuous Doctor, then, frankly acknowledges that he is unable to show up the connecting links so as to present an unbroken chain of causation, but admits that such a chain does exist, and hopes that some future investigator may succeed in demonstrating it.

A writer in the Transylvania Medical Journal says: "But notwithstanding the immense labor that has been devoted to the investigation of fever, the annual number of its victims is a sufficient evidence that the disease is not yet understood, at least so far as treatment is concerned."

Another, in a late number of the Boston Medical Journal, terms this disease, "That embodiment of transcendentalism which we call fever."

Professor Cross, of Transylvania, says: "No topic has been more earnestly discussed than the nature and treatment of fever, and yet few points have been settled less to the satisfaction of the generality of physicians."

Dr. Cooke says: "When we seriously consider the striking difference, not to call it positive opposition, which marks the different theories of fever, and the modes of treating it, and again consider the mass of testimony, equally voluminous, and apparently alike reputable, in favor of each, we are puzzled not a little to decide how far it is safe to

confide in human perception." Since Fordyce, but very little light has been shed upon the subject by any writer.

The author is, therefore, warranted in saying that the profession is yet ignorant of the real cause of the phenomena of fever.

Some years since, while the author was watching the disease at the bedside, as it then manifested itself in the various forms of Dysenteric Fever, Intermittent and Remittent Fever, and Typhoid Fever, all in the same neighborhood, at the same time, his mind was drawn to inquire what could be the nature of the cause which produced such various manifestations. And while meditating upon the subject, an idea of the probable nature of the remote cause of fever presented itself, which was entertained only for a moment, and then dismissed; but it recurred again and again, each time associated with increased evidence of probability. Finally it was deliberately entertained, and a searching train of inquiry and investigation instituted, which resulted in an entire conviction of its truth. But as it could not be demonstrated to be absolutely true, and as no *practical* benefits were perceived which would arise from its establishment, it was only received as a satisfactory means of accounting for many of the otherwise unexplained circumstances connected with the rise and spread of febrile diseases, and nothing was said about it or ever would have been by the author, but for the circumstance that it was the occasion of directing his mind

to inquire still further into the series of causations producing fever, this being the first link in the chain. This inquiry was prosecuted until the author believed he had unravelled the whole series, and could with great distinctness perceive *how* the first cause produced the second, and it the third, and so on until the chain, in doubling back upon itself, terminated where it began, in a restoration of healthy action, or was suddenly broken by dissolution. The series of causations once clearly perceived, it appeared so plain and natural, that it seemed surprising that it should not have been understood long ago. It reminded the author of the circumstance, that in a certain part of North Carolina the cottagers had been for half a century or more filling the cracks in their log tenements with clay, rich in gold-dust, but yet remained ignorant of their wealth; but when discovered, the evidence of its existence could be easily recognized at various points. So when the true condition of the system was understood, out of which the various groups of symptoms arise which characterize fever, the evidence of its reality was soon perceived to be abundant: memory furnished a thousand incidents which had occurred in past practice which, though at the time of their happening not well understood, now all pointed with distinctness to the existence of this condition. Authors were now re-read upon the subject, and it was interesting to find how much evidence they furnish of the correctness of the above view, without having perceived it them-

selves. Many striking examples of this will be given in the work.

Having traced out the conditions of the system out of which the symptoms of fever arise, the next natural inquiry was, What is the most natural mode of removing them? Here the author's experience in the use of remedial agents, and his habit of closely scrutinizing their mode of operation, gave him great advantage in making a judicious selection of the particular means which would be most likely to accomplish the desired result. Articles were selected with whose mode of action he was perfectly familiar, and which, though simple in their properties and perfectly innocent to the constitution, were known to possess great power to accomplish the precise thing wanted to be done. Accordingly, the first case of plain, uncomplicated fever which presented itself was treated upon this plan: it succeeded promptly; the next, and the next, were similarly treated, and yielded as by enchantment; all the varieties of fever common in this climate were similarly treated, with such variations as to suit accidental complication or peculiarities, and it succeeded in all, but not with equal promptness: in that form of fever which has been the most stubborn in its character, viz., the *typhoid*, it succeeded more speedily and with more certainty, without collateral aid, than in plain common chills and fever. But, with the aid of other means to meet particular symptoms, it proved equal to the contest with all. After pursuing this plan for three years, in a tole-

rably large country practice, without calling attention to it in any way—for it was thought best to multiply evidences of the superiority of the plan of treatment beyond the possibility of being mistaken before subjecting it to public scrutiny—but being now fully convinced that his unusual success was not accidental, the author thought best to choose some location where he could have a better opportunity of demonstrating the truth of his discovery, and for that purpose moved to this city, where he has been successfully pursuing the same plan of treatment in all the febrile cases which have come under his charge, and they have been pretty numerous; for besides other practice, he purposely sought and obtained the management of many charity cases, among which, as is well known, the most unmanageable cases of fever are usually found. But notwithstanding this disadvantage, in a practice extending through more than three years, he has lost but one patient laboring under any form of febrile disease in this city, and that was the result of a cause outside of the fever. But he has not only not lost his patients, but they have almost uniformly recovered in an incredibly short time; not more than one in fifty has failed to be decidedly convalescent before the fifth day of treatment; and the author classes among his fever patients all those laboring under Pneumonia, Dysentery, Cholera Infantum, as well as the various forms of Bilious and Typhoid Fever. He is, therefore, confident he is not mistaken in the opinion that he has discovered

the true nature of the febrile movement, and has instituted a plan of treatment which very materially shortens the period which it usually runs, and in a still larger proportion lessens the mortality; in view of which he believes it to be his duty, both to his God and to his country, that he should take the proper steps to place this discovery fairly before the world.

If he had consulted his own feelings, or his private interest, he would have kept this matter concealed in his own mind, for he has ever felt a repugnance to making himself conspicuous in any respect; and his private interest would certainly have been better served by quietly pursuing this plan in his own practice, and gaining character by superior success. Besides, he was not ignorant of the fact that the world is not well pleased at being shown that it has all the time been in error, and no part of the world less so than the Medical Profession. He therefore expected to meet with the condemnation of the prejudiced, the contumely of the shallow and the self-conceited, as well as the rigid scrutiny of the prudent seekers after true medical knowledge; but having decided what was his duty in the premises, he resolved to perform it.

He accordingly, as a kind of introduction, prepared a treatise on Dysentery, which was published in the March number of the Nashville Journal of Medicine and Surgery, 1853; and although in that he said nothing about fevers in general, yet, as all scientific physicians very well know that dysentery

is a formidable disease only in consequence of the malignity of the *fever* which attends it, it was supposed that the peculiar success of the plan of treatment therein detailed would lead some to inquire whether the same plan, modified according to circumstances, might not be equally successful in other forms of fever. But this idea did not seem to strike the profession; for although many physicians in the South and West adopted this plan of treating dysentery with signal success, as is evidenced by letters received from them, yet no one appeared to have thought of carrying out the idea of adapting the same, or a modification of the same plan, to other febrile diseases; so, the next year, the author brought the subject more directly before the public in some articles published in the State Sentinel and Tennessee Organ, of which he was editor. In these articles he set forth, in a concise manner, his views of the philosophy of the *febrile movement*, and his method of treatment. Again, in the winter of the same year, he delivered some Lectures before the Medical Class of the University of Nashville upon the same subject, and was requested by that body to publish them. Again, in the spring of 1855, he read a paper before the Tennessee State Medical Society, containing an abstract of the principal topics discussed in the Lectures, which that body referred to a committee of investigation, to report at its next annual meeting. During the past summer the author has received many letters from physicians who had read his articles in the Sentinel,

and from young men who heard his Lectures and had since gone into practice, requesting him to write out his views more extendedly, and present them in some form suitable for reference. A number of them state that they had adopted the author's plan of treatment as far as they could recollect it, particularly in typhoid fever, and found it unusually successful. The author, therefore, feels himself almost compelled to accede to this request; and while "making a book," he has concluded, by way of making it more interesting and useful, to add some views on several other subjects. The contents of the present little volume will, therefore, be found to be as follows:

1. Capillary Circulation.
2. General Circulation.
3. The substance of his Lectures to the Medical Class, embracing a pretty full exposition of his views of the theory of fever and its treatment, commencing with the remote cause, and showing how it first impresses the system; then, how this first impression brings about that *condition* of the system out of which grow the particular manifestations by which we recognize this disease; then, showing how this *condition* may be removed by the spontaneous efforts of the inherent powers of the system, and how art may be brought to aid in this work, when nature is likely to prove inadequate to the task; explaining fully the means which the author has found best adapted to do this, and what have been his reasons for selecting these particular

means, and the philosophy of their action. Also, a general view of all the principal theories of fever and of diseased action that have obtained in past time, showing that all that is true in each agrees with and corroborates the peculiar views of the author.

4. Several subjects which have been acknowledged to be *mysterious* connected with the febrile movement, or the *modus operandi* of remedial agents used in its management, will be considered, and, the author thinks, explained upon rational principles according to what he has embraced as the true nature of diseased action, thus affording strong evidence of the truth of his positions.

Dr. Smith, of the London Fever Hospital, says: "When the theory of a disease collects, arranges, and points out the true relation between all the phenomena, there is good reason to conclude that that theory is sound; but when it moreover directly leads to that treatment of the malady which experience shows to be most safe and most efficient, its truth is established by every test that can be applied to it."

The explanations above referred to will be in the form of answers to the following interrogatories:

Why does the remote cause of fever act most potently at night?

Why do paroxysms of intermittent fever occur oftenest in the day?

And how account for evening exacerbations in continued fever?

How do fevers have a natural tendency to terminate in health?

How do fevers cure other diseases?

How does *emetic tartar* act in aiding or producing a *crisis* in fever?

In what does the delirium of the first stage of fever differ from that in the second and third stages?

How account for critical days?

How does inflammation sometimes cure fever?

How do contagious diseases prevent subsequent attacks?

How do topical remedies act?

Does the continued action of the remote cause, after a fever has been set up, render it more difficult to manage or dispose to relapse?

How does quinine cure intermittents?

How does turpentine act in typhoid fever?

Why are mercurials injurious in typhoid fever?

5. He will then take up each form of fever common in this country, separately, and give the variations of treatment necessary in each, and the collateral aids best adapted to secure the success of the main treatment.

6. An article on Dysentery.

7. His method of managing some of the misfortunes incident to pregnancy.

8. His mode of treating some infantile diseases.

9. His plan of treatment for catarrhal and scrofulous ophthalmia, chronic ulcers, etc.

10. Reports from a number of practitioners in the South and West who have treated fevers and dysentery upon the plan set forth in this work.

A TREATISE

UPON

FEVER, DYSENTERY, ETC.

CHAPTER I.

CAPILLARY CIRCULATION.

As the theory of fever, and the *modus operandi* of the remedies by which I profess to break up that disease, involve the idea of independent capillary action as an essential element in their explanation, I have thought that this subject demanded a more extended notice than it has received in the body of this work, and I now propose to devote a separate preliminary chapter to the investigation of this particular subject. I am aware that much difference of opinion exists among physiologists with regard to the circulation in general, including this particular part of it. Many contend that the *whole* is performed by the *vis a tergo* of the heart alone; that the momentum the blood receives from the contraction of the left ventricle carries it through all the ramifications of the arteries, through their capillary terminations, into the capillary commencement of the

veins; then, through the countless ramifications of the small veins, into the venous trunks, and along these to the right side of the heart, by which it is impelled, in like manner, through the pulmonary circulation, back to the left side, again to take the round of the general circulation. Other physiologists have thought that this was assigning too great an effect for the amount of the power, and have gone to work to hunt up some force which might aid the heart in this heavy task. Accordingly, some have thought they had found this collateral aid in muscular contraction of the arteries, others in general muscular action outside, but exerted on the blood-vessels, by which the vessels are squeezed, and as the blood cannot go backward on account of the peculiar arrangement of the circulatory apparatus, it must of necessity move forward. Others, again, have imagined that they had found in the lungs, connected with respiration, a power which is not only capable of assisting the heart to carry on the circulation, but of actually relieving it of that duty, and that the heart only acts a secondary part, merely filling the minor office of marking time. In other words, that in the lungs is the great motive-power, and that the heart merely acts as a regulator, by measuring out the blood into parcels.

Now I believe that neither of these notions is entirely true, but that each contains *some* truth, though they altogether fall short of embracing the *whole truth*. The heart was never constituted by nature such a powerful muscular body for the mere

purpose of acting the part of a regulator. The common sense of mankind points to it as the great agent in arterial circulation, and the painful sensations produced by its contractions in fever of a high grade, would itself convince the sufferer that a *force* is exercised by this organ of no insignificant power. But the very fact that wise men have brought themselves to believe that the great motive-power by which the blood is circulated through the body is connected with *respiration*, is proof of itself that there are good reasons for the belief that the exercise of the peculiar function of the lungs does, in some way, contribute to the accomplishment of the business of getting the blood conveyed through its "mysterious rounds." It is equally certain that the arteries themselves, by independent action, do also assist in producing the grand result; else how can we account for the superior momentum given to the blood in an inflamed limb, over that of the corresponding sound one? Many other reasons could be given for the belief that arteries exert an independent propelling force in the circulation of the blood through them, but, as it is not necessary to establish this in order to accomplish the object of this chapter, it will not be dwelt on. The same reason renders it unnecessary to say any thing concerning the agency of general muscular action in aiding the return of the blood through the veins; and, without being further tedious, the main object which formed the motive for writing this chapter will now be considered, viz.: that the capillaries cir-

culate the blood through themselves by their own independent action. This is proven, first, by the fact that arterial and capillary circulation are often found in very different degrees of activity at the same time.

When the system is in good health, and there is consequently a corresponding activity of all its powers, there will always be harmony of movement among all its parts. But, in disease, this correspondence of action is always more or less broken up; otherwise there could be no disease, but either a strong or a weak development of the phenomena of life; it is only when there is a broken balance, a want of correspondence between the activity of the arterial and capillary circulations, that manifestations of disease are observed. In some instances there is increased capillary action, without any corresponding activity of the arterial circulation. Blushing is a familiar example; the erection of certain tissues, constituting what is termed orgasm, is another; and the increased activity of the circulation in the secretory organs during the period of the active exercise of their functions presents another. The florid hue of the surface, and the increased cuticular exhalation which usually succeeds active exercise, and continues after the activity of arterial circulation has subsided to the natural standard, or even sunk below it, as is proven by the diminished force of the pulsations, presents another very striking example of the exaltation of capillary circulation, without a corresponding increase of the power of the heart's

action, showing that the former is in some measure independent of the latter. But do we meet with any examples of the reverse of this in health, where the arterial action is greater in proportion than that of the capillaries? I know of no such instances. But, in disease, this condition is not only frequently but uniformly present; in fact, it constitutes a *sine quâ non* to the very existence of disease; a *condition* which, being absent, disease is absent; and being present, disease is present also. In very low and collapsed conditions of the system, when the heart beats feebly and there is little force in the arterial pulsations, but profuse discharges from the cuticular and mucous surfaces, it might be thought that capillary action exceeded that of the heart and arteries; but it is not so. For, though the latter is weak, the former is still weaker; and these discharges are not *vital secretions*, but *mechanical exudations*, the action of the capillaries being so entirely suspended, and their coats so relaxed, as to suffer the more fluid parts of the blood to pass through without check; and sometimes even the more solid constituents, the red globules themselves, transuding without any rupture of continuity. I once saw a very interesting instance of this. A colt had been foaled on the margin of a ditch containing water, and in scuffling had rolled into it, where it remained from twelve to twenty-four hours, entirely submerged, with the exception of its head. Evidences of life were barely perceptible, but, under the influence of stimulants, the heart's action revived and increased to consider-

able force before there was a corresponding return of vitality in the capillaries; the consequence was, that the *vis a tergo* sent the blood through them unchecked, and bloody exudation took place from all the mucous surfaces and the more delicate parts of the external surface, as the eyes, ears, lips, etc. External stimulation, by means of warmth and friction, however, soon restored sufficient capillary action to prevent exudation, and finally restored the animal to health and activity.

Inflammations furnish another example in which arterial action is in excess, and capillary action deficient. Here we find the blood flows rapidly *to* the part, but passes slowly and sluggishly *through* it. This is also the case in the beginning of the hot stage of fever, almost to the same extent.

Secondly: capillary circulation commences before the existence of arterial action. We have an example of this in the development of every individual animated existence. In the embryo of the chick we can see the blood circulate before there is any heart or main arteries formed. Carpenter, upon this subject, says: "The first movement is *towards* instead of *from* the centre; and even for some time after the circulation has been fairly established, the walls of the heart consist merely of cells loosely attached together, and can hardly be supposed to have any great contractile power. The last of these facts may be said not to have any direct bearing on the question whether the 'capillary power' has any existence in the adult condition; but the phenomena

occasionally presented by the fœtus at a later stage appear decisive. Cases are of no very unfrequent occurrence in which the heart is absent during the whole time of embryonic life, and yet the greater part of the organs are well developed.

"It is evident that a single case of this kind, if unequivocally demonstrated, furnishes all the proof that can be needed of the existence, even in the highest animals, of a capillary power, which, though usually subordinate to the heart's action, is sufficiently strong to maintain the circulation by itself when the power of the central organ is diminished.

"It has occasionally been noticed that a gradual degeneration in the structure of the heart has taken place, to such an extent that hardly any muscular tissue could at last be detected in it, but without any such interruption to the circulation as must have been anticipated if this organ furnishes the sole impelling power."

Thirdly: it continues after arterial action ceases. The capillaries in the higher order of animals are not so directly dependent upon nervous influence as the heart for their action; consequently, they continue to circulate the blood for some time after it has ceased its motions, especially in sudden deaths. This is proven by the return of the natural color of the complexion, and often the florid hue of the surface, which is witnessed an hour or so after the patient has ceased to breathe, and all motion of the heart has been stilled. It is proven also by the continuance of some of the secretions for some time

after death, and by the removal at times of all traces of inflammation which had been sufficiently active to destroy life, so that a *post mortem*, some hours after the demise, will reveal no evidence of its having existed. And, lastly, by the fact that the arteries are uniformly found empty, and the veins full, after death. No sophistry of reasoning can break the force of this stubborn truth; for if the capillaries did not continue to act after the heart ceases its motions, the blood would remain divided between the arteries and veins, exactly as it was when the last contraction left it—except, perhaps, an inconsiderable reduction might be made in the arteries by the elastic contraction of their coats. But, in order to make it look reasonable that this elastic contraction emptied the arteries of blood, it would have to be shown that it extended so far as to obliterate their cavity, or nearly so; but we find them in fact of nearly the usual size, but still empty. The evidence, then, of the independent circulation in the capillaries is, that in health it is often increased without any corresponding increase of the force of the heart's action; that in disease it is always slower and weaker in proportion to the arterial circulation; and that it begins before the heart is formed, and continues for some time after its contractions have ceased.

CHAPTER II.

GENERAL CIRCULATION.

Though not absolutely necessary to a correct understanding or successful defence of my views of the nature of the febrile movement or its management, yet, as it has a bearing upon them, I have concluded to present a general outline of the manner in which the blood circulates through the general system, and point out the forces which I believe combine to propel it through its "mysterious round." In doing this, I will make the general capillary system the beginning point, and let what has been said suffice, at least for the present, upon the subject of capillary circulation.

The next stage of movement is the passage of the blood through the veins from the capillaries to the right side of the heart, which will be now considered.

But as the general reader (and I hope to have many such) cannot be expected to have a sufficient anatomical knowledge of the parts immediately concerned in the circulation to follow me understandingly in my efforts to explain the nature of the forces and the manner of the movements involved in the

description, I will, therefore, before proceeding farther, give a hasty outline of the parts concerned in this interesting process. The *veins* are membranous tubes, having no muscular coat, and possessing but little elasticity, which, arising from the capillaries and uniting to form larger branches, finally terminate in the right *auricle* of the heart. In most of the veins, particularly those which are exposed to the pressure of muscles when contracting, we find what are termed *valves*, a kind of membranous septum, or partition wall, so disposed as to admit the blood to flow easily forward, but offering resistance to a backward movement. The right *auricle*, or, as it may be considered, the first chamber of the heart, is a membranous bag containing a few muscular fibres in its walls, which gives it some independent action of its own. It has no well-defined valve at the point of its connection with the great venous trunk, to prevent the blood from flowing back into the veins when the auricle contracts; but this office is performed by valves in the veins a short distance back. It communicates directly with the right *ventricle* of the heart, by an opening in the walls of the latter sufficiently large to allow the blood to have free ingress into it while in the act of dilating, and during the contraction of the former. The right *ventricle* is a muscular sack, whose cavity constitutes the second chamber of the heart, and receives, as we have seen, the blood from the auricle. The capacity of this chamber is much greater than that of the auricle, and its walls are many times thicker. It is

furnished with a valve which prevents the backward motion of the blood into the auricle during its contraction.

The right ventricle communicates directly with the pulmonary arteries, through the ramifications of which the blood is conveyed into every part of the lungs, and finally emptied into the *pulmonary capillaries;* which, by their infinite divisions, bring the blood into such close contact with the air in the cells of the lungs, that gases are enabled to pass from one to the other, and so affect the blood as to change it from a dark to a bright sparkling red color, and perform whatever other changes are necessary to prepare it for the various purposes for which it is needed in the animal system. The pulmonary capillaries, by uniting into larger trunks, form the commencement of the pulmonary veins, which give passage to the blood from the lungs back again to the heart; opening into the *left auricle*, which is similarly constructed and answers a like purpose with its fellow of the right. The blood from this auricle passes into the *left ventricle*, by which, by a similar arrangement to that we have noticed in the right, it is passed on through the arteries back to the *general capillaries*, the place of our beginning. The left ventricle of the heart, however, differs from the right in possessing much thicker muscular walls; the difference is estimated to be as three to one. The arteries of the general system also differ from all other blood-vessels in having much thicker and stronger walls, and possessing

more elasticity, and having a distinct muscular coat, more or less perfect in different parts.

Having now given a general outline of the course of the blood in the circulation, and a bird's-eye view of the machinery used for its conveyance, the general reader will be better prepared to understand what may be said with regard to the forces which are concerned in carrying it on, which will now be considered.

And first: *Of the forces concerned in the general venous circulation.*

Several very distinct and independent forces are brought to bear upon the blood in the veins, which all jointly coöperate in causing it to flow from the capillaries to the heart. But the most efficient, as I conceive, of these, is the *suction power* of the heart, and this will be first considered. I am aware that this power is wholly denied by some physiologists; and by others, though acknowledged to exist, is supposed to exert but very little power: among the latter is found the justly celebrated and standard medical philosopher, Dr. Carpenter.

Carpenter admits that the dilatation of the heart may exert *some* effect in aiding the circulation of the blood in the veins, but contends that this power must be of very little force, as it "is doubtful how far the auricles have such a power of active dilation as that which would be required for this purpose." Now, I admit that the *auricle* has not this power, as it is little more than a membranous bag, a mere arrangement of the terminus of the great vein, by

which the blood may be collected for the purpose of more easily filling the ventricle when it dilates. In the dilatation of the *ventricle* resides the real *suction power*. But how, it may be asked, can the ventricle act upon the blood in the veins when their valves are shut by the contraction of the auricle? But these valves are not interposed so as to prevent the exercise of this force, as will be seen when we consider what Carpenter himself says upon the action of these parts: "The contractions of the ventricles, and that of the auricles, alternate with one another, each taking place (for the most part, at least) during the dilation of the other. But there is a period during which the auricles and ventricles of both sides are dilating together. This occurs during the first part of the ventricular diastole; for at the conclusion of the systole, the auricles are far from being completely filled, and they go on receiving an additional supply from the great veins, (a portion of which, however, passes immediately into the ventricles,) until after the *middle* of the ventricular diastole, by which time they become fully distended, and immediately contract. The contraction of the auricles is, therefore, synchronous with only the second stage of the ventricular diastole, and their dilation is going on during the whole period of the ventricular systole. Thus, while the entire period that intervenes between one pulsation and another is nearly equally divided between the systole and diastole of the ventricles, the division is very unequal as regards the auricles; scarcely more

than one-eighth of the whole being occupied in their contraction, and the remainder being taken up by their dilation." Thus we see that during more than half the time occupied in the dilation of the ventricle, the auricle is dilating also, and the valves being open, the whole force of the *suction power* of the ventricle is exerted upon the entire mass of venous blood. And let it be observed further, that it is during the first half of the dilation of the ventricle that this free access is admitted, the very time when the action of the ventricle is the most powerful; for within the first half of the whole amount of a muscle's capacity to contract, the power it can exert is vastly more than within the second half. It would seem, therefore, that this peculiar arrangement, which did not escape the observation of Carpenter, (but for which he assigns no object,) was planned by the great Author of our existence for the very purpose of giving the greatest possible efficacy to the *suction power* of the heart, acting as a *vis a fronte* in drawing the blood towards it.

But Carpenter, with others, insists that this *suction power* can be of little force, for "acting upon a tube having membranous walls will cause the sides to collapse at no great distance, and thus destroy the power." But the veins are not in the condition of *disconnected* membranous tubes; they are everywhere externally joined to the parts through which they pass, which would prevent this collapsing of their sides.

Carpenter admits the forcible dilatation of the right ventricle. He says: "The *diastole* of the ventricles, according to Cruvilhier, has the rapidity and energy of an active movement triumphing over pressure exercised upon the organ, so as that the hand closed on it is opened with violence. This is an observation of great importance; and it concurs with observations made upon the heart when emptied of blood, to show that the diastole is not a mere relaxation of the muscular fibres, permitting the cavity to be distended, but is effected by some power inherent in the walls themselves."

Another force which is often brought to bear very efficiently in aiding venous circulation, is general muscular contraction. Carpenter says, "One of the most powerful general causes which influence the venous circulation is doubtless the frequently recurring *pressure* of muscles upon their trunks." Every one has seen the effect of working the fingers in augmenting the flow of blood from an orifice in the vein of the arm in ordinary venesection.

"Another important agency," (continues Carpenter,) "has been found by some physiologist in the respiratory movement; this is supposed to draw the blood of the veins into the chest, in order to supply the vacuum which is created there at the moment of the descent of the diaphragm. That the movement in question has *some* influence on the flow of the venous blood into the chest, is evident from the occurrence of the *respiratory pulse*, long ago described by Haller, which may be seen in the veins of the

neck and shoulder in thin persons, and in those especially who are suffering from pulmonary diseases. During inspiration the veins are seen to be partially emptied; whilst during expiration they become turgid, partly in consequence of the accumulation from behind and of the check in front; and partly, it may be, in some cases through an absolute reflux from the veins within the chest. On the other hand, the expiratory movement, while it directly causes accumulation in the veins, will assist the heart in propelling the blood into the arteries; and by the combined action of these two causes is produced, among other effects, the rising and sinking of the brain synchronously with inspiration and expiration, which is observed when a portion of the cranium is removed. Several considerations, however, agree in pointing to the conclusion that no great efficacy can be rightly attributed to the respiratory movement, as exerting any *general* influence over the venous circulation." The circumstances which he refers to are the tendency of *suction* to produce a collapse in a membranous tube, which has been answered; and that, as the pulmonary circulation is within the walls of the chest, therefore *suction*, or the pressure of the atmosphere, can have no action in affecting the pulmonary circulation, which I think can be answered presently. The blood is then returned through the general venous circulation to the heart by the following forces: 1. The impulse received in passing through the capillaries by their own proper action, or by the *vis a*

tergo of the heart in certain conditions of the capillaries. 2. The pressure of the muscles while contracting. 3. The combined *suction power* of the diastole of the right ventricle of the heart, and the expansion of the chest in inspiration.

The blood, having reached the right ventricle of the heart, is, by the force of its contraction, sent through the pulmonary arteries into the lungs; receiving also, perhaps, some additional impulse from the elastic and muscular contraction of the arteries themselves.

But as the right ventricle has not nearly the muscular power of the left, as is evident from its having much thinner walls, and from the fact that the impulse of its contraction is so much less than that produced by the left that we are not sensible of it at all, it is therefore almost certain that there is yet some other power concerned in sending the blood into the lungs besides those already named. It is true that the distance which the blood has to be propelled is not so great in the pulmonary as in the general circulation; yet, when we consider the immense number of bifurcations which take place in the pulmonary arteries, so as to bring the blood into a state of division suitable for entering the minute capillaries which expose it to the action of the air through the delicate walls of the air-cells, it must be confessed that the power necessary to propel the blood through the pulmonary round in a given time cannot be much less than that required to carry it round the circle of the larger circulation. For it is

not so much the distance which causes a loss of force of momentum in a fluid, as the interruption it may receive from the frequent division of its current occasioned by a multiplicity of bifurcations in its channel.

We are therefore forced to the conclusion that nature has prepared some additional force in aid of the heart and arteries.

Now, this force must necessarily be found connected with, or growing out of, the exercise of the function of respiration. Lately a theory was advanced by a distinguished lady, and adopted and defended by some medical writers of considerable distinction, which not only finds in the function of the lungs a power equal to any deficiency which may be found in the action of the heart and arteries to carry on the pulmonary circulation, but equal to the task of performing the whole business of that, and the general circulation also, with but little assistance from any quarter. But, independent of this theory, there is still reason to believe that the lungs do act some considerable part in the pulmonary circulation; and I think that the manner of this action can be quite easily made apparent and explained upon well-established philosophical principles. The act of respiration itself, as has been shown in another place, will necessarily have a tendency to produce a flow of blood to the lungs as a purely mechanical result. In inspiration the cavity of the thorax is dilated by active muscular power. This is done by certain muscles attached to the shoulders

and other fixed bony points at one end, and taking hold of the ribs and cartilages composing the anterior walls of the chest by the other, in such a manner that a shortening of their body will necessarily elevate the ribs and increase the capacity of the space within them. This enlargement is also much increased by the contraction of the diaphragm, which is a muscle of immense power, the fibres of which are attached to the back and the lower margin of the inferior ribs, the cartilages and breast-bone, and, converging toward the centre, terminate in a strong tendinous substance, so as to form a complete division between the cavity of the chest and that of the abdomen. But this muscular partition is not stretched across the body like the head of a drum, but is quite too large for the space when the muscle is at rest, and has its centre pushed up into the chest in the form of a cone. This arrangement is such that when the fibres of the muscle contract, the centre of the diaphragm is drawn downwards, producing the same mechanical effect as the moving of a piston. The action of all these muscles produces a considerable increase of the space contained within the walls of the chest, which is occupied by an immediate expansion of the lungs, causing a rush of air to take place into the air-cells, constituting the act of *inspiration*. But the muscular contraction almost immediately subsides, and the elasticity of the walls of the thorax brings them back to their former position, expelling the air and constituting the act of *expiration;* which, however, when

at all difficult, or desired to be forcible, is aided materially by the contraction of the abdominal muscles, which, by being attached to the ribs, draw them forcibly down, and at the same time, by compressing the contents of the abdominal cavity, push its movable contents forcibly up against the diaphragm, causing it again to act upon the principle of a piston by encroaching upon the cavity of the chest.

Now, it is just as certain as any truth in philosophy, that the expansion of the chest will exert precisely the same force in causing *blood* to flow into the cavity as it does upon the air, and that the contraction of the thorax will also exert the same propelling force upon the *blood*, and expel it with the same power that is brought to bear upon the aerial fluid; that is, that the expansion and contraction of the chest, by which the *air* is drawn in and forced out, must, by a physical necessity, produce the very same effect upon the *blood*. How great this power is, cannot be accurately estimated. In quiet respiratory movements the power exerted is very little, but in laborious breathing it is very considerable; and the same causes which call for rapid and extensive movements in connection with the true respiratory function, will also call for the same unusual efforts in aid of the circulatory function; in other words, whenever there is a necessity for the use of *much air*, there is a like necessity for *much blood* in the lungs; so that it is a happy arrangement that one set of motions will serve both purposes at the same time.

It may be objected that as the movements of respiration and the action of the heart do not take place synchronously, the first being much slower than the latter, therefore these forces cannot agree in producing the same result. But this is a mistake which can be removed by a moment's reflection. During inspiration, the blood is forcibly impelled along the pulmonary arteries into the expanding thoracic cavity, independent of but in aid of the force it has received from the contraction of the right ventricle of the heart; while the blood in the pulmonary veins, though feeling the same power, cannot be drawn backward, by reason of the valvular construction of the vessels which contain it. But when the chest is contracted in the act of expiration, the *air* is not only expelled from the lungs, but the same pressure which forces it out, also operates with equal power upon the *blood* contained in the pulmonary vessels; but as the blood cannot be made to flow backward in the arteries, it must flow forward in the veins, and lessen by so much the whole amount contained in the thorax, making room for a fresh supply to be driven in by the next expansion. It may be objected that this arrangement will make the influx of blood into the lungs very unequal. But that is precisely what we might expect of the circulation in the lungs, *à fortiori:* nature always does things right, and at the right time. The only object for getting the blood into the lungs at all being that it may be brought into contact, or at least into very close proximity with the atmospheric air, so as to

have certain changes performed essential to its usefulness in the business of ministering to the wants of the system, consequently it is not wanted in the lungs except at the precise moment when the air is there too, ready to meet it. And the instinctive feeling which prompts to unusual inspiratory effort, is doubtless as much owing to a sense of want of blood in the lungs as it is from a feeling of lack of air, and is at times occasioned principally from one cause, and at other times mostly from the other. In a very rare or very impure atmosphere, and when the ingress of the air is impeded by bronchial obstructions, these unusual efforts are owing to a call for more *air;* but in incipient syncope, in prostration from hemorrhage, in capillary obstruction in febrile diseases, and in atrophy of the heart, the call is for more *blood*, rather than more *air* in the lungs. And the benefit derived from forcible inflation of the lungs in restoring suspended animation, must be accounted for principally by its direct effects upon the pulmonary circulation.

Having read the preceding observations on the forces concerned in the pulmonary circulation to a medical friend, he observed that he could not see the justice of the conclusion that dilatation and contraction of the cavity of the thorax must necessarily influence the circulation of the blood through the lungs. He said he could readily perceive how the expansion of the cavity of the chest must cause the *air* to rush in, because *it* has direct access through the medium of the bronchia into this cavity; but that the *blood* is dif-

ferently situated—that it is excluded from the real cavity of the lungs, being confined in the vessels. I replied that his objection was imaginary, and founded in a misconception of the philosophy of the effect of the vacuum; that the pressure from without being equal on every part, if an empty bladder were placed in the receiver of an air-pump, and a tube arranged so as to connect it with a fluid without, the exhaustion of the air would cause the fluid to rush into and distend the bladder, causing it to occupy the space in the receiver made by the displacement of the air; and that if there were a hole in the receiver at the same time, the working of the pump would cause the fluid to still flow into the bladder with exactly the same force with which the air pressed in through the aperture in the receiver; but that I could refer him to a striking example, which to him must be quite familiar, of the effect of a vacuum upon fluids contained in vessels, viz.: the dilatation of the blood-vessels under the cupping instrument, when performing the operation of dry-cupping; and though I freely acknowledged that in the act of inspiration there is no vacuum formed in the lungs, yet there is an attempt to form one, which is prevented by the rushing in of blood as well as air. Now, although it is plain that the force exerted on the blood is not nearly so great as if the cavity of the chest had no external opening, yet it is a *physical truth* that the external pressure is just as operative in forcing the blood into the pulmonary vessels, during the expansion of the chest in the

act of inspiration, as it is in propelling the air through the bronchial tubes into the pulmonary vesicles. These explanations silenced my friend, if they did not convince him.

Thus we see that the conclusion is incorrect that the respiratory movement can have no effect upon the pulmonary circulation. But, as I have already admitted, this power can have but small force in ordinary quiet breathing, yet it is a most important resource which may be called on in case of difficulty, and affords our only chance of starting the blood again after the circulation has been suspended by strangulation or asphyxia, or in still-born infants. But still, this falls far short of furnishing the amount of force which we have seen is lacking to carry on the pulmonary circulation, and we must therefore yet push our inquiries for further aid. Now, the theory of Mrs. Willard presents us with the very power which we are seeking. Her theory seems to be substantially this: That in the act of inspiration, the play of chemical affinities which takes place between part of the constituents of the blood and of the air causes an amount of latent caloric to be set free sufficient to expand the blood in the capillaries so as to force it into vigorous motion; and as this motion cannot be backward, it must be forward.

Now, that heat is evolved by the action of the function of respiration, is held to be a fact by most physiological writers of any eminence, and that heat produces expansion is a philosophic truth, and that expansion of a fluid contained in a vessel must pro-

duce motion is equally undeniable; so that this constitutes a moving force upon the blood in the pulmonary vessels, is just as certain as that heat is an expanding agent. But the measure of this force will be difficult to estimate even proximately, as it will depend upon the amount of heat evolved, which is itself an unknown measure; but that it is considerable, is more than probable—sufficient at least to supply the lack of power of the other forces.

Whoever has seen the *smoke* arise from a stream of arterial blood, as it spouted from the subclavian or aorta, will not be ready to sneer at the idea that part of it has been converted into *steam:* boiling water, issuing from the spout of a tea-kettle, does not throw off near the same amount of vapor. The reason why steam, escaping from arterial blood, affords a denser vapor than that arising from boiling water, is plain: it is simply because the former has less heat to lose before condensing than the latter. Water boils in *vacuo* at 78°; hence, as the blood is in vacuo in the lungs, steam may be formed very readily at the usual temperature of the human body; but when it escapes into the air, it becomes instantly condensed; hence the *fog* which arises from a stream of arterial blood.

I presume Mrs. Willard never saw the murderous knife plunged into the chest of the squealing porker, or she would not have failed to notice this strong evidence of the truth of her theory. But she is not lacking of proof, as any one will find who reads her modest-looking little book. The scoff of

incredulity proceeds alone from those who never read it.

I would dwell longer on this circulating force, but that I have already more than occupied the space I allotted to the subject of the circulation, and must therefore close, after taking a very brief view of the general arterial circulation.

The blood, in obedience to the forces enumerated, reaches the left auricle, or third chamber of the heart, from which it is drawn into the left ventricle, or fourth chamber, during its dilatation; and, by its contraction, aided to some extent by the elastic contraction of the arteries, and *perhaps* by some real muscular contraction of the arteries themselves, and by the expulsive force of the respiratory movement, and whatever impulse it receives from colorific expansion, is made to pass from the heart through the various ramifications of the general arterial system, until it reaches the capillaries.

I cannot dismiss this subject without taking some notice of the positions assumed by Neil Arnott, M. D., in his "Elements of Physics," which is now a standard work in our colleges. He assumes the broad ground that the blood is driven the entire round of the circulation by the *vis a tergo* of the heart, and talks quite *sneeringly* of the want of a knowledge of the physical laws by those medical writers who hold a different opinion. Now let us for a moment examine his reasonings, and point out some of the absurdities of his conclusions.

After stating, in unqualified terms, that the heart

alone is concerned in propelling the blood, when he comes to capillary circulation he admits, with Carpenter, that these vessels *control* the passage of the blood through them, and that they have independent action, and refers to the act of blushing, of the turgescence of the erectile tissues, of the increased flow of blood to the glands when actively secreting, etc., for evidence; and yet, in the next breath, as it were, asserts that the blood is forced back through the *veins* by the impulse it had received from the contraction of the ventricle of the heart. Now by what law in *physics* can he recover a lost *impulse* without a renewal of the *force?* As well might he contend that a ball projected along the ground until it reached a stream of water, and then carried for a time by the current "whithersoever it would," could leave the water and return to the place of starting without the intervention of any new force: the idea is too absurd to merit a serious refutation.

Again, by way of proving that the blood returns to the heart, in consequence of the impulse it received at starting from it, he says that the experiment has been tried of dissecting up the companion artery and vein of the leg, and placing a ligature around the limb, so as to prevent the passage of blood in other vessels, and one around the insulated vein, and making a small opening below; then, by pressing upon the artery with the thumb and finger, and so compressing it as to prevent the passage of blood, it was observed that whenever such pressure was made, the blood ceased to flow from the orifice

in the vein. Poor Richard's saying, that "always taking out of the meal tub, and never putting in, will soon come to the bottom," will explain this experiment sufficiently. Again he says, that if a ligature is applied to a vein, as in bleeding in the arm, the blood is seen to flow with force from a puncture below it; but did he not know that all admit the force of capillary action in propelling the blood into the small veins? and that muscular contraction, and even rigidity, powerfully aids it in the extremities? He forgot, too, to notice the fact that when pressure is made upon a vein, the blood above the point of pressure, and below the nearest valve, will soon disappear, and the vein become empty, showing that there is a *vis a fronte* acting upon it. He also denies the power of the respiratory movement in aid of the circulation, and, for proof, refers to the experiment of the action of a pump upon yielding tubes; which I have already answered, by denying that the veins, in a living body, are in the condition of unsupported membranous tubes, but that they are kept expanded and held *in situ* by their connections with contiguous tissues, especially in deep-seated parts, while on the surface, and especially in the extremities, they possess walls of considerable firmness. Arnott himself admits that the superficial veins of the leg have nearly the firmness of the arteries. It will not avail to say that veins are found collapsed after death; for then the surrounding parts have lost their vital tonicity, and are themselves relaxed:

so that this much-noticed experiment proves nothing at all. Arnott admits the suction *arrangement*, and says it *might* be equal, in quiet breathing, to a force sufficient to raise the volume of blood one inch, and in laborious breathing it *might* amount to twelve inches. Now this is as much as I contend for in health, and when the body is at rest, other forces are fully sufficient; it is only during active exercise, and when the other forces are deficient from disease, that the aid of the respiratory movement is needed, and a force equal to the task of raising the volume of blood twelve inches is more than equivalent to the power needed to do the entire business of circulating the blood in the recumbent position, and equal to about one-third of the power needed in the erect position. So that, after all his efforts to disparage the attainments of others as to their knowledge of the laws of physics, as applicable to this subject, he at last grants all that can be reasonably claimed by any one, besides having displayed a good deal of egotism, and a little fallibility of judgment.

It is true, he only admits the existence of the suction *arrangement*, but infers that it is not exercised, because, if it were, it would produce collapse of the veins. But is it not *physically* certain that this *arrangement* must of necessity exert suction force upon the blood equal to its mechanical capacity? and is it not also certain that the veins are *not* collapsed by the operation of this force? It follows, therefore, that they are not in the condition

of unsupported membranous tubes, and that the respiratory movement does perform the part in the circulation of the blood which it, as a mechanical operation, is calculated to do according to established physical laws, for Arnott says that "nature does nothing in vain."

Arnott refers to certain experiments which prove that the blood flows through the arteries with a power equal to four pounds to the square inch; and taking it for granted that this power is all exerted by the left ventricle of the heart, and estimating the cavity of this ventricle to be equal to ten square inches, he comes to the conclusion that it contracts with a force at least equal to forty pounds. Now let it be remembered that the heart is suspended in the cavity of the chest, so as to admit of free motion, and that its fibres are so arranged, that when they contract, the lower end is drawn upward and to the left, with a force equal to the power of contraction, and that this organ is closely invested by its sack, which is in immediate contact with the lungs, and that, consequently, the whole force of its motion must be received by the lungs; and I would suggest that the reader contemplate for a moment the effects of a forty pound hammering upon any of the tissues of the body.

The arm or leg can be struck hard enough by a common tallow candle to cause great pain and soreness, if the tapping be continued long, showing that *human nature* could not stand a continual beating of a forty pound power. And this is the esti-

mate of the ordinary force of the arterial circulation. What must it then be while "running race," or in high grades of fever? I think this Doctor of Physics must, upon "sober second thought," admit that it is much more reasonable that the *respiratory movement*, and the evolution of caloric during that process, do produce their natural effects, as mechanical and chemical powers in aid of the heart's action in circulating the blood, rather than that the thoracic walls can be hammered with a forty pound force, and not become sore or complain.

This multiplicity of forces is a most beneficent arrangement, by which the chances of human existence are greatly increased; for as life must inevitably be lost by even a momentary entire stoppage of the circulation of the blood, had its continuance wholly depended upon any one force, any accident which would, even temporarily, interfere with the performance of the function of the particular organ in which this force resided, would put an instantaneous end to vital existence. But by the present arrangement of division of service, any one of the forces may be materially deficient or wholly suspended for a time, and yet the circulation be carried on by the others until the machinery can be repaired, and the balance of duties again adjusted. This is not merely a plausible conjecture, but is seen actually occurring under the eye of the physician in many instances.

If the circulation, for example, had been carried on by the force of the heart alone, every disease of

that organ would immediately jeopardize life; but we find that persons have not only lived, but experienced a tolerable degree of the enjoyment of life, with a condition of the heart which would seem to be wholly incompatible with its ability of being in any degree useful as an agent for propelling the blood. *Post mortems* have revealed the fact that the heart had for a considerable time been little more than a hollow bone, the walls being as immovable as a well-ossified skull; or that, by a process of softening, the integrity of its muscular structure had been so completely broken down, that it could have been no more useful as a propelling agent than a bundle of fat. I once saw an instance of this myself, in which the heart was so soft that the finger could be thrust through it with the ease with which it would penetrate a well-boiled turnip. The case was a negro man who had fallen dead in the act of walking through the house, while in the discharge of his ordinary business, and was brought by his master to the anatomical amphitheatre in Lexington, in order that a *post mortem* might reveal the cause of his death. Now with this condition of the heart, which certainly rendered it incapable of aiding in the circulation, his master informed us that, though rather short of breath, he had been able to attend to the common duties pertaining to a house-servant up to the moment of his death. Again, had the circulation depended wholly upon a force connected with the pulmonary function, its stability, not to say its

existence, would have been put to hazard by every interruption in the respiratory movement. But we know that respiration may be performed very imperfectly, and yet the blood flow on with but little interruption. A very interesting case once came under my observation, illustrative of the facility with which nature enables a coöperating organ to perform its own share assigned to it in a partnership operation, and also do the work of its partner when it chances to be unable to do its duty. A gentleman had lost almost the entire right lung by a pulmonary abscess, causing that side of his thorax to shrink, and the shoulder to drop down very perceptibly, but the circulation went on about as usual, and in the course of a few years had increased to an extraordinary vigor. At this time I found his pulsations much stronger than natural, and the action of the heart evidently greatly increased in force. He appeared to be in perfect health, and was able to endure an unusual amount of fatigue. I informed him that, in consequence of the loss of so much lung, the heart had more work assigned to it than common, and that it, like the blacksmith's arm, had grown stronger by the increased labor, and that I feared it was acquiring a dangerous degree of power, which might end in apoplexy or hemorrhage of the lungs. But he did not profit by my advice to avoid heat and fatigue, and a few months thereafter fell insensible in the harvest field, and died in a few hours. Now I suppose, had nature formed the heart with sufficient power

to drive the blood by its unaided force through the entire round of the circulation, it would indeed have been a dangerous monster, which would have threatened the integrity of the system every time it happened to become unusually aroused. Then again, had the circulation been entirely dependent upon the force of the heart or the lungs, or both coöperating, every momentary suspension of the action of these organs from syncope, concussion, etc., would have terminated life, no power in that case remaining by which those organs could again have been excited to action. For did not the *capillaries* continue to circulate the blood for some time after respiration and the heart's action ceases, the blood would at once be arrested in the vessels, no nervous power could be manufactured, and the whole machinery of life must inevitably stand still for want of motive-power; but the capillaries, by acting as a balance-wheel, keep the machinery in motion until the great driving forces, the heart and the lungs, can be again brought into exercise. In viewing this wonderful arrangement for the maintenance of life, one is almost forced to exclaim, O, the wisdom and goodness of God!

CHAPTER III.

FEVER: ITS NATURE AND PHENOMENA.

FEBRILE diseases have, in every age and country, formed the principal outlet of human life. Sydenham estimated that in his day two-thirds of the human family died of fevers. At this time, I suppose the mortality from this cause is not so great; perhaps not more than one-half now die of fevers. But this is sufficient to give the subject paramount importance in the eyes of the physician; and any plan which would materially lessen the mortality, we would reasonably suppose, would be seized on by every one who had a spark of philanthropy in his composition.

Suppose a plan were adopted which would stop this outlet to human existence: it would save in one year, in these United States alone, at least half a million of lives; a number greater than that which composed the entire belligerent forces in the Crimea. But a plan which would save half of this amount would in all fairness entitle the discoverer to be classed among the prominent benefactors of man-

kind. And this, yes, more than this, I profess to have discovered, and propose to explain. I will not only speak of the means used, and present the evidence of their success, but trust that I shall be able to prove, by sound philosophical reasoning, that there is a *fitness* between the means and the end; and that the remedies *ought* to do exactly what I have proved by experience that they *will* perform. But whether this important discovery will be duly appreciated, is yet to be ascertained.

I am aware that we are all slow to acknowledge any thing to be great which is the work of a contemporary, and especially of a fellow-citizen and an equal: it requires the charm of distance, of time, or space, to throw around any great discovery that prestige which commends it to our acceptance. But, of all ages of the world, this is the time; and of all places, this is the locality, in which we might most reasonably look for something great to originate; for I shall not deal much in the imaginary when I say that this is the most enlightened age of the world's history, and these United States contain more intelligence than any other country which the sun looks upon; and Tennessee is the centre State of this great Republic, and Nashville is the *sensorium commune* of Tennessee.

But, without further preliminaries, I will proceed to the subject of this chapter.

In order to a correct understanding of any derangement in complicated machinery, it is essentially necessary to possess a thorough knowledge of

its construction, and the philosophy of its operations. With regard to the mechanism of the human system, and the manner of its natural movements, I shall presume that the reader is already well informed; and will therefore only stop to give a general summing up of the most important points.

For example, we have the osseous system, forming the framework; the fibro-cartilaginous system, to invest the bones and tie them together; the muscular, whose office it is to act upon the bones and other parts, and put them in motion; the glandular system, to prepare material for ingress and excretion; the circulatory, by which material for reparation is carried to every part, and the wornout particles conveyed from every part; the cerebrum, where thoughts are manufactured and sensations recognized; the organs of sense, which serve to put this thinking apparatus into connection with the world; the cerebellum and its spinal prolongation, where motive and other powers are generated for propelling all this complicated machinery; the nerves, which serve as electric wires to transmit intelligence or influence to the great mental or sentient emporiums, and return the proper responses; then we have the cuticular system, which forms a covering for the whole, and a lining for all the hollow viscera which have an external opening; and, lastly, we have the cellular system, which serves the important purpose of connecting all the other parts together, of filling up irregularities and unoccupied spaces, and giving to the whole

the proper form and pleasing contour which is presented by a well-developed human body.

Now, in order that we may have a perfect manifestation of the phenomena of life, all these parts must be fully developed, and in a healthy condition, and each perform its function well. It is evident that if any part be preternaturally developed, or act with unwonted power, a loss of harmony must be the consequence, and the whole machinery be caused to work imperfectly or unnaturally. If all should work with *increased* power, we would certainly have a high development of the phenomena of life; and if all should act with *diminished* energy, we would have a weaker development of life; but so long as there remained harmony of action between the various parts, in neither case would we have *disease*. I wish to impress this particularly upon your attention, for some important inferences will be drawn from it. I therefore reiterate, that neither increased nor diminished action constitutes disease, so long as there is harmony maintained between the various organs. Example: take a young man whose system is fully developed; place him under the influence of the various excitants addressed to each organ and sense; let his stomach be stimulated by the most savory food and exhilarating viands; his hearing enraptured by exciting music; his smell regaled by the most delicious odors; his eyes wander over a galaxy of captivating beauty; his touch vibrate to the magic influence of contact with the embodiment of female loveliness; his muscles strung to their

highest tone by the whirling dance; his sexual orgasm thrilled by the uncovered developments of feminine charms; his intellect excited to its highest capacity by sallies of wit, sentiment and humor from his bewitching partner. Look at him! His eye flashes light; intelligence and zest of enjoyment beam from every feature; his whole form is full from centripetal action; his step is as elastic as the steel bow; his heart beats vigorously, and sends the red current coursing through the minutest vessels. Is he sick? He never could feel better. But will not disease ensue from this state of excitement as a consequence? It may, or it may not. If his stamina is good, if his organs are well balanced, this state of high excitement may be indulged in a thousand times with impunity; but if there should be some weak point, some imperfectly developed organ, it may suffer under the pressure, become exhausted by the excessive tension, and afterward be incapable of performing well its part; disturbance in the play of the machinery of life be the consequence, and disease be set up, more or less dangerous in proportion to the importance of the suffering organ to the continuance of life. By this disturbance in the play of the vital forces, resulting from the broken harmony among the organs, some are thrown into increased action, but others are debilitated. There is never general increased action; for then would there be harmony in the working of the machinery, and a high state of life would follow, and not disease. But it is philosophically certain, that no one organ

can act with unusual power for a considerable length of time without exhausting its energies, and occasioning subsequent debility in itself; and also, by breaking up the harmony, and calling upon the general supply for more blood and more vital influence than are allotted to it, either the whole system must feel the shock, or, what is more common, some other organ, from accidental or constitutional weakness, will suffer in particular. I grant that increased action in any one part does not, in itself, constitute disease; it is only when over-action has produced debility in itself, or some other important part, that disease may be said to commence. This, then, narrows down the matter to a single point, that disease consists in *debility* in one or more organs, and that there can be no disease without debility; and that this debility must of necessity be partial. A general feebleness does no more constitute disease than general power. The machinery may be quite fragile; have but little momentum; afford but feeble resistance; but as long as there is correspondence among all its parts, there will be harmony of its movements. So of the human system: so long as the various organs work in harmony, so long there can be no disease. The individual, though capable of little mental effort, of trifling muscular exertion, having weak perceptions, and small manifestations of desire or passion, yet has a feeling of well-being; has neither pain, nor sickness, nor fever, nor any other manifestation of disease; and this will continue to be the case, though the powers of life should

continue to become more and more feeble, until the machinery stops its motion. We would then have a case of natural death, but not a case of disease.

This view of the nature of disease in general forms the key by which I propose to unlock the mystery of the febrile movement in all its forms, and it is upon the theory based upon this platform that I have built a mode of practice which has succeeded in my hands, and may succeed in others, in divesting this formidable family of maladies of nearly all its terrors. But I am not content with knowing that I can *abort* all the forms of fever common in our country. I wish to know *how* the thing is done, and to be able to point out a philosophical adaptation of the means to the end. I beg indulgence, therefore, for being a little tedious in the defence and illustration of the first principles upon which my plan is founded. And here I will observe, that I shall not stop to point out what part of the views I shall present is original, and what part is borrowed, but will give them as a whole; and if you should perceive them to be altogether patchwork, still I think you will concede that the parts fit well together, and form a fabric sufficiently expansive to cover the whole ground.

I repeat, that all disease is dependent on, and has its very existence in, debility; general disease arising from debility in some general tissue or system of organs; local disease, from debility in one or more of the structures of which the part affected is composed. For illustration, I will refer to a familiar

example, viz., local inflammation of the cellular tissue. From observations made under the inspection of the microscope, the following phenomena have been observed to follow the application of stimuli or irritants to any part suitable for observation—increased action of the capillary vessels, and increased momentum of the blood; and if the irritation be now removed, the parts soon return to their natural condition, showing clearly that disease or disordered action does not necessarily follow increased action. But if the stimulation be continued, the capillaries soon have their vitality so much exhausted that they fail to respond to it, but, on the contrary, dilate mechanically to the expansive force of the blood, admitting a much larger amount than usual, which flows on slowly and sluggishly; and if the stimulation be now increased, instead of quickening the action of the vessels, they become entirely torpid from exhaustion, and the blood stagnates and presents the well-known phenomena of gangrene. Carpenter says, "If whilst we watch the movements of blood in a companion artery and vein, we draw the point of a fine needle across them three or four times, without apparently injuring them or the membrane over them, they will both presently contract and close; then, after remaining for a few minutes in a contracted state, they will begin again to dilate, and will gradually increase in diameter until they acquire a larger size than before the stimulus was applied. When in this condition, they will not again contract on the same stimulus as

before; the needle may now be drawn across them much oftener and more forcibly, but no contraction ensues, or only a trivial one, which is quickly followed by dilation: with a stronger stimulus, however, such as that of great heat, they will again contract and close, and such contraction may continue more than a day before the vessels open and permit the blood to flow through them." We see, then, that notwithstanding the pain, soreness, swelling, throbbing, all seem to indicate *over-action* in inflammation, it is nevertheless essentially a disease of *debility;* and all these concomitants can easily be accounted for consistently with that fact; more than that, they all grow out of it, are produced by it. The redness is the consequence of the accumulation of blood, which we have seen is occasioned by the debility of the vessels, causing them to yield to the pressure of the blood. This engorgement produces the swelling; it also causes the pains; for the nerves in the coats of the vessels are necessarily stretched by the enlargement, and those in the intermediate cellular tissue are necessarily compressed, and of course pain and soreness are the consequence. The engorgement also occasions the throbbing; for the blood, not finding a vent through the capillaries, is accumulated in the small arteries, and a concussion more or less powerful in proportion to the heart's action is felt during every pulsation. The obstruction also causes the heat: any of you can at once prove to yourselves that a simple obstruction to the circulation of the blood causes heat, by draw-

ing a ligature around any of your extremities, so as to obstruct the return of the blood.

The *rationale* of the curative means most relied on for subduing inflammation, also goes to establish this view of its nature:

1. Equable pressure obtained by the roller bandage, by which the quantum of blood flowing to the part is lessened, and the coats of the vessels supported, and thus enabled to reäct and assume their former size.

2. The application of cold and other astringents, which produce the same effect in a different mode.

3. Stimulation, by which the vitality of the capillaries is aroused, and they made to contract upon their contents.

A stimulus of a different kind from that which produced the debility, or even a milder application of the same kind, will often prevent an inflammation, or speedily arrest it when set up. Example, a frozen extremity: the application of ice-water will often restore the suspended animation; and it is well known that a degree of heat a little less than produced a burn will "draw out the fire," in common parlance; that is, it prevents the inflammation which would otherwise follow. Spirits of turpentine, camphor, and other stimulants act upon the same principle. We all know the efficacy of cayenne pepper in subduing inflammation of the throat; of creosote, oil of cloves, etc., for allaying inflammation of a carious tooth; of strong alcoholic spirits for alleviating mercurial inflammation of the mouth,

or for superficial erysipelas. But why multiply examples? The common sense of mankind has in all ages led them to cure inflammation by stimulation. But, says an objector, how will you account for the soothing effect of emollient poultices, upon this view of the subject? Very easily. The tension of the parts causes the pain, and the irritation of the pain causes an increased flow of blood to the part; the poultice relaxes the skin and subjacent cellular tissue, enabling them to yield to the pressure of the engorged capillaries, thus taking off the pressure from the nerves which caused the pain; less blood flows to the part, as a consequence, and the inherent vitality of the capillaries returning enables them to resume their usual calibre and regain their natural action in circulating the blood.

Perhaps it would be well for me here to make a few observations upon what I mean by the *capillaries*, and their office. You no doubt have been already taught that they form the connecting link between the arteries and the veins; they also are the secretory vessels which separate the various materials intended for the use of the system, or for excretion from the system; they also separate from the blood the material for building up the system in its various parts; also separate the worn-out particles from the various tissues, and convey them into the veins or lymphatic vessels, to be forwarded to the appropriate organs of excretion. In short, the capillaries do every thing that is done in the way of building up or taking down the system.

But to return. Having shown that *inflammation* is essentially a disease of debility, it will be easily demonstrated that *fever* is also; for fever is, "to all intents and purposes," an incipient inflammation. Common observation indicates, and the microscope demonstrates, that the condition of the capillaries in fever, and the first stage of inflammation, is identically the same. We have but to observe the phenomena of a case of fever, and analyze the symptoms as they consecutively occur, to become assured that this, as well as inflammation, has its foundation in debility—begins in it, continues with it, and sinks under it, or ceases with its removal. Let us, for illustration, take a case of common symptomatic fever, and consider its cause, its inception, its development, and its cessation, and see if we cannot show up the footprints of debility throughout its entire march. A boy runs heedlessly over rocks, and bruises his heel; inflammation, and, of course, swelling ensue; but the hard integument will not yield, and the consequent pressure upon the nerves, with which the part is abundantly provided, causes intense suffering; and the cellular tissue, being here dense and fibrous, suppurates slowly, so that the case is protracted long enough for the painful impression carried by the nerves to their spinal origin to stimulate that organ so as to occasion debility in its structure; which, as in incipient inflammation, is attended with engorgement of the capillaries which enter into its composition, causing a retarded circulation of the blood through

its vessels; the nervous matter is pressed by the distended vessels, and its energies cramped, so that it sends out deficient or morbid nervous influence, which is manifested as follows: The boy feels listless, weak, and mopish; complains of aching in his back and limbs, soreness of the flesh, and rigidity of the muscles, which make him averse to move; the organs of secretion work languidly and unnaturally; there are fitful sensations of heat and chilliness; his appetite is capricious, feeling hungry, but refusing food when presented; his heart beats languidly or rapidly, with a weak and variable motion; the congested capillaries give a duskiness to the surface; his eye is watery, weak, and lustreless, and his intellect clouded. But the blood, not finding a ready passage through the congested capillaries, necessarily accumulates in the large vessels; the heart is soon made to feel the stimulus of distension, and is excited to increased action; but, though it labors with augmented force and frequency, and throws the vital fluid bounding to the surface, the engorged capillaries offer an obstruction, and it reäcts still further upon the heart. The patient now complains of heat, restlessness, headache, throbbing of the temples, dryness of the skin, mouth and throat, nausea, or sickness of the stomach; in short, of all the distresses usually attendant on a well-developed case of fever; every feeling, every sensation is unnatural; every function is deranged; every secretion is morbid.

Here let us pause a moment, and consider the

real condition of our patient. Has he too much blood in his system? Not an ounce more than he had a few hours ago, when he was laboring under the period of depression. Is his blood charged with some poisonous matter that is fretting the heart into this tumultuous action? It is the same blood that flowed tranquilly through his veins a few nights since, when he slept as quietly as an infant, and breathed so softly that a feather would not have quivered from its force; the same that, in his waking hours, gave buoyancy to his feelings, and filled him with the zest of young life's enjoyment. There is, then, not too much blood; neither is his blood poisoned; but it is obstructed in its circulation; and the proper method of relief is, not to lessen its quantity, nor to purge away part of its constituents, but to stimulate the capillaries, restore their vital activity, make them contract upon their contents, and thus remove the obstruction to the onward current of the blood; and the heart, being relieved from the stimulus of distension, and the nervous matter from the stimulus of compression, all the over-action will quietly subside, all the morbid sensations will disappear, and the machinery of life run on smoothly again, without having sustained any loss of *power* by the means that have been used to set it right. But the first consideration in this case should be to remove the cause; for, while it is still operating, it will be very difficult to keep down the effects. Every one would agree that the abscess should be opened to relieve the tension, and poulticed to allay the irrita-

tion; and then, if the system had been previously healthy, it will perhaps return again by its own inherent powers to its former state of well-being. And, I presume, it will be conceded that it would be just as important to remove the cause in every other form of fever as in this, if it could be done; and I hope to be able to show that the cause of at least some other fevers can be as certainly and as speedily destroyed as the one we have considered.

But to progress with the matter immediately under consideration; that is, that all fevers are dependent on *debility*. The next most appropriate form of fever that presents itself for illustrating this position, is child-bed or puerperal fever. In this case the whole system has received a severe shock; there has been exhausting muscular effort, severe and perhaps protracted suffering, and a considerable drain of the vital fluid—all calculated to produce general debility and irritability. An important organ has suffered a serious lesion, and its capillaries, by being debilitated, have become congested, which readily runs into the stage of actual inflammation. The already excited uterine nerves convey the morbid impression to their spinal origin, and there produce stimulation which is felt through the medium of the nerves throughout the entire system. And here, at the commencement of this disease, we have the opportunity of witnessing the very first febrile movement; I mean the excitement felt throughout the whole system from stimulation of the nervous centres. Who, that has

had experience in puerperal fever, has not marked that, just before the onset of the disease, the lady felt unusually *well?* She enjoyed with a zest the society of her friends; dallied with her infant; took nourishment with a relish; and was buoyant under the confident anticipation of a speedy recovery. But the philosophic and practiced eye of the physician could see through these deceptive manifestations of vital energy, and perceive that they all arose from undue excitement, sent to the nervous centres by a suffering organ; and the same eye will also be able to perceive other changes which are silently taking place: over-excitement in the nervous centres is producing debility, and the debility engorgement; the first manifestation of which, perhaps, is, that the lady complains of feeling tired, and falls asleep, from which she is aroused by regular rigors. The whole system now feels the shock; the head, back, and limbs ache; the heart beats laboriously; the lungs play with difficulty, and there is restlessness and sighing; but the blood soon accumulates in the great vessels, so as to excite the heart by the stimulus of distension to increased action, and the blood is sent with power to the periphera; but the weakened, torpid, congested capillaries do not transmit it freely, and hence soon follow heat, throbbing, pain, restlessness, and all the other concomitants of febrile excitement, complicated with inflammation of a most important and sensitive organ. All this is very unexpected to friends who were misled by

the deceptive excitement of the first stage, and all are ready to give a reason. She was too *smart*, they say; sat up too much; talked too much to the baby; ate more than was proper, etc., etc.; never imagining that all these were done under the excitement of the disease itself.

The common sense of the profession has led it to adopt a plan of treatment for this disease which corresponds very well with the views here expressed of its nature. The remedies mainly relied on now are blisters to the abdomen, and the internal use of opium and spirits of turpentine. Now opium, we know, is directly a powerful nervous stimulant, and indirectly an equally powerful *general* stimulant, and turpentine is directly a most potent stimulant, only a little less severe than fire itself, when applied to a sentient or raw surface, and, hence, is much used as an application to burns to "draw out the fire." It does act beneficially, and upon the same principle that heat does; that is, when a part is debilitated by the action of a powerful stimulus, one a little less powerful will keep up the action of the capillaries until they have time to regain their tone. The other leading agent referred to, viz., the blister, acts upon the very same principle, it stimulates the external capillaries immediately over the inflamed organ, which, by being intimately associated with those within, causes them to act also; just as we arrest an internal hemorrhage by dashing cold water upon the surface. There is, therefore, a fitness between these

remedies and the object to be attained; and the practice is, consequently, reasonably successful. But there is a better means for producing stimulation, which I will give in full in the proper place.

The next disease which I will refer to, for illustration, is inflammatory fever; and I should suppose if I can clearly show *this* to be a disease of debility, you will find little difficulty in following me in the application of this term to all the rest of the febrile family.

A young man, unused to active exercise, concludes to amuse himself with a hunt, and, shouldering his heavy double-barrelled fire-arm, sallies forth on a sultry afternoon: he sees game, but it eludes him; yet, stimulated by the hope of ultimate success, he pursues on, forgetful of the unusual tax which he is imposing upon the vital forces; but, finally, his tired muscles complain so urgently, that he is obliged to give heed; and now, yielding to the impulse, he throws himself upon the ground beneath the wide-spreading branches of a forest tree, and drops to sleep; on waking, he finds his limbs numb and stiff, and his muscles obey the will with difficulty; but by an effort he reaches home and retires to bed. In the night he awakes out of unquiet slumbers, and finds his tongue parched; his throat as dry as his powder-horn; his temples throbbing; back and limbs aching; general heat of the surface; restlessness, etc. Now let us analyze these symptoms, and refer each to its appropriate cause. The heat and unusual muscu-

lar exertion produced a real, general, temporary debility, which, if he had returned slowly home, and retired to his own comfortable couch, would have subsided of itself, without leaving any unpleasant consequences; but to the debility from exhaustion of vital energy, he superadded that arising from a protracted exposure to cold; for however powerfully a sudden application of cold may act as a stimulus, its protracted influence is, most certainly, a decided debilitant; and in this case, the whole sum of vital energy having been lessened by exhaustion, the capillaries were unable to retain their tonicity, and, giving way under the onward pressure of the blood, became engorged beyond their ability to make efficient contraction. Now, this engorgement is all that we need to enable us to understand the *why* of his soreness, stiffness, and listlessness on waking. The cause was a general one, acting upon the whole system; and the capillaries everywhere became debilitated and engorged, and everywhere produced an unnatural condition, which was manifested in each part according to its nature and office. The cerebrum is engorged, and, as a consequence, the intellect is dull and beclouded; the other nervous centres are engorged, and the supply of nervous influence is deficient and of a morbid quality, causing deficient or morbid action in every part; the capillaries of the nerve-matter, of its investitures, of the cellular tissue in which it is imbedded, the muscular fibres, are all engorged; and of course the nerve-matter

everywhere is compressed and stretched, so that every contraction of a muscular fibre, every change of position, causes suffering; and even when the body is kept motionless, this compression and tension of the neuramilla are sufficient to cause that indescribable sensation, known as "a sense of ill-being." But, say you, there is no controversy here: all agree that this stage of fever is one of debility, but we deny that the subsequent one, that of reäction, is.

Well, I will now proceed to examine it, and see if I cannot trace the marks of debility through it also.

I have already alluded several times to the manner in which the reäction—the hot stage of fever—is brought about; viz., that the blood, by meeting with difficulty in its passage through the capillaries, accumulates in the heart and arteries, and stimulates them to increased contraction: now if the capillaries did not *remain* debilitated and inactive, and thus continue to offer resistance to the momentum of the circulation, what prevents the heart from relieving itself from the stimulus of the accumulated blood? What prevents an immediate return of all the parts to a natural, quiet condition? In mild cases, this desirable result often does take place, in consequence of a reäction of the capillaries from their innate vigor, coinciding with the general reäction, or by the assistance of a timely exhibition of some of the domestic remedies, which the common sense of mankind, or

rather of womankind, has led them to adopt, such as a warm ginger-stew, aided by the stimulus of a hot foot-bath, etc.

But suppose these timely remedies be neglected or prove inadequate: the symptoms go on increasing in inveteracy; the throbbing carotids drive the blood into the vessels of the brain with tremendous force, but the capillaries are torpid, congested, and do not transmit it freely; every available space is therefore crowded to the full, and the medullary substance so compressed that it cannot perform its functions well; the numerous nerves in its investing membrane are compressed, until they give out the sensation of pain, more or less intense; association between the faculties of the mind is lost, and the patient has wild vagaries and talks incoherently: a little more pressure, and mental action ceases entirely, and the patient sinks into coma, and is dead to all thought or sensation.

Similar events have been taking place in other parts of the system; the capillaries of the stomach have been weakened and congested, and caused compression of the gastric nerves, which at first, perhaps, only gave the sensation of hunger, but which food would not satiate; an increased pressure will, however, cause them to complain in their usual way, viz.: by nausea, sickness, and perhaps vomiting. The same pressure upon the nerves of ordinary sensation, in the coats of the stomach, causes pain and soreness. The liver suffers in the same manner: at first, perhaps, the congestion only increases its na-

tural secretion; a higher degree depraves it; and a still greater suspends all secretion, and distends its substance so as to present an evident fulness externally, with more or less soreness, pain, and sense of weight. All the other organs suffer in like manner, and complain in a way suited to the office and sensibility of each. The skin being exposed to the immediate inspection of the eye, we are enabled to examine its condition more readily and more accurately than any other organ. Now what are the developments presented? In the hot stage of fever, we see its vessels are evidently distended with blood, but it does not present the ruddy glow which we see follow active exercise in health; but it is dusky, like that presented by passing a ligature around a limb, so as to partially obstruct the return of the blood in the minute vessels.

But it may be objected that this view of the nature of inflammatory fever is not consistent with the known character of the agencies by which it is successfully treated; that these are blood-letting, active purgation, nauseants, etc., all of which are debilitants.

But an analysis of the *modus operandi* of these remedies in this particular case will show that their success harmonizes well with this theory. We will take venesection as a type of the class: it will be conceded, I suppose, that there is no actual increase of the quantum of blood over what was in the system immediately preceding the attack, or a few hours ago, when our patient was laboring under the period

of depression; then there appeared to be an actual want of blood, judging from the pallid countenance and the shrunken surface; there cannot, therefore, be too much blood, but there is an obstruction to its circulation, and what is wanted is not a reduction of its quantity, but a removal of this obstruction, so that the heart may be able to relieve itself without exerting such unusual force. This can be effected in two ways. First, directly by stimulating the capillaries, and causing them to resume their usual action in passing the blood, which, finding a free passage, soon relieves the heart of the stimulus of distension, and it quiets down to its usual force of action; or, secondly, the same thing can be done *indirectly*, by the use of such means as will lessen the heart's action; for by lessening the force by which the blood is pressed into the capillaries, they are allowed time to regain their usual contractibility, and resume their function. This can be done by abstracting blood from a vein; the supply to the heart being reduced, it is enabled to relieve itself of the stimulus of distension, and of course acts with less power; or it may be done by acting upon the heart through the medium of the nerves: for example, nauseants and veratrum viride, by their depressing influence on the nervous centres, diminish nerve-power generally; and less nervous influence being sent to the heart, and it made less sensitive to the stimulus of the blood, it will act with less force under the same amount of stimulation.

But venesection, nauseants, veratrum viride,

drastic purgatives, and all that class of remedies, relieve the obstruction by producing positive general debility, and necessarily make convalescence protracted and uncertain; whereas, by the first mode, that is, by direct stimulation of the capillaries, relief is obtained without any loss of general strength, without any shock to the vital powers, and convalescence is consequently direct and complete. But I will dilate more upon this at the proper time.

I will now take a hasty view of miasmatic fever, and see how well its symptoms harmonize with the general theory I have advanced.

But it will amount to little more than a tiresome repetition of much that has been said, to go into a detail of its symptoms, and an explanation of the condition out of which they arise. We have here the same evidence of nervous and capillary debility in the forming stage which has already been described, with this difference: the cause having acted a longer time in bringing about exhaustion of vital energy, the manifestations of debility and morbid nervous influence are greater, and the functional derangement in the various organs is also usually more decided, so that we have, along with the listlessness, languor, muscular weakness, etc., more headache, pain in the back and limbs, greater disturbance of the stomach, bowels, and liver, and a more decided sense of chilliness. But still it must be acknowledged that the symptoms of the first stage of all fevers, whether sympathetic, malarial, or contagious, are so nearly similar that the most experienced practitioner,

when consulted at that period, is often wholly unable to determine what form it will assume when fully developed. And, fortunately, it is generally of the least possible consequence that he should determine, provided he have a clear comprehension of the condition of the system out of which the symptoms have grown: a hot mustard foot-bath, frictions along the spine, with some stimulating liniment, and a mild nervous stimulant, resorted to in this stage, will often so effectually arouse the capillaries, and quiet the morbid nervous excitement, that the physician is never able to learn what kind of a fever would have been developed, had the case been left unmolested.

But to return to the case: after this stage of depression has continued for some time, a reäction is brought about in the manner already described, and differs nothing from the reäction in inflammatory fever, except that it is usually not so perfect; for, owing to the very gradual way in which the stage of depression has been brought about, the debility is more profound, and the vital energies are, consequently, not so readily aroused. In some cases the exhaustion of the vital energies is so complete, that the patient dies in the stage of depression; in others, it takes place very imperfectly, not amounting to more than a mere throbbing of the heart, with but trifling increase of force, the extremities still remaining cold. But in whatever degree reaction does take place, it is always attended with the symptoms of capillary obstruction and morbid

enervation; showing that debility is a prominent feature of this stage also. Perhaps there is no one fact connected with this stage that points so conclusively to debility as constituting its most prominent character as this, that notwithstanding the *reäction*, there is no increase of *strength:* on the contrary, the more powerful the heart's action, the more utter is the feeling of prostration; unless, indeed, the brain becomes maddened by inflammatory action, causing frenzied demonstrations of power. After this state of excitement has continued for some hours, it begins to abate, and we have an intermission, or at least a remission, which appears to be brought about in the following manner: The heart, being nothing more than a great muscle, is subject to the laws governing muscles in general. One of these is, that when they are made to contract with more force than usual, they become tired, or have their vitality exhausted. Accordingly, the heart, after acting with unusual power for a few hours, becomes weary, and gradually assumes a more moderate action, under the same amount of stimulus; and besides, the capillaries reäct in some degree during the excitement, and, by passing the blood on, lessen the stimulation of the heart. But there is still another way in which a remission, or at least a subsidence of actual excitement, may be brought about. At the termination of each capillary, whether it communicates with a vein or an absorbent, or opens into an excretory duct, or performs a secretory office, or opens externally through

the skin or into the bowels, there resides a peculiar sensibility which acts as a kind of sentinel over the passage of the contents, and refuses to allow any constituent of the blood to pass, except that which nature designed to pass. But in cases of great debility this guard leaves its post; in other words, the sensibility becomes so deficient, that the mouths of the capillaries relax so as to suffer the more fluid parts of the blood to pass out unchanged and unchecked, and a wasting colliquative sweat or diarrhœa lessens the heart's action, and at the same time wastes the energies and resources of the system.

If the intermission be perfect, and the vital energies of the system not much exhausted, the system regains nearly its usual healthy condition, the sensations become more natural, and the secretions more or less restored, showing clearly that debility was the cause of both *inaction* and *reäction*. But the *cause* is still in operation, and the capillary inaction only partially removed; and, after a certain period, another paroxysm is set up in the same way as the first. Now, in case the debility in the capillaries is greater and more persistent, in place of a perfect *intermission* succeeding the reäction, there will only be a remission; and this will be more or less perfect, exactly in proportion to the degree in which the capillaries can be aroused by the stage of excitement.

Now, all will be ready to admit that the lowest cases, such as prove fatal in the first stage, and such

as are attended with a feeble reäction, are characterized by debility. Well, according to the fair analysis which has been given of the various stages of fever, it is plain that these lowest cases are founded in the very same morbid changes which gave rise to the slighter and most open forms; the only difference being the greater intensity of the debility occasioned by a more powerful action of the remote cause, or from its acting for a longer time, or on a system debilitated by some other cause.

I will now hastily sum up what I think have been proved to be the essential circumstances connected with, and constituting, a case of fever. 1st. Morbid impressions made upon the nervous centres. 2d. Weakened and vicious nervous influence sent out through the whole system. 3d. That this weakens the action of the capillaries, and causes them to dilate under the pressure of the blood, while, at the same time, the sensibility of their mouths is so altered by having been supplied by morbid nervous influence, that they refuse to let the blood pass; just as the neck of the bladder, when irritated, refuses to give passage to the urine, though of a natural quality, thereby retarding the blood in the capillaries, and obstructing the circulation. 4th. That the accumulation of blood in the large vessels, from this cause, occasions the heart and arteries to be unduly excited by the stimulus of distension, thus producing the reäction which constitutes the hot stage. 5th. The pressure and tension occa-

sioned by the engorgement of the capillaries, give rise to all the unnatural phenomena which attend a case of fever throughout its several stages, viz., the increased, depraved, or suspended secretions, painful and unnatural sensations, inflammatory complications, etc. 6th. It is only by a partial restoration of the natural action of the capillaries that an intermission, or remission, is obtained; and only by permanently restoring this action can the disease be broken up. And lastly, if capillary action is not restored, the patient dies.

CHAPTER IV.

CAUSES OF FEVER.

We now have but one more point to examine, and we will then have a plain, practical, common-sense view of the whole subject; that is, the original cause, the agency by which the deleterious impression is made upon the nervous centres. In symptomatic fever, we know that the remote cause is morbid impression sent by a suffering organ to the brain and its appendages. The remote cause of inflammatory fever is known to be the depressing influence of a protracted exposure to cold upon a system exhausted by fatigue; but the remote cause of other idiopathic fevers is not so well understood, but it is certain that it must be something that is capable of floating in the atmosphere, and that it is either organic or inorganic, and that it either acts upon the nerves of the sentient surfaces, as the skin and alimentary canal, or it enters the circulation and acts immediately on the nervous centres. Now, it does not appear to be reasonable to suppose that it acts primarily upon the surfaces;

for then those parts should *first* show signs of disease before they could send morbid impressions to the nervous centres, so as to bring about the debility which characterizes the first stage of all fevers. But we know that there is no previous manifestation of irritation in these surfaces; then the cause must enter the circulation, either through the lungs or along with the ingesta, and reach the nervous centres through the blood, and, when there, act either as a mechanical irritant or a narcotic stimulant. My views of the whole subject are briefly as follows: That the cause of all malarious epidemic and contagious fevers is organized existences—infinitesimal animalcules or sporules which float in the atmosphere, and are inhaled with it into the lungs, and, permeating the lining of the air-vesicles, enter the blood, and by it are carried to the nervous centres, where they act as narcotic poisons.

This view of the subject will enable us to explain many of the facts connected with the spread of febrile diseases, which no other theory can. It also enables me to give a rational explanation of the mode in which one of the remedies which enter into my plan of treatment operates in arresting fevers, which I otherwise cannot explain. But whether organic or inorganic, it most certainly acts as a narcotic stimulant, or nerve poison: the nature of the first symptoms indicates this most unmistakably.

Fordyce says: "One of the symptoms of fever is diminution of the sensibility of the skin; of this one great instance is its insensibility to heat, which

has been so great in some instances that no sensation has been impressed on the mind when hot bodies have been applied so as to coagulate the scarf skin. This insensibility to heat is not from the sensation of coldness, for it extends to the sensibility of the skin of every kind; it is a degree of what is called numbness or indistinctness of the ideas which are obtained by the feel of the figure of bodies, their smoothness or roughness, their hardness or softness, etc. It is extended to other means that give pain, as stimulating applications, the pricking of sharp instruments, etc. The degree of this numbness or want of sensibility of the skin takes place in various degrees, but exists in almost every case of fever." He also describes as common, "a sense of weight, fulness and uneasiness about the breast and pit of the stomach, similar to what is produced by fear, grief, and other depressing passions."

Persons sometimes die in the first stage of fevers, and the symptoms show that the cause of death was purely a powerful nervous impression. Fordyce says: "When the first attack is fatal, it sometimes kills in less than five minutes; sometimes it requires half an hour, seldom a longer time. While the patient is yet sensible, violent headache with great sense of chilliness takes place, the extremities become very cold and perfectly insensible; there is great prostration of strength, he becomes pale, his skin is of a dirty brown, and he is soon insensible to surrounding objects," etc. He adds, that "when in any paroxysm of fever rigor and horror take

place, the patient is never carried off by that paroxysm." The reason of this is obvious: these rigors are evidence that the nerves are not totally overwhelmed. When reäction takes place in the nervous system, there is great exaltation of sensibility; hence the patient complains greatly of heat, although the actual heat of the body may not be greater than in health, or even much less. Fordyce says upon this subject: "The heat over the whole body seems intense to the patient, yet, upon the application of the thermometer, it is found even less than it was when the patient felt himself cold." This exaltation of the sensibility also accounts for the extreme restlessness experienced by patients in this stage. The reäction of the heart sends the blood with force to the capillaries, but they, being debilitated, do not transmit it freely, and become distended so as to press upon the interstitial nerves, whose sensibility is now greatly augmented, giving that indescribable sense of suffering and restlessness all have experienced who have endured the hot stage of a paroxysm of fever, producing a sense of "prolongation of time," so that a minute will appear an hour, and an hour an age. Fordyce further says: "It happens often, at the beginning of an attack of fever, that the patient has a sensation as of some light body moving over the hairs that arise from the skin, as if, for instance, a number of little insects were walking the points of these small hairs." The above is exceedingly suggestive of the nature of the cause of fever, for what but a narcotic

could produce this peculiar sensation? All who have taken opium will recognize this description as answering admirably for the sensations occasioned by that drug, the impression of insects running over the surface being often very distinct and very annoying after being narcotized to a certain extent, or fully impressed by alcoholic potations.

That the cause of fever, be it what it may, acts primarily upon the brain and appendages, has been fully acknowledged by the best authorities. Dr. Smith says: "The immediate cause of fever is a poison which operates primarily and specifically upon the brain and the spinal chord. The diseased state into which these organs are brought by the operation of this poison, deprives them of the power of communicating to the system that supply of stimulus (nervous and sensorial influence) which is required to maintain the functions of the economy in the state of health." A superabundance of authority could be arrayed in support of the position, that the impression is first made upon the brain and nerves, but no one appears to have been led to perceive that this impression was that of narcotism; but still, without having this idea before the mind, authors, as above, in describing the manifestations of the operation of this poison, have unknowingly traced out a good description of the effects of narcotic stimulation. This point being settled, the field of investigation is at once open for means to neutralize the poison or counteract its effects.

CHAPTER V.

TREATMENT OF FEVER.

I HAVE now dwelt long enough upon the cause of fever, its nature and phenomena, and have doubtless convinced all whom I shall be able to convince, that the theory I have offered is the true theory. I will, therefore, leave this part of the subject, and proceed to give the plan of treatment which has grown out of these views, and which in my hands has proved successful beyond any parallel of which I am informed.

According to the foregoing views, there are four points to be met in the treatment of every case of fever.

1. To remove the cause.
2. To allay the nervous disturbance.
3. To restore the action of the capillaries.
4. To allay over-action and equalize the circulation.

And first: To remove or neutralize the remote cause. In symptomatic fever, it is obvious that our first efforts should be directed to the removal of the local irritation which has given rise to it. If

it be from a protruding tooth, we should remove the tension of the investing membrane by dividing it. If worms in the alimentary canal, expel them. If local inflammation, use such means as will assuage it, etc., etc. The same thing should be done in idiopathic fevers—that is, those which are produced by the direct action of the remote cause upon the nervous centres; for as long as the cause is still acting, it will still keep up the effect. But how is this to be done? It had never been attempted, so far as I am informed, until I attempted it.

Having come to the conclusion that the cause of all idiopathic fevers is a something possessing the property of a narcotic poison, I believed that the oil of sassafras would destroy or counteract, or neutralize it. I had tried it for these purposes a thousand times, successfully, in other analogous cases. For example: I had tried it repeatedly for destroying insect life, and found it immediately fatal to every kind; had also tested its powers upon the infusoria in impure water, and saw it destroy them instantly. I therefore felt assured that if the cause was really the presence of minute animalcules disturbing the nervous centres, this would be conveyed by the circulation in sufficient strength to destroy them at once. I also knew that it would as certainly destroy or neutralize any poison which they might have infused into the system, for I had tested its power fully in destroying the poison of insects and reptiles, such as mosquitoes, fleas, spiders, bees, wasps, etc.; and, on one occasion,

had an opportunity of testing its powers over the venom of the snake known as the copper-head, and found it to succeed promptly. I also knew, from repeated experiments, that it would destroy or prevent the effects of vegetable narcotics, such as tobacco, henbane, etc.; I therefore judged that, should I be mistaken in the cause of fevers being animalcules, it must of course, then, be a poisonous gas; and, believing, from observing its effects, that it must be of the narcotic kind, I had good reason to infer that the oil of sassafras would succeed, should this be the case. I therefore expected it would meet the first indication, and was not disappointed. I have seen its exhibition followed, again and again, with as sudden a disappearance of the febrile movement as I ever saw follow the opening of an abscess, when the fever originated from that cause.

The second indication, to allay the disturbance in the nervous system, I believed could be best met by the use of valerian, it having the property of quieting the nerves, easing pain, and procuring rest, without any of the unpleasant effects of opium, and most other narcotic stimulants upon the brain, having, too, no tendency to suspend the secretions, as they have, but, on the contrary, being a very good diuretic, a valuable consideration in selecting remedies for fever, the urine always being scanty and depraved.

The third indication, that of restoring the action of the capillaries, I believed could be met more

effectually and pleasantly by the use of piperin than by any other known agency. I had often used it as an adjunct to quinine, in intermittents, and discovered that it not only produced a fine capillary action in the surface, but it evidently aroused the capillaries everywhere, as all the suspended functions were usually restored without the aid of any other means. I liked it because, although it produced a fine action on the surface, it rarely excited perspiration; on the contrary, checked it when present, if of the passive kind. It is also recommended by the fact that it is borne well by the stomach, and has a good effect in quieting the nausea, so often distressing to the patient in fever. Its cousin, cayenne pepper, is of too fiery a nature to be borne well by the stomach, or readily imbibed by the absorbents; the piperin has, in my hands, answered the purpose so finely, that I have had no reason to desire a better remedy.

The fourth indication, that of quieting the heart's action, and equalizing the circulation, is in a great measure met by the means already enumerated; for by removing or neutralizing the cause, quieting the nerves, and exciting the capillaries, the condition of things is removed which gave rise to the disturbance, and it, as an effect, consequently subsides. Still there are many other minor means which may be appropriately called into requisition to aid the former and facilitate their action, such as sponging the surface with water of a temperature suited to the degree of general or local heat. For

example, when the head and trunk are hot, and the extremities cold, apply cold water to the first, and hot water to the latter; and when there is restlessness rather from nervous excitement than over-action, tepid water will answer the best. The bowels should also be kept moderately loose, so as to remove the debris, which is constantly accumulating in every case of fever. For this purpose I choose rhubarb, on account of its certainty of action, while it rarely acts too much, and, also, because it operates without producing debility, but, on the contrary, is tonic and bracing in its effects. A considerable source of irritation in fevers is an accumulation of sour phlegm in the stomach and bowels, to remove which I choose sup. carb. of soda. I also found much advantage from friction along the spine with some kind of liniment containing both stimulant and anodyne properties; of course, many adjuvants have been resorted to, from time to time, to meet particular symptoms; but the above forms a general catalogue of the means by which I attempted to subdue the most potent enemy that has ever assailed our race, and with them I have succeeded, not occasionally, not generally, but uniformly and promptly. These means, though simple, as regards any unpleasant effects they can have upon the system, have proved as powerful in the contest with disease, as did the pebble which David used in his hostile meeting with the uncircumcised defier of Israel.

For the sake of convenience, and to obtain an

eligible form in which to administer the remedies, I formed them into a syrup after the following formula: Take half a pound of common rhubarb-root, and after breaking it coarsely, add half a gallon of water; place it over a slow fire, and keep it near the boiling-point for three hours, then strain: there will now be about a quart; to this add five pounds of loaf-sugar, and set it on the fire until it simmers; to this, after it has cooled a little, add half an ounce of oil of sassafras, one and a half drachms of piperin, and two ounces of sup. carb. soda, first having rubbed them well together in a mortar, with a few ounces of water; stir the whole together well, and then add one pint of saturated tincture of valerian.

There are two things to be particularly remembered and observed in making this compound: the sugar must be the crystallized white sugar; if the common brown sugar be substituted, and the proportions used that I have directed, it will form a candy rather than a syrup. I have heard of some having made this exchange, and of charging me with giving a formula that was unpharmaceutical, they not knowing, I suppose, that crystallized white sugar, containing so much more water of crystallization than the brown, requires a smaller amount of water to give it fluidity.

When I make a prescription to be filled at a drug store, I make it as follows:

Syrup Rhubarb, ℥ iv.; Tinct. Valerian, ℥ ij.; Oil Sassafras, drops, xx.; Piperin, gr. x.; Sup. Carb. Soda, gr. xx. Mix.

The other point to be observed is, that the piperin

and oil of sassafras must be intimately mixed with the syrup before the tincture of valerian is added; otherwise the compound will be so pungent that it cannot be swallowed; after having been well combined with the syrup, the alcohol in the tincture does not unite with them so as to produce this pungency. I formerly used the infusion of valerian in order to avoid the pungency, but water does not extract the full strength of that article, and having fallen upon the above method of avoiding the difficulty, I now use the tincture in preference. Those who have seen recipes for making this compound which differ from this, will recollect this change, and also, that in this formula there is less piperin and oil of sassafras, it having been ascertained that less of these articles would produce the effect desired.

In the course of the foregoing observations, I referred to the advantage of rubbing the spine with stimulating liniments for the purpose of relieving irritation of the spinal marrow. Much of the suffering experienced in fever arises from this cause; the back itself not only complaining, but pains and distress of various kinds are felt in different parts, which have their origin in spinal irritation, and can be relieved in a few minutes by the right kind of local application to the spinal column. I have used various compounds for this purpose, such as camphor and turpentine, volatile liniment, etc., but by far the best that I have ever tried, not only for this purpose, but also for pain or aching of the head, dis-

tress of the stomach, or in fact any other distress of whatever kind or wherever seated, so it be of nervous origin, is the following.:

Sp. Nit. Dulc., ℥ iv.; Aqua Ammonia, ℥ i.; Oil Juniper, ℥ ½; Oil Sassafras, ℥ ½; Gum Camphor, ℥ ½; Chloroform, ℥ i.: when the Chloroform is not at hand, Sul. Ether answers pretty well.

But although I give the above syrup in every case of fever I am called upon to treat, and in many cases give nothing else and do nothing else, yet, as I have before intimated, I do not always rely on it exclusively, but, in bad or obstinate cases, call into requisition every other means which I think will aid it in meeting the indications I have pointed out. For example: In intermittents I often give a dose of some strong nervous stimulant to aid the syrup to break up the chill; quinine or some preparation of opium is preferred. After the chill is prevented, and the periodicity of the disease by this means destroyed, I depend upon the syrup alone to restore the system, and prevent a return. If there is so much torpor of the liver that the syrup, aided by a mustard-plaster to the right side, does not arouse it, I give a blue mass pill every three or four hours, until I obtain a bilious discharge, still continuing the syrup to aid it, so that the effect may be obtained without mercurializing the system; and if there should be deep congestion in any part, or actual inflammation, I apply a fly-blister, and let it stay until its full effect is obtained; then have it dressed with an emollient poultice, without breaking the cuticle more than is necessary to afford an

escape to the serum, the object of drawing the blister being to obtain a powerful stimulation of the capillaries during the process of drawing, and not to set up a drain. I therefore endeavor to get rid of it as soon as possible, and for this purpose apply soothing applications, in order to hasten the formation of a new cuticle, which, under proper management, is generally obtained in forty-eight hours. An irritable blister I dread as I do a salivation: both add to the debility by increasing the irritability of the system, and thereby retard the recovery. But it is not only a dread of salivation that causes me to be chary of the use of mercurials in fever; I am equally fearful of their irritating effect upon the stomach and bowels, when there is any tendency to inflammation in these organs, or even considerable irritation.

Consequently, I resort to mercurials only when the case is urgent; and it would be surprising to those who have been in the habit of looking upon them as the only means of stimulating the liver, so as to incite the secretion of bile, to find how much they have been mistaken. It is very seldom indeed that the simple means which I have laid down for stimulating the capillaries in general, fail to also excite those of the liver, sufficiently to cause it to secrete bile of a good quality, and in sufficient quantity. But I do not, as I once did, expect to see my fever patients improve, as a matter of course, because I have obtained consistent bilious discharges, or that they must of necessity die, if such

are not obtained. On the contrary, I merely look upon the liver as *one* of the many organs whose office is essential to health. Persons may *live* many months without any secretion of bile. In fever, all the secretions are deranged or deficient, and health cannot be regained until all are restored to a natural condition; but I do not look upon the restoration of the biliary secretion as of such paramount importance as to warrant me in being in a feverish hurry to accomplish it, for, as I have said, by exercising a little patience, I usually find the liver and all the other secretory organs return to the performance of their appropriate functions under the influence of the prescribed general treatment for restoring the action of the capillaries in general.

I have yet said nothing about bleeding as a collateral aid; I seldom bleed, yet there do cases occasionally arise which imperatively demand it. In cases complicated with active local inflammation, in which general inflammatory action is considerable, there is often so much contraction of the mouths of the capillaries, that it becomes absolutely necessary to relax them by bleeding or nauseants, or both, before a proper capillary circulation can be set up. Pleuritic fever and pleuro-pneumonia present good examples of such cases; here the heart's action must be first partly controlled, and the spasm of the capillaries relaxed, before the syrup can act efficiently in bringing about a healthy action of these vessels. It is true that in such cases the convalescence cannot be so speedy as though no debilitants had to be

used, but if they are pushed no farther than is actually necessary to relieve the spasm, and the means then used to act directly on the capillaries, there will only be a few days' difference in the time of recovery.

The remarks which have been made apply to fever as a class, and are consequently applicable to all its forms; it might be expected, therefore, that according to custom I should now proceed to treat of each form particularly; but I think the view I have given of the unity of the febrile movements renders this almost unnecessary; my theory embracing the conclusion that all the forms of fever consist in similar derangements of the same parts and organs, differing only in the extent or intensity of that derangement, and a tendency to affect more injuriously particular organs, occasioned, I suppose, by the remote cause acting with the greatest force upon the particular part of the brain, or other nervous centres, from which the nerves supplying that part originate, or from the accidental coöperation of some other remote cause, acting at the same time upon a particular organ, as cold upon the surface, worms in the bowels, a determination of blood to the head, from a debauch, disturbance of the alimentary canal, from improper ingesta, etc.; which, by weakening the vital energies of a particular part, will cause the debility occasioned by the remote cause of the fever to be more profound in that organ than in other parts, and disqualify its capillaries from reäcting with the same facility; and hence,

when a remission occurs, this organ does not recover its functional movements as perfectly as the rest, which circumstance has often led the observer to look upon the fever as originating there, and as being a disease essentially of that particular organ. But notwithstanding these differences and complications, all fevers consist essentially of the same derangements in the same parts; and it follows, as a consequence, that the same mode of treatment is equally applicable to all, only requiring such other means to be occasionally called into use as may become necessary to meet these accidental differences. This view of the subject, you will perceive, cuts off the necessity of going into a detail of the treatment of particular fevers. It also saves the practitioner the necessity of making nice discriminations at the bedside as to the particular *name* of the fever he may have to contend with—a discrimination which the most astute and experienced are often unable to make. The only call for the exercise of judgment upon my plan, is to decide upon the amount of stimulation that may be required in any given case, in order to arouse the dormant capillaries, and to call into use such means as may be necessary to meet accidental complications. And if there should be any doubt as to the amount of stimulation required, we have only to watch the effect of the medicine, and increase or diminish the dose, according to the effect produced.

But, says one, I thought you would at least go into a detail of your method of managing typhoid

fever, as that is the disease which I heard your plan was particularly successful in breaking up. This is a mistake: it arrests and breaks up all the other forms of fever common in this country, with the same promptness that it does the typhoid; and the only reason why particular attention has been drawn to this more than others, is that they are usually successfully managed upon other plans, and this is not—at least, not aborted, broken up, and cut short in its progress. So firmly has the idea taken hold of the medical mind that typhoid fever is a *self-limited disease*, having a determinate course to run, which it will run, in spite of all efforts to the contrary, if the patient should live so long, that a contrary idea is looked upon as heterodox, and the expression of an opinion that it can be arrested, is taken as evidence of ignorance, or of a wild imagination. All of this I knew, and had counted the cost of, before making the announcement that I claim to be able to do this thing; which odium I have realized in some measure, and may yet have to endure still more; but if God lets me live a while longer, and the laws of matter and of life continue permanent, I will yet be able to establish that in this instance, as in many others, universal experience does not necessarily prove any opinion to be absolutely true. If it did, then small-pox cannot be prevented; then the arteries do not contain blood, but air, as their name imports: negative experience cannot weigh against positive. If all the world had, from the beginning, closed their eyes upon the approach of twilight, and only opened

them again at broad day, the world would still be in ignorance of the existence of moon and stars; and should accident cause me to open my eyes, and see the moon in its beauty, and the stars in their brilliancy, would I not be as certain of their existence as though all the rest of mankind had seen them too? When Hervey experimented upon the *living* subject, and saw the blood coursing through the arteries, did not he know at once that the idea that they contained only wind was false, although the *world* believed it? Did not Jenner know that the small-pox could be prevented, after he had seen those who had received the vaccine virus exposed to its contagion, and remain unscathed? And have not I a right to know that typhoid fever can be aborted, when I have seen that result take place scores of times, under the use of the same means, and without ever failing? Why, the time has been, in the history of medicine, when the universal opinion of the profession was, that all the grades of even miasmatic fevers were self-limited, had a determinate course to run, and could not be aborted, or broken up. Even the great Sydenham acknowledged, in his early writings, that he knew of no means that would break up even the simplest form of intermittent fever; yet he did not think it impossible that means *might* be discovered, and says, that should such discovery be made, the discoverer could neither be a wise nor a good man if he did not make the means known for the public good. At a later day, Sydenham became acquainted with the powers of the cinchona, and now everybody and

their wives know how to cure *fever and ague*. And typhoid fever is more easily cured than even that; the *syrup* I have described will do it alone, promptly and permanently, though it often fails to arrest intermittents immediately without the aid of a little quinine or opium.

Well, says one, I am willing to admit that you are sincere in what you say upon this subject, and that you have arrested the progress of all the grades of fever you have happened to meet; but it has so fallen out that you have never been called upon to treat a true case of typhoid fever. Then, I say, there is no truth in description, for I have contended with many cases bearing all the marks by which that disease is diagnosed: there is no certainty in memory, for I had seen, in other years, many cases which went on through the regular and tedious course which characterizes this disease, some continuing unto death: there is no uniformity in nature, or I could not have had a practice for several consecutive years, in the midst of a population in which typhoid fever was prevailing, and yet all the cases to which I was called happen to be of another kind. A part of these years I had almost the entire management of all the sickness within a certain bound; other physicians, on every side of me, had cases which they reported to be typhoid fever; and yet, according to this supposition, my bounds were strangely exempt, except, I suppose, a few cases which other physicians were called upon to treat within my neighborhood, and a few others to which I was called in an advanced

stage of the disease, after the rose-colored eruptions and the sudamina had made their appearance; all the other cases, which I saw early and aborted, I suppose must be considered mistakes. The last year and a half, (now near six years,) I have spent in this city; and although there has not been an abundant supply of fevers of any kind within its limits in that time, yet I have heard of cases of typhoid fever, and some which proved fatal. Now I admit that I have not visited as many patients as some other practitioners in the city, yet I have treated a goodly number, many of whom were of that class which is most exposed to the causes of fever, and would be the most likely to contract the worst forms: persons destitute of all the luxuries and most of the comforts of life; and yet it must be assumed that none of them had contracted typhoid fever, for, if they had, I aborted it: no fever case to which I have been called in the city has lasted over five days, except the case of a very feeble, ancient lady, who relapsed and remained sick ten or twelve days. But perhaps the inquiry may arise, Why have not other physicians obtained the same results from the use of the same means? that, as I published a compend of my plan of treatment many months ago in the State Sentinel, some other practitioners have had the curiosity to try it, but have not been so successful as I profess to have been. But I deny that any one has tried my plan; at least, not in a way to insure success; many may have *half-way* tried it, but half-measures always fail; better by far do like the

homœopathists—give nothing, or the representative of nothing, and depend upon the inherent powers of nature, aided by diet, rest, and appropriate external means. I am now fully satisfied that this do-nothing system will succeed better than any plan that reduces the system by *improper* depletion. It is not at all surprising, then, that a combination of my plan with depletory remedies should fail; if my theory is true, it must fail. But if others would rely upon my plan as I do, and not frustrate it by counteracting means, they would obtain the same results. I am no seventh son; I use no hocus-pocus; the cure is not performed by *me*, but by the *means* I administer, and these are at the command of every one. (The experience of others, received since this was written, will be found at another place.)

I do not claim for this theory of fever *much* originality; most of the truths on which it is predicated had been perceived by great minds long ago, but not with the distinctness necessary to enable them to deduce therefrom a correct plan of treatment.

CHAPTER VI.

THEORIES OF FEVER.

HUMORAL THEORY.

THE symptoms of nervous disturbance, and of capillary debility and congestion, attracted the attention of the very earliest cultivators of medical knowledge; but at that time, very little was known of the office of the nerves—most of the phenomena connected with innervation, whether natural or morbid, were then referred to the workings of an imaginary intelligent presiding genius, called vital principle, or *vis medicatrix natura*, which, they supposed, watched over and regulated the operations of the system in health, and made efforts to restore order again when any thing went wrong. Of the circulation of the blood they were wholly ignorant, for, having confined their observations to the dead subject, and finding the arteries empty, and the veins full of blood, they supposed the latter to be the only vessels concerned with the blood, and,

consequently, there could be no real circulation; but they fancied that the blood flowed out towards the surface in the day, and back again in the night, similar to the ebbing and flowing of the tide. As the capillaries always continue their action for some time after the heart and arteries cease to beat, they of course pass all the blood from the arteries to the veins, leaving the former entirely empty. The ancients, therefore, not perceiving any other use that they could assign to these hollow tubes, supposed that their office was to give passage to the *animal spirits*. And believing that the blood was pretty much stationary, and supposing that all diseases were occasioned by impurities in the blood, they imagined that the torpor of the forming stage of fevers was owing to the blood being clogged in the vessels by its impurities, and that the subsequent reäction was gotten up by the *vis medicatrix*, in order to throw it off, which they imagined was done by getting up a commotion in the blood, similar to an ebullition or fermentation, by which the offending matter would be separated and cast to the surface, as syrup cleanses itself by boiling, or wine and cider by fermentation. The congested state of the capillaries they supposed was occasioned by the viscid matter which was being thrown to the surface, and contented themselves to wait until the fermentation would thin it, and prepare it for expulsion; consequently, all that the medical attendant then thought could be done, was to use such means as he supposed would facilitate the fermentation, or guard

against excess, lest there should be too much stress upon the vessels, and occasion a rupture.

We may now smile at their fancies, but some of the greatest minds that ever illuminated a human body believed them, and their successors for many centuries were contented to take them for granted. Even the great Sydenham could see no farther, but was evidently much perplexed to account for some of the facts which presented themselves to his observing eye upon this then established theory. Among other difficulties, he acknowledges himself unable to explain how the blood of a healthy person could be rendered so impure in a few days after going into a sickly atmosphere, as to cause such grave results as he often witnessed.

SPASMODIC THEORY.

After the discovery of the circulation of the blood, and after physicians had acquired a better knowledge of the nervous system, their views as to the cause of the phenomena of fever became somewhat changed. Seeing that the idea of an ebullition or fermentation of the blood, as understood by their fathers, was unreasonable, but still holding on to the principal idea, that the cause was some offending matter in the blood, and still, too, retaining the notion of a *vis medicatrix*, they imagined that the excitement was brought about in this way: that the morbid matter, coming into the extreme arteries, was then detected by a kind of sentinel placed at their mouths, which caused

them to contract and prevent its passage, and, consequently, refusing passage to the blood also; and that the *vis medicatrix*, perceiving this difficulty, raised an excitement in the heart and arteries, in order to overcome the contraction, and force the viscid humors on, *nolens volens*, to the secretory organs, where they could be eliminated. It is therefore evident that those embracing this theory perceived the nervous disturbance and the congestion of the capillaries; but, misunderstanding the cause of the congestion, were led to adopt a mode of treatment that was not the best. For, seeing that the commotion raised by their blind presiding genius was only making matters worse, by causing a still more profound engorgement in the capillaries, by forcibly pressing the blood into them, which they did not transmit, and thus endangering the integrity of the system, they resorted to vigorous bloodletting for overcoming the excitement, which, though justifiable when nothing better can be done, yet is always an evil, because of the subsequent debility which must necessarily ensue; and still having the fear of lentor or peccant matter before their eyes, and having discarded the gross idea of a fermentation, they could think of no plausible way of getting clear of it, but by giving powerful secretory remedies to excite the organs to throw it off: hence arose the famous system of active purgation. Now, to excite the secretory organs to resume their natural action is proper enough, but this over-action was as blind an effort as what they attributed

to the *vis medicatrix*—it prostrated the patient by a foolish onslaught on an imaginary enemy. That this system was *generally* successful, does not prove that it was beneficial; for, after the doctor had bled and purged his patients, until he saw that he dare bleed and purge no more, he usually referred the case to the efforts of nature; which, being let alone, often gradually brought about a restoration; and though the patient did not die, yet the system was so debilitated that his health was usually ever afterwards precarious.

SYMPATHETIC THEORY.

In the process of time, as the nervous system was still better understood, and its influence in the economy more clearly perceived, the attention of theorists was directed more particularly to it, and still another theory of fever was gradually concocted. It was supposed that a strong impression being made upon the sentient extremities of the nerves, would be transmitted throughout the whole system by *sympathy*—not exactly through the medium of the nerves, but in some other way, never defined, and by some other power, never understood; but either the same or something else, in the place of the ancient *vis medicatrix natura*. But however it was defined, or however it was undefinable, this class of medical theorists supposed that an impression, once made, would be continued for a definite or indefinite period: as the vibrations of a musical cord continue after it

has been struck; or like waves which widen and follow each other after having been set in motion by the contact of a pebble with the surface of a placid lake; or as vital action, once excited in an *ovum*, will go on until it converts the whole mass into an organized body, bearing the impress of the first cause that sprung it into motion, though that cause was applied but once. In some such way, they supposed the impression which was made by the remote cause upon a sentient surface, was continued by independent action until it spent its force, like the vibrations of a wire, or was stopped by the power of some other greater or contrary impression, which would set up a new form of action. Upon this theory was founded a new plan of treatment: discarding entirely the notion of peccant matter in the blood, the leading idea was, to give some medicine that would set up a different action; which would swallow up or counteract that produced by the remote cause, and one which they fancied would be less injurious to the system, and would subside of itself. This was an ingenious and beautiful theory, but, like many other beautiful things, it led its votaries astray. It was this theory which introduced those potent remedies into general use, arsenic, mercury, and some others; and whether the morbid action by them set up has sent more persons prematurely into the other world than the diseases they were intended to counteract would have done, is a problem which eternity alone can solve.

So completely did this theory take possession of the medical mind, that, within my own recollection, no physician thought himself at liberty to neglect to mercurialize his patient, in almost every form of fever of the continued and remittent types; while arsenic was the remedy chiefly relied on for intermittents; since then, quinine has taken its place, which, though not much more certain in its operation in arresting that disease, is greatly less objectionable in its effects. But though both are successful, yet the success of neither is owing to the truth of the above theory.

PHLEGMASIAN THEORY.

The next theory which obtained any general notoriety, was what might be properly designated the PHLEGMASIAN THEORY, originated by the celebrated Broussais. He believed that all fevers originate from, or rather are identical with, inflammation of the stomach and bowels, and hence ignored the term *fever*, and substituted that of *gastro-enteritis*.

He argued that the condition of the system known as fever, whether the cause be a local injury, or miasm, or contagion, or any thing else making morbid impression, can never become developed till inflammatory action is set up in the stomach and bowels.

Broussais, being learned and talented, presented his doctrines in such plausible form, and defended them with so much ingenuity, and enforced them with such self-reliant confidence, that, supported by his great popularity, they swept over the civil-

ized world with the force of an epidemic, so that, with few exceptions, all who did not fully embrace them were more or less impressed. What aided powerfully in giving currency to this theory was, that it presented something as a foundation which was readily recognized by the senses—something that could be understood; and, as the medical mind had become weary of abstractions, it embraced this with a zest.

Now I think I have shown that fever consists in nervous disturbance and capillary inaction; and, as the capillaries of the chylopoetic viscera are equally involved with those of the surface and other parts of the system, and as they are all dependent upon the ganglionic system, or nerves of organic life, for nervous influence, and as these have their great centres in close proximity with the digestive organs, from which they receive the nervous power by which their functions are performed, it is not at all surprising that a lesion in their centres, sufficiently grave to cause general capillary inaction, should also develop marked evidence of chylopoetic derangement; and, as capillary debility and consequent engorgement constitute the first stage of inflammation, it is but a natural consequence that the symptoms characteristic of incipient inflammation in the stomach and intestinal canal should be evident in the early stage of most fevers, or that actual inflammation should afterward be set up by the disturbing forces. But a belief that there is *gastro-enteritis* in

all cases of fever, depends upon evidence so far-fetched, and so little supported by fact and observation, that it seems now passing strange that as good a thinker as Broussais evidently was, could have been led to adopt it as a foundation for a hazardous plan of treatment; and still stranger that others, not biased by the pride of opinion or the ambition of originality, should have been also misled. But argument is unnecessary, as at present but a few fossil examples of the genus *medicus* retain the impress of this theory. But, though founded in error, this theory also sprang from the observance of real phenomena connected with fever, and its attempted defence was the occasion of throwing much light upon the nature of the lesions which take place in the digestive viscera during an attack of fever; as, also, to show up the sympathies which obtain between these organs and other parts of the economy. It did more; for though in practice it caused many a poor fellow to succumb under the use of the lancet, yet it effectually checked the more murderous practice of hypercatharsis.

COOKE'S THEORY.

Some twenty-five years ago, a great man, then a resident of Virginia, had his attention particularly drawn to the nervous disturbance and subsequent congestion which are such prominent characteristics of every form of fever, and, not being able to account for them upon the sympathetic theory, directed his strong investigating powers to the

subject, and framed a very plausible theory, and instituted a mode of practice in accordance with it, which had a wide popularity for a number of years, especially in the Mississippi Valley, and is still adhered to by many. I refer to John Easton Cooke, of Transylvania. His theory was substantially this: First, disturbance of the nervous system. Second, weakened action of the heart. Third, congestion of the veins, and especially of the *vena cava ascendens*. Fourth, reäction of the heart, to relieve itself of the pressure of the blood. Now this theory came very near being true the only departure was this, that he made the debility and congestion of the capillaries secondary to engorgement of the great veins; which one departure misled him, and caused him to adopt a mode of practice which was not the most appropriate. For, looking as he did upon congestion of the vena cava as the cause of all the other symptoms, he only thought of the shortest method of unloading it; and knowing that the liver is the only organ which forms a secretion from venous blood, and therefore draws its resources directly from the stagnant fountain of all his fears, he, of course, chose that as the outlet through which the *black blood*, by being converted into *black bile*, could be discharged from the system.

Cooke's theory led him to denounce the then fashionable practice of mercurializing fever patients, as useless; but as he knew of no means which would excite the liver to throw off black bile equal

to calomel, he chose that as his chief reliance; not giving it, however, in small, salivating doses, but in twenty, forty, and one hundred grain potions; just as much, in fact, as might be found necessary to bring the black bile, submitting to an occasional bad salivation as a necessary evil.

To calomel he added rhubarb and aloes, which formed his famous cava pills, and with these, and occasionally some other more active purgatives to make them operate, he proposed to do all that could be done in the management of all the forms of fever. This theory was plausible and easily comprehended, and the practice based on it easily followed: the black blood caused the disease, and this must be converted into black bile, and calomel must be given in just such quantities as would bring the black bile; and if the patient sunk under the ordeal, or died of a gangrenous mouth, why, it could not be helped.

Now if this great man's mind had laid hold of the fact, that torpor of the capillaries causes the congestion of the large vessels, he would have directed his efforts rather to opening these sluices, so that the obstructed blood might flow on naturally again, rather than have attempted to empty the congested veins by the indirect process of pumping at the liver.

THOMPSONIAN THEORY.

About the same time as the above, another theory was gotten up, and a practice founded thereon,

which had a wide-spread popularity for a time; and though the author was not of the profession, and very few within its pale embraced it, yet as it made a great sensation, and especially as there were some things embraced in it which pointed to the truth, I shall here briefly notice it. I mean the theory and practice advocated by Samuel Thompson, of "number six" and steam notoriety; who, though not a really great man, had some of the elements of greatness about him, viz.: inquisitiveness, some originality, great boldness, and indomitable perseverance. He, seeing the evidence of capillary debility and consequent want of vital action, and perceiving that in all fevers there are alternate paroxysms of heat and cold, and sometimes both, in excess at the same time, in different parts of the body, conceived the idea that fever consisted, essentially, in this contest between heat and cold; and thus, by mistaking an effect for a cause, and considering heat and cold both positive entities, and investing them with a kind of fanciful personation, he imagined that a strife for mastery was always going on in the system between them; and that when heat preponderated, the man was well; when cold obtained the ascendency, the man was sick; and if cold triumphed, then the patient died.

Now all that the physician had to do, according to this theory, was to assist the friendly power to overcome the unfriendly—to generate heat and subdue cold. For the purpose of generating heat and throwing it out, he gave many powerful stimu-

lants, the best among which, and the one most relied on, was cayenne pepper, variously prepared.

If he had stopped here, he would have done less harm; but, filled with the idea that internal cold was the enemy he had to contend with, he went to work with heated rocks, and the vapor of vinegar and water, to drive it from its hiding-place. And here, for the want of a philosophic knowledge of the operation of the hot vapor bath upon the system, he worked contrary to the object intended; for hot vapor, though highly stimulating to the surface, when first applied, soon becomes one of the most debilitating measures, in consequence of the exhaustion it occasions of the vital energies of the surface, and the profuse exhalation of the serum and salts of the blood. But Thompson, from what motive is not apparent, adopted a means which repaired, in a measure, the mischief he had occasioned by excessive steaming: at the conclusion of the operation, he threw upon the patient a pailful of cold water, which, being a powerful stimulant and generator of vital energy, tonicity, and irritability, prevented the previous exhausting process from having as disastrous an effect as it otherwise would have had. But many a poor fellow had his vital powers so completely exhausted by, first, the lobelia, and then the protracted application of steam, that even the cold *douche* could not arouse him, and he died very quickly under the eye of his steamer.

The most intelligent of the Thompsonians soon discovered that the steaming would not do, and

abandoned it. My chief object for noticing this subject, is to draw attention to the fact, that notwithstanding the steamers were not directed by the least glimmering of sound philosophy, yet the powerful *stimulants* they used, often proved so beneficial, in spite of the injudicious steaming, that their success at times was most striking—sometimes breaking up a disease in a single day, which, under the regular depleting regimen, would have required weeks. Such instances could not fail to draw attention; and had it not been for an occasional case which suddenly went off under the operation of lobelia and steam, which affrighted the common observer, and the ridiculous absurdities which abounded in Thompson's book, which disgusted the intelligent, this system would have spread wider and lasted longer. It had its day and is gone, but it may be said of it, which cannot be said of some other theories, that the world is the better for its having existed. It did this, at least: it raised in the public mind such a horror of the abuse of mercury, that the faculty was obliged to curtail the use of it, in deference to public sentiment. It did more than this: it satisfied many observing physicians that fevers could be carried to a successful termination by other means beside the everlasting purging of the liver, and that, because a man had a hot skin, he was not obliged to exchange it for an artificial cholera morbus. They saw, too, that, contrary to preconceived opinion, cayenne pepper did not always increase a fever, but that, under its influ-

ence, the heat often subsided and left the patient relieved without any evacuation whatever; and little as the faculty may be disposed to acknowledge it, and, in fact, little as they may be generally conscious of the fact, yet, to those who have been observers for the last twenty years, it is evident that the Thompsonian theory impressed some of its features upon the regular practice, which was thereby improved. It cannot have escaped the observation of every physician who has been in the practice during the above period, that the method of managing fevers has, in that time, changed very materially. I speak generally, for there are some *set-fasts* in the profession, who, as a worthy contemporary has said, "serve to designate the exact state of theory and practice of the period of time when they studied their profession, or of the particular school of which they took lessons." Such practitioners still salivate, give the cava pills, or bleed and nauseate, just as their preceptors did when they studied. But to the honor of our profession it may be truly said, that physicians are generally men of intelligence and discernment, and are ever ready to embrace truth in science, and avail themselves of every better way of combating disease.

PRESENT THEORY.

The present seems to be a transition epoch, with regard to both theory and practice; the revelations of science have exploded all the old theories, and no well-digested new ones have yet been estab-

lished, so as to gain any thing like the general consent of the profession. Old modes of practice have also given place to means which the enlightened observation of practitioners has discovered to be better adapted to subdue or counteract diseased action. We accordingly find bleeding only resorted to in urgent cases; salivation is almost wholly discarded, as a means of curing fevers; calomel is given less often, and more sparingly; drastic purgatives mostly abandoned, and nauseants limited to particular cases. In the meantime, other means have become fashionable—almost all of which, I wish it to be noted, are stimulants—such as opium, quinine, spirits of turpentine, etc. This change has been brought about so gradually and so silently, that one can hardly tell when or how it was done, or point to any person in particular who has been prominently instrumental in bringing it about, and many successful practitioners perhaps would be puzzled to give a sensible reason for the change, unless it should be the ever ready one, that diseases have changed, and the mode of treatment had to be varied accordingly. But this reason is more specious than sound; for although it must be acknowledged that fevers are of a lower grade of action in this country now than they were twenty years ago, still the difference is not so great as to sanction a *contrary* mode of treatment. The description of fevers by our oldest authors will do very well by which to identify them yet. Even the modern disease, as some suppose it, typhoid fever,

was very accurately described by Sydenham in 1685, under the name of *stationary fever*. The fact is, man has remained, ever since we have any history of his characteristics, very much the same kind of animal: he eats and sleeps, loves women and wine, and other excitements, just as he did in the days of father Noah. The laws of nature, I suppose, have since that time undergone but little change, and it is fair to presume that, both remaining the same as of old, like causes would produce like effects still. But while the laws of matter and of vitality have remained the same, man's knowledge of them has been steadily enlarging, and, as a consequence, there has been a corresponding change in the means which he calls into requisition to correct the aberrations of vital action, as he has improved in the knowledge of the workings of the vital forces and the nature of their aberrations, and the nature of curative agents with regard to the relation they sustain to the vital forces. And I am proud to be able to say, in truth, for the profession of the present age, that they have made larger advances in obtaining a correct knowledge of the laws of matter and of life, than were accomplished in any other, or, indeed, in many other centuries; and I am glad that I can say that the practice of the present time is not what it was when I studied the rudiments of my profession: the change has been for the benefit of mankind, as well as for the honor of the profession of medicine.

But to return to the subject under consideration,

that the present prevailing practice is a stimulating one, and therefore this plan of mine is in harmony with the tendency of the age. I claim very little that is original; merely this, that I have pointed out, somewhat more clearly than was before done, the nature of the remote cause of fever, the kind of impression first made upon the system, and the effects produced by these impressions; and then pointed out the means by which these effects can be best removed. I have shown why stimulation should cure, and how it cures, and thus, by understanding exactly what is wanted to be done, I have been the better able to select the exact articles best adapted to meet the indications. I have said that opium, quinine, and spirits of turpentine are the means now most relied on in the treatment of fevers, and we know that they are all powerful stimulants; and I contend that it is exactly because they *are stimulants* that they have attained their present popularity. But they are not the best stimulants for this particular purpose. Opium is essentially a nervous stimulant; given in moderate doses, it excites the nervous centres and causes increased nervous power to be sent out through the whole system, and in this way becomes, indirectly, a general stimulant. But in large doses it overwhelms the brain and other nervous centres, and disqualifies them for sending out their usual influences; hence loss of consciousness, loss of sensation, diminished circulation, diminished or suspended secretion, diminished irritability—in short, diminished

vitality. And although there are occasions when the bringing about of this fearful state of things is not only proper, but imperatively called for, yet it is always obtained at a serious loss of vital energy, and should not be resorted to when less objectionable means will succeed.

The same objection lies against the use of quinine. It too is a nervous stimulant, and unquestionably produces its specific effects by stimulating the nervous centres: all who have taken it in moderate doses will testify to the pleasurable excitement it occasions, and all who have taken it in large doses can equally testify to its horrid effects upon the brain; no other kind of intoxication is so insufferable. And although I would regret the loss of this Sampson stimulant, still I would never use it for exciting the capillaries, by its indirect action through the medium of the nerves, when I could obtain a better result by the *direct* action of piperin, without any expense to the nervous system. It is true, I give in my prescription a nervous stimulant, the valerian, but it is one of a mild nature, not capable of producing intoxication like opium and quinine, and therefore not liable to exhaust the vital energies. Some persons with whom I have conversed on this subject have seemed to labor under some difficulty in understanding the difference between *direct* and *indirect* stimulation; this appears to me to be very plain: direct stimulation is obtained by exciting the part itself, indirect stimulation by exciting the origin of the nerves, by which a part is supplied:

the kidneys may be stimulated directly by giving a diuretic, and they may be stimulated indirectly, by acting upon the brain by fear. The stomach may be excited to vomiting by an infusion of mustard, or by presenting disgusting images to the mind, or by a concussion of the brain; so the capillaries may be excited by the direct action of piperin carried to them by the blood, or it may be done indirectly by exciting the brain and spinal marrow, and increasing the nervous irritability sent to them. But it may be asked, Why should indirect stimulation produce exhaustion rather than direct? I answer, first, because every unusual excitement of a nervous centre has a tendency to weaken its energies; we know that this is true, because every time a nervous stimulant is repeated, the dose must be increased, in order to obtain the same effect; this is true of alcohol, opium, quinine, and every other nerve-stimulant. And secondly, the reverse is true of direct stimulation: the more the senses are exercised, the more acute they become—that is, they are more easily impressed by their appropriate stimulants; the oftener emetics are administered, the less doses will be required to produce emesis; the oftener the surface is excited by friction or the cold bath, the more active it becomes; and so of all direct stimulants. The philosophy of it is, that they act not upon the nerves themselves, but upon the irritability of the part which the nerve has supplied. We must recollect, however, that even direct stimulation may be so powerful as to exhaust the irrita-

bility of the part, when no further impression can be made until it is restored; thus, the skin may be stimulated by rubefacients until it becomes insensible; the bowels may be over-stimulated by drastic purgatives until nothing will excite them; but, unless carried to this stage of exhaustion, the more any part is stimulated by direct action, the more impressible it becomes. I think these reasons are explicit enough upon the subject, and show why medicines which act primarily upon the brain exhaust the system, and should not be resorted to unnecessarily.

But to return to the subject. Spirits of turpentine is the next stimulant now most relied on in the treatment of fevers, and is much less objectionable than opium or quinine, as it is a simple, direct stimulant; but there are objections that lie against it also. It is disagreeable to the taste and offensive to the stomach, and, in considerable doses, is too powerfully stimulant to be borne well by the coats of the stomach and bowels; and when taken into the circulation, it often acts so powerfully upon the kidneys as to produce subsequent weakness in these organs. In the oil of sassafras I find a less objectionable remedy; it is pleasant to the taste, grateful to the stomach, harmless to the mucous surface of the alimentary canal; it readily enters the circulation, and acts as a valuable assistant to the piperin in stimulating the capillaries. Besides these important effects, as has been said before, it reaches the nervous centres and destroys or neutralizes the

remote cause, thereby preventing any further injurious effect upon the system.

MODES OF CURE.

Some, perhaps, may think light of my system, because it promises to do so much by means of so few remedies; but it is not more restricted in its means than many other systems which have at different periods obtained the ascendency and run the rounds of popularity. In fact, every theory of fever has suggested a certain class of remedies as the appropriate means for effecting the object thought necessary to be accomplished, and these means have generally been few.

The Humoralists relied upon a few bold detergents; the disciples of Cullen and of Rush, upon the lancet, and a few other debilitants; the Sympathetics, upon mercury almost entirely; Broussais, upon the lancet and demulcents; the followers of Cooke, upon calomel and the cava pills; and the present dominant practice embraces little more than the trio, opium, quinine, and turpentine.

The fact is, that this limiting of remedies is characteristic of science, and distinguishes it from empiricism.

In the dawn of medical knowledge, when there was nothing to guide the practitioner but blind experience, a doctor's prescription often embraced a multitude of medicines—sometimes as many as fifty, including those of the most diverse and contrary powers; and while some of these might hap-

pen to suit the case, others would be useless, and others injurious. It is the glory of the scientific physician of the present day, that he gives nothing without having a specific object in view, which he expects to accomplish by its exhibition; and as the elements of disease are always few, it follows that the better these elements are known and recognized, the fewer will be the means called into requisition to meet the indication.

It would be very strange if any theory should be originated by a great mind, and receive the assent of the learned for a considerable time, unless it were founded upon some ascertained undeniable truths. We accordingly find that each of the theories of fever which have received the confidence of the medical profession at different periods, was based upon some of the recognized phenomena presented in the progress of the disease, and hence all contain some truth. The ancient Humoralists perceived the nervous disturbance and the capillary engorgement, and if the case terminated favorably, they saw that much morbid matter was thrown off by the secretory organs, and believed that these vicious humors had caused the disease. Now it is a truth that morbid matter does accumulate in the blood in every case of fever, but it is a consequence, and not a cause: the secretions being suspended by the debility of the capillaries, and the spasm or altered sensibility of their secretory terminations, a retention of the excrementitious parts of the blood is the consequence, and a depravity of that fluid is the

result. But the *condition* of things which caused the fever and suspended the secretions must first be removed, either by art or nature, before the secretory organs *can* be made to act, so as to depurate the blood; so that the cure is actually performed before the expulsion of the morbid matter.

The next theory in succession—that of spasm of the extreme vessels, and peccant matter in the blood—was based upon the observance of pretty much the same phenomena as the pure humoral. Those who embraced this theory had their attention drawn to the spasm which evidently takes place in most cases of fever in some degree, and in high grades forms the most prominent symptom; and supposing that the engorgement of the capillaries was, in all cases, wholly caused by this spasm of their mouths, went to work to cure the disease with this idea prominent above all others. But this spasm is not the prime cause of the congestion of the capillaries, for the congestion takes place in the stage of depression, before there is any evidence of spasm, which only comes on during arterial excitement, and gives way when that is allayed. In low grades of fever, there is little or no evidence of spasm at any time; and in the collapsed stage of all grades, it is not only wholly absent, but the mouths of the capillaries are often so relaxed, that they pour out, as we have seen, the fluid constituents of the blood without check and without alteration; producing effusions, colliquative sweats, or diarrhœa. But the purple hue of the surface shows that the

capillary debility and engorgement are *still* not at all relieved.

The sympathetic theory was based less upon truth and reason than either of those which went before it; still, there may be found some support for it in analogy: we know that a specific action does obtain in some forms of chronic disease, by which a peculiar organization is produced, and until the specific action of the vessels is changed, they will manufacture no other product; but the action in fevers is not specific, but is simply too high or too low, and all that is necessary to be accomplished, in order to remove the disease, is to equalize the action; for as soon as this is done the fever is gone, though the patient may still be feeble and require time to regain his vigor; yet the moment there is harmony established between the action in the large blood-vessels and the capillaries, the fever is destroyed.

The Broussain theory being based upon supposed inflammation in the stomach and bowels—a circumstance which often does actually exist in many cases of fever, and causes that condition which constitutes the first stage of all inflammation, viz., nervous disturbance and capillary debility, followed by engorgement—had therefore many undeniable facts to lean upon; but as this condition of the digestive organs is only a part of a general condition obtaining throughout the entire system, it must therefore be a consequence and not the cause of the febrile movement; in other words, the aberrations ob-

served in these organs being coëxistent with like aberrations in every other part, must be referable to the same cause; and, in fact, all do arise from general nervous disturbance, which has produced general capillary debility. The one idea of the disciples of this theory was to subdue inflammation, which naturally suggested the lancet, demulcents, tisans, etc.

The next theory considered was that of venous congestion: this had a most prominent phenomenon, one that is conspicuously observable in every case of fever, to rest on. But it is strange that the far-seeing mind of Professor Cooke failed to perceive that venous congestion cannot, in the nature of things, ever take place without capillary obstruction: mere weakened action of the heart can never be the cause of venous congestion; for while the capillaries continue to act with usual vigor, there can be no oppression of the heart, for we see that they pass the blood on from the arteries to the veins, even after the heart altogether ceases its motion, and if the heart acts, though it should be feebly, it will send the blood on and relieve the veins, unless there is obstruction. We often see those whose pulse is ordinarily so feeble that it can but barely be felt, and yet the individual feels well—there is no venous congestion; and, on the contrary, we often find the heart in the hot stage of fever beating with many times its usual force, even causing the whole frame to tremble under the shock of its power; yet there is, nevertheless,

great venous congestion at the same time. So we see that Cooke was wrong in giving weakened action of the heart as the cause of venous congestion. Capillary inaction is the cause, and the congestion can be removed in no other way but by restoring the action of the capillaries.

CHAPTER VII.

MYSTERIES CONNECTED WITH FEVER.

Fordyce, in his Treatise on Fever, mentions a number of phenomena connected with its history which he considered mysterious, and professed himself wholly unable to explain. Subsequent writers have also failed to give any solution of these difficulties. Now, as I profess to have discovered the true condition of the system in which the *febrile movement* consists, and the nature of the remote cause which produces this condition, it might be expected that I should be able to point out, more clearly than had been done, the cause of the phenomena which grow out of the *febrile movement;* and I think I am not mistaken in the opinion that I can, in a good degree, succeed in meeting this expectation. I will, therefore, take up the difficulties as they are presented by Fordyce, and endeavor to show that they all are natural results, growing out of the nature of the remote cause of fever, and the condition of the system brought about by its ordinary operation.

The first mystery which Fordyce points out is,

that the remote cause is more operative in the night than in the day; the second is, that paroxysms of intermittent fever occur principally in the day; and the third is, that in continued fevers there is commonly an evening exacerbation. I will include the explanations of all these in one chapter, as they grow out of the same circumstances.

The points upon which these explanations are based have been pretty fully dwelt on in the preceding pages of this work, and will here only be stated, viz.: that the remote cause of fever is of the nature of a narcotic poison, a nervous depressor; and the condition of the system produced by its operation is that of narcotism or nervous depression, followed by nervous disturbance and capillary debility; and that the hot stage or reäction in fever grows out of these, with all the other phenomena, as pains, aches, suspended secretions, etc., etc.

It is a well-established fact, that exposure to the cause of fever in the *night*, is much more apt to bring on an attack than the same exposure during the *day*. Certain malarious districts, which can be visited in the day with comparative impunity, cannot be remained in during the night without the most imminent hazard of an attack of fever, and more especially if they be slept in. The same thing has been observed to be true, in a less degree, in every malarious district which has been made the subject of observation. But why this is so, has never been clearly accounted for.

There probably are several coöperating causes; but the one most operative is, as I think I can show, that at night there is a lower degree of vital energy possessed by the system than in the day. That there is less vital energy at night, is evident from many considerations: the system is much less impressible—which is a beneficent arrangement of our Creator, by which we are better able to compose the body to sleep. The same amount of noise or other annoyances will not keep a person awake in the night that will have that effect in the day; and this is not because of the habit of taking sleep at night; for I have conversed with a number of men whose business required them to be awake all night, and forced them to take their sleep in the day, who all inform me that slighter causes will prevent sleep than did when they slept at night. It is not because of the stimulus of light, for persons soon acquire a habit of sleeping in a strong light, and often prefer it. It can, therefore, only be accounted for by admitting that the nerves are less impressible in the night than in the day. Many other sources of evidence that this is so could be given if it were necessary; for example, we do not digest as well in the night; are more subject to take cold from exposure, etc.: all showing a weak condition of nervous energy.

Carpenter admits the fact that nervous *sensibility* is less in the night than in the day, and, among other evidences of this, refers to the well-known circumstance, that a much greater effect will be pro-

duced by drinking a given quantity of alcoholic liquor in the morning than in the evening. He does not attempt to account for this, but I presume it arises from the same cause which operates upon certain plants and flowers to induce them to fold up their leaves or petals in the evening, as though about to die, but which expand again in full vigor to greet the morning sun. Solar light, or something in connection with it, plays an important part in the business of vitalization. But not to go into any speculation as to the cause of the deficiency of vital power experienced in the night, I will assume it as a fact conceded by the best writers, and abundantly susceptible of proof. Even wagoners are fully aware of this fact, and hence will allow their teams to rest, especially in the latter part of the night, should they have to drive the harder in the day.

The traveller who rises before day to press forward on his journey, will find both himself and steed injured by the imprudence, and both will fail, if the journey be long, sooner than he who takes his morning rest, and pushes harder in the day, even beneath a tropical sun. But whatever the cause may be, it is certain that it is less operative in the *first* than in the *latter* part of the night. Fordyce supposes that the greatest liability to be impressed by the cause of fever, is between the hours of eight in the evening and eight in the morning, and that the liability is much greater in the latter half of this period than in the first, viz., between two and eight in the morning. Southern planters have

arrived at the same conclusion by observation alone, and hence find it better to let their hands remain indoors in the early morning, and work them later in the evening.

It would therefore seem, that either the supply of vital influence, which is generated during the sunny hours, lasts for some time after they have fled, or that the solar rays leave the air charged with vivifying power, which is operative for a time after they are shut off; and that it is only after the sun has again blessed the earth for a season, that this vital influence is felt in sufficient force to restore the nervous power to its full vigor. Hence the almost universal desire for stimulation in the morning, laying the foundation of the old custom of the *morning dram*, (better broken than kept, however,) and the still more universal one of the morning cup of tea or coffee. Hence, too, the well-known fact that the convalescent and the valetudinarian dare not venture abroad during the early morning hours. It is therefore certain that nervous sensibility and nervous power are both less in the night than in the day. But the inquiry may naturally arise, If the nerves are less impressible in the night, and the cause of fevers acts upon the nerves primarily, how comes it that a greater effect is produced? This question implies the idea that the cause of fever operates by producing nervous *sensation*. Now *sensibility* resides in the remote nervous expansions, and only there; and *sensation* can be excited in no other way but by impressions made upon these expan-

sions. But almost all writers upon the subject now agree that the poison which causes fevers does not act by impressing the sentient nerves on the surface, or in the lungs, or alimentary canal, but that it enters the circulation, and reaches the brain and other nervous centres, and there exerts some influence upon the nerve-matter, which sets up in it a morbid *condition*, and that morbid *innervation* is only a manifestation, an evidence, a symptom of this condition.

Now a living organization possesses the power to a certain extent of preserving itself from, or resisting the action of, hurtful agents; and this power of resistance is known to be in direct proportion to the degree of vital action that may be possessed at the time—and vital action, we know, is dependent on, or rather is the direct result of, nervous power. The brain and nerve-matter in general is as much dependent upon the vital action of the capillaries which enter into its structure for a due performance of its function as is the liver, or any other organ, and hence we can perceive a good reason why the nerve-matter should be more easily injuriously impressed when nervous power is deficient than when it is in full vigor. But perhaps some one may say, As you believe the cause of fever to be something having the properties of a narcotic stimulant, of a nature somewhat allied to opium, alcohol, etc., and as you have admitted that persons are less excited by these at night than in the morning, how is it that the reverse is true with regard to the fever poi-

son? This question arises from confounding nervous *impression* with nervous *lesion*. Now, though alcoholic stimulants do produce more *excitement* in the morning than they do at night, yet by being drunk at night they prove much more *injurious* than when drunk in the day: common observation has decided that he that drinks in the night will not live long, while the belief is equally general that a morning dram is good for health. Now, however erroneous the latter conclusion may be, it is certain that between drinking in the night and drinking in the day, the odds in the injury done the system are all on the side of the former. It amounts then to this, that narcotic stimulants do not injure the system by the amount of excitement they produce, but by injury done to the nerve-matter itself, and these effects are not at all the same, are not even coincident in either time or place.

Excitement is the immediate effect of the direct impression of the stimulant on the sentient extremities of the nerves of the stomach. This is felt in full force within a few minutes after imbibing the potation; an hour or so later, enough of the stimulant has been absorbed and entered the circulation to begin to manifest its deleterious effects upon the nervous centres: we now have a mixed effect of *stimulation* and *intoxication*. But if the draught has been large, in the course of a few hours more the direct effect made by the absorbed poison upon the nerve-matter so far preponderates over the impression made upon the nervous extremities,

(though the stomach may be again plied with fresh potations,) that excitement becomes as nothing, and intoxication profound.

I hope I am now understood, that stimulation is in the stomach, and is conveyed by nervous connection to the brain, and sent out to the whole system by reflex nervous action; but that *narcotism* or intoxication takes place in the brain itself, and other nervous centres, by the direct action of the absorbed poison, through the medium of the blood, and that the torpor of drunkenness is not a *sensation* sent out from the brain and spinal chord, but a want of *nervous power* occasioned by a *lesion* of the brain matter, by which it is disqualified for performing its functions of elaborating nervous fluid.

It therefore can be readily understood how this low state of nervous power, which has been shown to exist at night, should render the system more susceptible to the deleterious influence of narcotic poisons, whether alcohol, opium, or malaria, and that we should expect the cause of fever to more readily impress the system at night than in the day.

The next inquiry is, Why do the paroxysms of intermittents occur principally in the day? Fordyce says, that so great is this tendency that a retarding intermittent, after having travelled through the hours of the day, in place of keeping on through the dark hours, will leap over the night and occur the next morning; and an anticipating intermittent, in like manner, will travel backward through the hours of the day, and, skipping over the night, will

come on the previous evening; so that in an anticipating quotidian there may two paroxysms occur in the same day. Now, preparatory to an attempt to explain this phenomenon, and give a reason for it, it will be well to consider what is the cause of a paroxysm of intermittent fever. I am aware that *chills* and *rigors* are supposed to be the result of *depression*, and so they are remotely, but not immediately. A regularly built chill or *shake* of an intermittent is really the result of *reäction*—it is the first unsteady rebounding of the nervous power, and the more vigorous the rebound is, the more violent will be the rigors. Fordyce gives the full weight of his great character as a close observer in support of this position, though he merely stated a fact without perceiving its tendency. He says that whenever there are *rigors*, the patient will not *die* in that *paroxysm*—showing that rigors are evidence that reäction has commenced. Everybody in the South and West knows that if a fever is turned to the *shakes*, the danger is over. Is it not fair, then, to infer, as the fully developed rigor is the result of reäction, that the less degree of the same state, known as *chill*, is occasioned by the same thing, less operative? But I acknowledge that there are conditions, called chills, which are not the result of reäction, as in low grades of pernicious fever; but in them there is no sensation of a chill; on the contrary, the patient has a sensation of heat. I cannot now stop to explain this condition, but it is very different from the first stage of a paroxysm of ordinary intermittent fever.

But though I contend that rigors are the result of reäction, it is not arterial reäction, but is simply a rebound of the oppressed nervous power, and when the nervous reäction becomes tolerably perfect, it brings about arterial reäction. We are now prepared to comprehend why the increased nervous power and nervous sensibility, in the day, should invite nervous reäction; and why it does not often take place in the night, when nervous power and sensibility are lowest. So that, while the low state of vitality in the night favors the impression of the remote cause of fever, the higher state of nervous power in the day excites the reäction which produces paroxysms of intermittents.

We have only now to find a solution of the last query. How account for evening exacerbations in continued fevers? Every writer admits that fevers are usually worse in the evening than in the morning, but the cause has been an enigma, about which few have ventured even a conjecture. But admitting the conclusion, which has been fairly arrived at, that there is a lower degree of nervous power in the evening than in the morning, and another fact, which is generally conceded, viz.: that malaria enters the circulation—to these append the conclusion, which I think has been honestly drawn, that malaria is of the nature of a narcotic poison, we can then easily perceive that it, like all other narcotics, will exert an influence upon the nervous system, more or less readily, as nervous power at the time may be more or less vigorous; and that,

as the morbid sensations which are felt in the system are nothing more than symptoms of the altered condition of the nerve-matter at the time, it is reasonable to suppose, nay, is absolutely certain, that these symptoms will be aggravated just in proportion as the impression upon the nerve-matter becomes more overpowering, and the resistance afforded by vital action less. We would therefore expect, that though the continued force of narcotic power of the remote cause remains the same in the morning as in the evening, yet its effect in producing nervous disturbance would be as much greater in the evening as the vital action is weaker. And even should the original cause have ceased to act, and the disease be the effect of the *condition* brought about in the nervous centres, yet it is certain that this condition would interfere more potently with the exercise of the natural function when the vital action was lowest than it would when more lively, and that greater nervous disturbance would be the consequence. But we have shown that the *febrile movement* is the joint result of nervous disturbance and capillary inaction. Now, though the action of the capillaries is not *immediately* dependent upon nervous influence, yet the *power* to act is certainly derived from the nerves, and is soon lost if nervous power is cut off; so that a lower degree of nervous power will soon produce a lower activity in the capillaries also; and hence, the whole mystery of evening exacerbations in fever can be unravelled as fol-

lows: In the evening nervous power is less, and vital action weaker; and, as a consequence, the effect of the morbid condition produced by the remote cause of fever in the nervous centres is proportionally greater in causing morbid innervation, giving rise to increase of pain, restlessness, throbbing of the heart, and other evidences of nervous disturbance. Further, capillary action is in proportion to nervous power, and this being weaker in the evening, capillary action will be weaker also, occasioning, by necessity, an exacerbation of fever, for fever consists in nervous disturbance and capillary inaction—the disturbance in the general circulation, equally with all the other phenomena of fever, being a consequence of these; for the heart being supplied with morbid nervous influence, and being unduly excited by the stimulus of distension, occasioned by the partial stoppage of the blood in the capillaries, causing it to accumulate in the large arteries, increased action is the consequence. We therefore have, as the elements of an exacerbation of fever, increased nervous disturbance, decreased capillary action, and excitement of the heart and arteries; manifested by an increase of the pain, restlessness, headache, delirium, heat of the surface, paucity of secretions, etc.

But the reäction in the arterial system will, after a while, partially relieve the capillary torpor, and a better capillary circulation will cause an increase of nervous power; better nervous influence will be sent

throughout the system, a general amelioration will take place in all the symptoms, constituting the *morning remission.*

It appears, therefore, that answers to all the three inquiries which form the present subject can be found in the same facts, and that all can be contained in a nutshell, viz.: the cause of fever being a nervous depressor, and nervous power being lower at night, and vital action of course weaker, the nervous centres are, consequently, easier impressed injuriously; and that, although nervous power has a tendency to reäct after depression, yet it cannot do so until it gains force, or is aroused by some stimulant; therefore, no reäction takes place ordinarily until the stimulus accompanying the sun's rays arouses it; the first effect of which is manifested by that kind of reäctionary effort of nervous power which produces the constriction of the surface characterizing a chill, or the irregular muscular action producing rigors. But sometimes this nervous reaction is so very slight that nothing more than a little shrinking and dinginess of the surface are perceptible; and this, too, rather late in the day, so that the arterial reäction which follows is continued into the fore-part of the night, making the *evening exacerbation* in continued fever.

HOW FEVER TERMINATES IN HEALTH.

Speaking of the natural tendency of most fevers to terminate in health, Fordyce says, page 121: "The author has, therefore, never seen, nor can him-

self conceive of any cause why a simple paroxysm of fever should go through its three stages, (cold, hot, and sweating,) and terminate in health." Now, this does not appear to be so very mysterious; the whole process may be explained as follows: The malaria, whatever else it may be, is a nerve stimulant of a depressing kind; the nerves bear the impression for a certain time without yielding to its influence, or, in other words, becoming impressed; just as they will stand a certain amount of tartar emetic, (which is known to act by entering the circulation and impressing the nervous centres, particularly at the origin of the gastric nerves,) but if the amount be increased, there comes a time when they can resist it no longer, and great nervous prostration and sickness are the consequences; just so the nerves or nervous centres may be annoyed by the action of malaria, and appear not to feel it, but finally they become impressed, and then the condition of nervous prostration is at once produced which characterizes a chill or cold stage of fever. But, as we see in the case of tartar emetic the nervous depression giving place to reäction, causing violent contraction of the gastric and other muscles, a sense of heat over the whole body, and increased power and velocity of the heart's action, so it is in the other case also—the nervous system reäcts, and increased nervous power or influence is sent throughout the whole system; the heart is called into increased action, the sensibility of the whole system is exalted, and all the phenomena of the hot stage

of fever are witnessed. And were it not for the debility of the capillaries which had been brought about by the previous action of the malaria, either by direct or indirect influence, the paroxysm of excitement would be, in this case, as ephemeral and would do as little harm as that occasioned by the tartar emetic. But the capillaries offering a resistance to the passage of the blood, all the phenomena of capillary congestion are added to those of arterial excitement, and the hot stage is protracted until the excitement has aroused the action of the capillaries—the first evidence of which is that those which compose the exhalents of the surface commonly relieve themselves of the congestion by pouring out perspirable matter; the other capillaries also go to work, but are not so immediately under our observation; the mouth becomes moist, dryness leaves the throat, the urine is again secreted, etc., etc. Now if the nervous debility of the capillaries has been but slight and of a short continuance, the excitement of the hot stage often restores them completely, and unless the remote cause is continued in its action, there will be no tendency to the production of a second paroxysm; and we often see examples of this in fevers occasioned by mere fatigue and exposure. But, generally, the debility of the capillaries is only partially and temporarily relieved by the reäction, and when it subsides in consequence of the relief of congestion obtained by secretion, the debility still remaining, and the first cause of the nervous disturbance still in action, a

second paroxysm is brought about, precisely as the first was; but as the nervous disturbance will generally become less and less, either by the remote cause ceasing to act, or from the nervous matter becoming less and less sensitive to its influence, the capillaries become more and more perfectly restored to their natural condition, and the paroxysms will cease to return, except from mere force of habit, and this is often broken up by the occurrence of some considerable mental excitement, produced by accident or purposely planned. Thus we see that it is very natural and reasonable that the regular stages of fever should terminate in health.

HOW FEVERS CURE OTHER DISEASES.

Another circumstance which has been noticed by Fordyce and others, as often observed in association with the history of cases of fever, and which has been considered inexplicable, is that other diseases which the individual had been suffering under, at the onset of the attack of fever, are very often removed and disappear with the subsidence of the fever. Fordyce says, "Fever thus, and many other violent diseases, (which go on in a similar manner,) proceeding through their natural course, often leave the patient free, not only from the disease itself, but also from the decay arising from some less violent disorder, that had not in itself a natural tendency to terminate in health. Perhaps the manner in which this happens will ever remain inexplicable." But when we remember that the

elements of diseases are few, how varied their effects and manifestations may be, and consider, too, the nature of these elements, it will not appear strange at all that the removal of the *condition* which causes the phenomena of fever will also remove the *condition* in which various other diseases essentially consist. In analyzing diseased action, we find it resolves itself into a very few simple elements: these are, alteration of the condition of the nervous power or sensibility of the local part, or of the whole system; diminished or capillary action; and the changes of structure or morbid deposits, which are the results of the deviation from a normal condition of the nervous influence and capillary action. Now many diseases, which are very annoying, consist entirely in debility or perverted action of the capillaries, and it is reasonable enough that the reäction of the nervous influence, which brings about the hot stage of fever, and the general capillary excitement which is occasioned by that hot stage, should not only arouse the capillaries in general into healthy action, but also bring up these, which were particularly concerned in making up the disease previously suffered, to the point of healthy exercise; and thus, by removing the very *condition* out of which the phenomena of that disease grew, it, as a matter of course, could afterward have no existence.

We will take the case of an indolent ulcer, as a good example for illustration. We know that there is want of nervous influence: its insensibility

to applications which would produce great sensation when applied to a recent healthy abraded surface, proves this; there is also much capillary debility, as is manifest from the congested state of the part and its venous color; the nervous power and capillary action, therefore, are too low to bring about a reparation of the lost parts; and when efforts are made, the vitality of the product is of too low an order, and it proves an abortion and is sloughed off, or is removed by destructive absorption. Now, the general reäction in the nervous system, and the general excitement in the capillary system, characterizing a natural and well-developed case of fever, often extend with such efficacy to this local part as to bring it under the general influence, so as to permanently elevate it into a natural condition; and when the process of reparation is fully developed, which restores the loss of *material* which is suffered in the course of every case of fever, this part also is included in the general reparative effort, and a radical cure is the result. But sometimes we see the very reverse of this take place: instances are numerous in which the general powers of the system are so far exhausted by the ravages of a protracted case of fever, and especially if its vitality has been further depressed by the improper exhibition of mercury and other depressing agents, that although a reäction is set up in the general capillary system sufficient to bring about a tardy convalescence, yet some of the parts most remote from the centre of vital influ-

ence, or which have been more seriously debilitated by local applications, or possess naturally a lower order of vital action, fail to respond to the general recuperative reäction; and destructive absorption destroys the integrity of the part, and very obstinate ulcers are the consequence. In other cases, the general reäction is barely sufficient to prevent actual dissolution, and the whole capillary system remains permanently debilitated; every function is performed with difficulty and imperfectly, causing the patient to linger out a miserable existence, poorly compensating him for a brief respite from death. In other cases the capillaries of some important organ, which had suffered most during the progress of the fever, remain debilitated, and the patient suffers from a want of the performance of its particular function. It may be the stomach, causing indigestion; the bowels, producing costiveness or chronic diarrhœa; the liver, spleen, or kidneys, occasioning the evils resulting from an imperfect performance of their office; or, what is still more deplorable, it may be the capillaries of the brain itself which remain debilitated, producing mental imbecility or idiocy. It is, therefore, of the highest importance that a case of fever should not only be managed so as to save the life of the patient, but so controlled that no permanent injury shall result to the system; and, better still, that the nervous and capillary reäction be made so perfect as to swallow up, as it were, any previous local debility which may have been set up in the system,

making the attack of fever prove a positive *good* to the patient, in place of an *evil*.

No plan of treating fever, of which I have any knowledge, can be compared in efficacy with the one I have described in insuring the above desirable results. And the reason of it is plain: it removes the disease without occasioning any shock to the system, without any expense to the vital powers, and without exciting over-action in any of the organs; its action on the system being a general one, and one in unison with the natural efforts of nature, viz., by directly allaying nervous disturbance, and exciting capillary action. Since adopting this plan of treatment in fevers, I had seen so many instances of local diseases yielding during the progress of the cure, that I afterward acted upon the hint; and, in other cases, purposely brought about the same general capillary excitement for the removal of the local debility, upon which chronic diseases are mainly dependent for their continuance. By this means, aided by external stimulation, I have succeeded in this city in curing many cases of obstinate scrofulous, syphilitic, and mercurial or fever sores, which had resisted all ordinary means for years. But I will speak more fully of this subject under its proper head.

HOW TARTAR EMETIC ACTS IN AIDING OR PRODUCING A CRISIS.

Dr. Fordyce noticed the effects of tartar emetic in producing a state of the system very similar to

that in the *crisis* of a paroxysm of fever, and often availed himself of this effect of that remedy to obtain a more perfect crisis than would otherwise have taken place; but he was altogether at a loss to account for the *mode of its action.* He says it was not owing to its emetic or nauseating properties, for it succeeded more perfectly in bringing about the *condition* referred to when it occasioned no sensible effect upon the stomach. These beneficial effects which he refers to, he describes as follows: "The skin becomes softer and moister; sometimes a profuse sweat; the mouth becomes moist; secretion is set up in the kidneys and intestinal canal, and a sense of ease is felt over the whole body." Now it does not appear very hard to explain these effects of *tartar emetic;* its general operation upon the system is certainly that of a powerful depressor of nervous power; its emetic effect is partly owing to its local action on the stomach, and partly (perhaps principally) to the general reäction which succeeds nervous depression. There is, I suppose, no real similarity in the mode of action of the nervous power and that of an elastic substance, but there is a striking *analogy* in the apparent phenomena of these operations: the further you bend a steel bow, the greater will be the rebound, unless you bend it beyond its power of reäction, when it will either break or remain permanently bent. It is so with the nervous power: a slight depression will be succeeded by inconsiderable reäction; a profound depression by powerful reäction; an overwhelming

depressing influence will at once break its integrity, or so cripple its powers that it can never recover its energy. Tartar emetic is capable of producing all these effects. An impression may be produced by it upon the nervous system so depressing as to extinguish life at once, without an amount of reäction taking place sufficient even to produce sickness, much less vomiting; but it may also be given so as to impress the nerves so slightly, that even nausea is not occasioned; and the only appreciable evidence of reäction is increased capillary action, by which all the secretions are augmented. This is a most valuable property of that medicine, and may be taken advantage of in many cases to serve our purpose most admirably in our contest with morbid action. When there is a high state of general irritability or actual inflammatory action, I often suspend the use of my common fever syrup, and give emetic tartar, in *sub-nauseating* doses, until the active excitement is subdued, and then resume the syrup for the purpose of establishing permanent capillary action. The mode I prefer in which to administer the *emetic tartar*, is to add a grain to a common glass tumbler-full of water, and direct the patient to take a tablespoonful every half hour, until very slight nausea is produced, and then lengthen the interval.

OF DELIRIUM IN FEVER.

Fordyce describes two kinds of delirium which often attend continued fevers. One is characterized

by hallucinations: the patient fancies he is talking with some person, but when spoken to this delusion vanishes, and he will answer correctly questions directed to him. This form of delirium is often quite violent in the evening and first part of the night, but subsides before day, and leaves the patient calm and quite rational. It is not attended with much stupor or obtuseness of perceptions, feeling, or intellect; and there is but little redness or injection of the eyes; and dissection discovers no trace of injury sustained by the brain. The second kind has many of the manifestations of the first, but differs in this, that the patient is not so readily aroused to a state of consciousness, and in the morning remission remains more stupid and insensible; the eye is duller and more injected, and the countenance more flushed. Dissection always reveals an injected state of the vessels of the brain. Fordyce is disposed to believe the first kind is purely an affection of the *mind;* but this conclusion is arrived at merely because he has not been able to conceive of an alteration of the condition of the brain which would produce it, and yet leave no trace after death. It is very strange that this should have presented any difficulty to his mind, when he must have known that febrile action throughout the entire system consists in merely functional derangement; and that it is only when engorgement takes place in a particular part, so intense as to destroy the ability of the capillaries to again contract and so resume their natural size, or when actual inflammation has been set

up, that any trace of the effects of the febrile movement can be detected after death.

The brain is as much concerned in producing thought, sensation, and nervous power, as the liver is in making bile, or the kidneys urine; hence, a functional derangement of the one will just as certainly and as naturally occasion an increased, diminished, perverted or suspended exercise of its appropriate duties as it will in the others. What the *mind* is, independent of organization, we have no means of knowing; all its manifestations are made known to us through the medium of organized matter; and as long as that connection exists, it can only give us healthy manifestations while the medium through which or by which it operates is in a healthy condition. It is not surprising, then, that the milder grades of delirium should be produced by the same capillary inaction and engorgement which give us the functional derangements which we continually see in the biliary and other secretory organs during the progress of a fever; or that dissections should be no more successful in showing an altered condition in the one case than in the others. All are the result of altered action, produced by a distension of the capillaries, which their contractility removes after the heart ceases to force the blood into them, leaving no marks of their previous condition. But if the distension of the capillaries is carried beyond a certain limit, they lose the power of contraction, and, in very low grades of fever, delirium gives place to coma; and dissection shows the capillaries of the

brain distended to their utmost capacity with black blood; other organs reveal the same condition. But if actual inflammation is set up in the brain, or any other organ, such alterations are speedily produced as death will not entirely deface; and the second variety of delirium described by Fordyce is undoubtedly the result of inflammation, more or less intense in this viscus. The symptoms indicate this, viz.: the throbbing of the temples, the redness of the eyes, the suffusion of the face, and the intellect remaining unrestored during the morning remission.

CRITICAL DAYS.

Fordyce appears to be at a great loss to account for the occurrence of what are called *critical days* in fever; and, after making some very lame attempts at an explanation, abandons it as one of the unsolved mysteries connected with this disease. Now, if I were to attempt an explanation, I would do it precisely as I should the appearance of *ghosts:* that is, by denying their existence except in the imagination of those who pretend to have seen them. The mere statement of the days supposed to be critical by those who contend for their existence is proof enough of their non-existence. Hypocrates and others designate them as follows: The fifth, sixth, seventh, ninth, eleventh, thirteenth, fourteenth, seventeenth, twentieth, and thirty-first. These are the *regular* critical days; then there are enumerated others which are called spurious critical days, as the eighth, tenth, and eleventh, etc. This is much like

some of the almanacs that make prognostications of the weather: they will say that "rain, more or less, may be expected on the third, tenth, seventeenth, and twenty-fourth of this month; the day before, or the day after;" so that if it rains at all, it will come with a good deal of certainty upon one of those days. But the sticklers for critical days have left even less chance of a failure, if the fever yields at all, for they have embraced nearly the whole time consumed by an ordinary case of continued fever, from the period of its full development to its usual period of termination.

Now all contagious fevers, we all admit, naturally run through certain stages—which stages consume nearly the same time in all cases: the duration of these fevers is, therefore, quite uniform; but this kind of termination is not what is meant by *crisis*, and therefore the usual days on which these fevers terminate cannot be called critical days. If a case of small-pox were to terminate suddenly in convalescence on the third or fourth day upon the occurrence of a hemorrhage, sweat, or diarrhœa, these discharges would be considered the cause of the sudden breaking up of the disease, and hence would be called critical discharges; and if it were perceived that such critical discharges were more apt to take place on particular days, these days might be properly called critical; but the occurrence of these discharges in the course of contagious or self-limited diseases has never been known to suspend the disease unless by causing death; hence there are no

critical days recognized as applying to that class of fevers.

But other forms of fever, particularly those caused by malaria, of which common intermittents are a familiar example, seem to have no very definite period to run: if left to take their own course, they will continue until the powers of the system either sink under their influence, or reäct and arise above them, breaking the chain of morbid action which sustained them, or in which they consist. Now, whenever the vital forces obtain this ascendency, either by their own inherent power, or aided by appropriate assistance, then the various secretory organs at once resume their functions, and a restoration of the usual discharges of excrementitious matter takes place of course, and generally in greater quantity than in health; because, first, the vital powers have obtained the supremacy by *reäction*, and this reäction generally will go beyond the natural standard; and, secondly, there is more excrementitious matter to be eliminated than usual, and therefore the same amount of vital action will throw off a larger amount of appropriate secretions immediately upon the breaking up of a case of fever than in health. But is the return of these organs to their duty the *cause* of commencing convalescence, or is it the *result* of some anterior change, and only the effect of that change, and therefore not the prime cause of convalescence, but only a sign of its commencement? Undoubtedly it is the latter. These secretions can be and are often set up under the in-

fluence of powerful remedies addressed immediately to the sensibility of these organs, but convalescence does not, as a matter of course, follow; on the contrary, the general powers of the system are often materially prostrated by their action, and the disease left still remaining in all its stubbornness. No discharge can therefore be a critical one in the sense in which that word has been generally understood, that is, that the discharge itself carries off the disease, either by eliminating the *peccant matter* of the ancient humoralists, or by giving vent to the black blood of the modern humoralists, or in any other way, corresponding with any other theory. And yet these discharges are critical in another and more appropriate sense: they indicate that a *crisis* has arrived, when the powers of the system have triumphed over and removed the *condition* in which the febrile movement commenced, and by which it was continued. If this error with regard to critical discharges had been a harmless one, it would not have been dwelt upon so lengthily here; but out of it have grown most of the errors in practice by which this formidable family of disease have peopled the nations of the dead much faster than they would have done under the influence of treatment directed by a more enlightened view of the subject, or, perhaps, if left to run their course without the intervention of any treatment. But what is the change which takes place in the *condition* of the system which gives rise to these discharges that may be called *critical*, because indicating that a favorable *crisis* has

arrived in the disease? It must be the removal of *that* which checked or suspended these secretions during the continuance of the febrile movement. In another place I have endeavored to prove that general capillary debility is the condition produced by the anterior causes of fever, out of which all the subsequent phenomena arise; and that without this condition there can be no such thing produced as the febrile movement. Now, a general capillary reaction is exactly what precedes and gives rise to general secretory action. When nature cures a fever, she does it precisely in this way: the remote cause either ceases to act upon the nervous system, or the nerves cease to be impressed by it, and the nervous disturbance gradually subsides, and nervous power gradually increases until an amount is sent to the general capillary system sufficient to overcome the torpor, debility, or want of action in them, which has been the cause of their dilatation, congestion, and slowness of circulation, from which the scanty, depraved, or suspended secretions, and all the other concomitants of fever, have proceeded. Now it is plain enough that as soon as this is effected, as soon as the capillaries are permanently aroused to action, then, as a consequence, the secretions will all be again set up, and all the other phenomena which arose from this common cause will also disappear. Hence, quieting nervous disturbance, increasing nervous power, and exciting capillary action, should be the grand objects of every effort to break up the febrile movement.

HOW AN INFLAMMATION MAY CURE A FEVER.

Fordyce has observed that an inflammation, coming on spontaneously, or purposely set up in some part not essential to life, during the progress of a fever, sometimes arrests the febrile movement, but is wholly unable to tell how this beneficial effect is brought about. He very properly discards the notion that the cause of fever is attracted by the inflammation, and through the medium of the suppurative process disappears from the system; neither can he allow that it is done upon the principle of revulsion. To my mind, the *modus operandi* of local inflammation in curing fever is just as plain as that of any other agency. It acts precisely in the manner of every other successful means—viz., by capillary stimulation; the nervous excitement produced by the inflammation, by being conveyed to the brain and spinal marrow, and thence by the medium of the nerves sent out through the whole system, may, when the capillary action is not very great, excite them sufficiently to bring them up to the healthy standard; and these being once set in motion, the phenomena of fever will as certainly subside as will the waters of a mill-pond when the flood-gate is raised. But inflammation, in order to be beneficial, must not be set up in a very vital part, or be very extensive in parts less vital, or the nervous impression will be so powerful as to produce depression of nervous power in place of excitement; hence we perceive the depressing influence of active in-

flammation in the stomach, peritoneum, etc.; and hence, also, very large blisters applied to children, or adults of a highly nervous temperament, sometimes produce fatal nervous collapse. An inflammation, in order to be beneficial, therefore, must not be intense enough to create nervous *disturbance*, or extensive enough to produce nervous *exhaustion*, but just sufficient to occasion a gentle nervous *excitement*. It is, therefore, improper to blister while nervous excitement is high; and it is unavailing or pernicious to blister when the nervous power is very low and capillary debility profound; for then it will either not produce any sensation, and, consequently, no reflex action, and therefore do no good; or it may produce a powerful *sensation* and occasion great nervous disturbance, and yet be wholly inadequate to arouse the torpid capillaries, and the excitement consequently occasion a further exhaustion of nervous power. We often meet with this condition of the system, particularly in cholera, congestive fever, and in profuse hemorrhage: capillary action seems to be wholly suspended in the surface and in the extremities—they are cold and shrunken, and of a leaden or purple hue; but yet sensibility is at its highest point of exaltation; the ordinary warmth of a healthy person's hand often occasions an insufferable sensation of heat; sinapisms are intolerable; and a blister at this time will prove the cause of most intense agony, and aid greatly in deepening the collapse. I was once called to a gentleman who was laboring under a

severe attack of cholera; I found him verging into collapse when I arrived, but by the use of judicious means, or by the powers of nature, his disease was kept in check more than ten hours. At this time two other physicians arrived, and upon consultation it was agreed to try the effect of a blanket wrung out of hot water and wrapped around his person. I did not approbate the measure, but as I had exhausted my resources and had obtained nothing more than a truce, the enemy still holding entire possession of the citadel, I could not reasonably object to a measure which two intelligent physicians thought would increase the man's chance for life. The measure was therefore carried into operation; but I shall never forget the awful expression of agony which the hot blanket occasioned. His flesh was as cold as marble when the warmth was applied; but, notwithstanding the heat was not greater than was quite bearable to my hand, the patient complained as if it had been red-hot coals of fire. But he was not destined to endure long. A scream, a flounce or two from side to side, a few moments of trembling, a gasp as from suffocation, and all was still: the blast had been too strong; the taper was blown out.

HOW DO CONTAGIOUS DISEASES SECURE IMMUNITY FROM THE DISEASE THEREAFTER?

A certain class of diseases usually considered contagious, it is known, very rarely are produced a second time in full force in the same person. Many

conjectures have been indulged in with respect to the cause of exemption. A learned professor supposes it may be that the vital sensibility which is enjoyed by the mouths of all the capillaries which afford ingress into the system, and which is placed there for the purpose of enabling these vessels to choose what is proper to enter the circulation and refuse that which would prove deleterious, may be off their guard one time, and allow this subtle enemy to enter; but having been once caused to suffer severely by this mistake, they can never be deceived again by the same agent. But this guarding sentinel, whose office it has been supposed is to protect the avenues to the circulation from hurtful intruders, is itself but a figment of the brain, having no existence anywhere but in the imagination. Of this we have the most positive evidence, for we continually meet with instances in which the most hurtful matters are actually taken up by the absorbents. It appears that there is no limit to absorption, except that arising from the insolubility of substances and their acrid qualities. Very pungent or irritating matters cannot be absorbed, because they either stimulate the absorbent vessels so intensely as to cause too great a contraction to admit of their ingress, or the impression is so powerful as to paralyze them and suspend all action; and of course all absorption ceases until a return of vital energy takes place. The only conditions therefore that are necessary to insure the absorption of the most deadly poison as readily as the most healthful

nutriment are, that it be soluble in water or the juices of the chylopoetic organs, if introduced by the mouth, and that it possess no powerfully stimulating or irritating properties; or, if it be a gas, that it be mixed sufficiently with the atmospheric air to render it respirable.

But we not only have evidence that the poison of contagion *might* be absorbed a second time, but proof positive that it often *is*. It is well known that persons who have had small-pox, by being afterwards exposed to the contagion, often take it in a modified form, and, consequently, the poison must have obtained ingress into the system; and this is rendered more certain by the fact that unprotected persons have taken the true variolous disease by coming in contact with one laboring under it in the varioloid form. Exemption is not therefore obtained because the poison cannot get into the system. The only good reason of which I can conceive why persons will ordinarily take these diseases but once is, that the poison which causes them is a narcotic stimulant. We know that some poisons of that class will very rarely fully impress the nervous system more than once. Take tobacco, for example: few persons who have acquired the habit of chewing this weed but have a vivid recollection of the horrible sickness and nervous prostration which followed their first initiatory effort at using it. But though a second attempt, perhaps, was not made until years afterwards, yet no violent effects followed. I had a personal acquaintance with a gentleman (a

distinguished divine) who, from motives of propriety, discontinued the use of tobacco after having been a constant user for some years, and continued this abstinence for over twenty years; but, being attacked with a dropsical disease, his physician advised him to resume the use of it, and he informed me that he used as much the first day of recommencement as he had been in the habit of using when he left off the practice, and that it affected him *no more* than formerly. The same thing is true of opium and alcoholic drinks, but not to the same extent. A full impression, amounting to complete intoxication, cannot be reproduced but by increasing the dose taken. The very same amount of whiskey which will throw a novice upon his back, may be repeated a few days subsequently, and will hardly make him stagger; and if often repeated, will at last scarcely affect him at all. It is just so with the poison of contagion. The first exposure to small-pox will produce very violent effects; a second will occasion a much milder effect; and perhaps the impression of the third will not be appreciated. But all contagious diseases are not preservative against subsequent attacks; and it will be observed that those which are not, make their first impression on some *external* part, and are therefore not of the narcotic kind, are not addressed to the nervous centres, and therefore one attack gives no security against a second.

DOES THE CONTINUED ACTION OF THE REMOTE CAUSE, AFTER THE OCCURRENCE OF FEVER, INCREASE ITS INTENSITY OR INTERFERE WITH ITS CURE?

Fordyce thinks it does not; but, with all due deference to the opinion of that correct thinker and accurate observer, I must differ with him in this conclusion, and for the following reasons: Every physician who has had a large country practice, embracing localities differing materially in their aptitude for generating malaria, could not have failed to observe that those cases of fever which occurred in the most malarious districts, and which remained there during the attack, were much harder to manage, and more apt to relapse, than such as happened to be removed to some friend's house in order to be nursed, who lived in a healthier vicinity. I resided for some years in McConnelsville, on the Muskingum river, in Ohio, and there enjoyed a fine opportunity for witnessing examples of the advantage of removing from a sickly to a healthy locality, after the accession of an attack of fever. The country on each side of the river is high, broken, and well watered; the banks of the river high, and its current rapid, so that it would seem that very little malaria ought to have been generated; and I suppose there was not much formed in that particular region. But this stream heads up in a very rich, flat, marshy country, where all the materials for producing malaria abound, and miasmatic diseases are rife almost every year. I

said that the Muskingum had a rapid current, but there was a great difference in its rapidity at different points: for a mile, or perhaps several miles, it was very gentle, and then for a shorter distance it was exceedingly rapid: (I speak in the past tense, for at present all parts are alike, the river having been dammed so as to produce slack-water navigation.) At the time I lived there, there were many salt-works in operation along the river, which gave employment to a great number of hands; who, added to other inhabitants, made the valley of the river quite populous. Now, those who worked or resided on the bank opposite to where the water ran gently, very rarely became sick; but fevers were exceedingly common among those who occupied a position opposite the rapids; and especially after a rise of water succeeding a hot, dry spell of weather. As many of the operatives at the salt-works were young men whose relatives lived among the adjacent hills, it was very common to have them removed there when taken sick, and I discovered a very marked difference in the stubbornness of the disease in those who remained, and those who "fled to the mountains;" and relapses were very common among the former, and rare among the latter. I will here digress from the subject immediately under consideration, to pay a passing notice to a fact already hinted at above, which elucidates one of the habits of malaria which has not been much noticed—that is, that it may be absorbed by or mingle with water, and be carried

by the current a long distance, and then set free by agitation and become exceedingly active. But to return to the subject, that the cause of disease by continuing to act upon the system will increase the violence of a fever, and invite relapses: Some years since, while practicing in Wilson county, I was called to attend on a family which resided near an extensive mill-pond on Round Lick Creek: one after another was attacked, and all proved exceedingly intractable to the influence of remedies, and all suffered frequent relapses, until it became quite evident that no effort I could make would save them from fatal prostration, if they remained under the depressing influence of that malarious atmosphere. I accordingly recommended a change of location, although three of the family were then not able to turn themselves in bed; but they were lifted on their beds into a wagon, and conveyed about four miles, to the residence of the gentleman's father, which was situated in a locality proverbially clear of malaria. The change was not only profitable, but remarkable; and, with but very little further medication, all rapidly recovered. I could narrate many other examples equally as striking as the above, which have come under my own observation; but every observer, whether he is a physician or not, can doubtless furnish examples from his own memory which will be amply sufficient to prove my position.

But is this true of the action of the remote cause of all other fevers? Not perhaps to the same

extent. In epidemic fevers it is equally true; but in contagious fevers it is not. In these, the remote cause, after having once set up the chain of morbid action which characterizes the peculiar disease, cannot, by continuing to act, set up a new series of morbid movements; but it can do this: it can exert a depressing influence upon the system, which will lessen its ability to successfully bear up under the disease while it runs its course. It appears that during the progress of every febrile disease there is some kind of emanation proceeds from the person of the patient, which, if it does not excite the same identical disease in another, will at least prove a means of aggravating any other form of fever under which the individual may be laboring at the time. A number of fever patients, therefore, in the same house, will stand a less chance of recovery than if they were each in a different house, although no two may have the same form of fever. This is the only rational cause that can be assigned for a greater mortality in hospitals than in private practice. It will not do to say that other circumstances are less favorable; that there is less ventilation, fewer comforts, more dirt, inferior nursing, or a greater lack of medical skill; for the very reverse of all this is generally true.

Every physician who does a general practice in a city, knows that a large per cent. of his patients are among the poor and degraded, where there is neither good air, good nursing, nor good food, but plenty of filth, and very few comforts of any kind;

yet even among these he will save a greater per cent. of his patients than is saved in the best-regulated hospitals. The reason is, that in the hospital there are many fever patients, every one of whom is continually throwing off a peculiar malaria to poison the atmosphere, and depress the nervous power, so as to cripple the recuperative energies of the system, making it more difficult to struggle with the particular disease under which it is laboring. It is therefore evident that the remote cause of fever does continue to act injuriously after the fever is set up; and it is therefore important to the safe recovery of the patient, and as a means of lessening the chances of those who are with him of taking the disease, that the remote cause be destroyed, if indeed it can be. And from a careful observation of passing events since I introduced my present plan of treating fevers, I have perceived sufficient evidence to convince me that this plan destroys or neutralizes the remote cause of fevers, as the very first step towards its cure.

My attention was first directed to this subject while engaged in treating dysentery, some years since, on the Caney Fork of the Cumberland river. That disease was highly epidemic at the time within certain bounds, not spreading all over the country uniformly and at once, but commencing at certain points, and extending in certain directions. The epidemic influence was so intense that, as the cause seemed to extend, nearly every inhabitant was brought under its control, and one after another of

a family were stricken down, until nearly every one had an attack. But I discovered that where I was called to take charge of the new cases in a family, and put them upon the plan of treatment which I believed was calculated to not only control the disease, but also destroy the remote cause, it rarely happened that any other new case occurred in the family, and no new attack took place in any case in which I was called to treat the *first* patient that sickened with the disease. From these facts I drew the inference, that the general epidemic poison was only sufficient to impress the system so as to strongly predispose it to take on this particular form of disease; so that some powerful exciting cause being brought to bear upon a certain member of the family, or that member, from constitutional or temporary weakness, being more easily impressed by the remote cause or ordinary exciting causes than the rest, the disease became developed, producing the first attack in the family; and that the poison developed in the progress of this case, if suffered to run its course, being added to the general epidemic malaria, now became sufficiently potent to fully impress other systems possessing more vital resistance, so that ordinary exciting causes would develop the disease, and thus one member of the family after another be stricken down; but if the last case which had occurred, and, especially, if the first case which was developed in a family, was taken in the early stage, before the peculiar poison which it is the nature of the disease to generate had been formed, and

treated in such a manner as to destroy the remote cause, and break up the disease before this poison was elaborated, then the other members of the family would, at least, be as little subject to the disease as though no case had occurred; and from the uniform exemption which I observed to attend this mode of treating the disease, we are warranted in the conclusion, that they would be much less liable to an attack than the members of other families, in which no case had been developed.

This can be accounted for by the natural supposition, that the same medicine which proved effectual in destroying the remote cause within the system of the patient attacked, might, in consequence of its volatile nature, causing it to be diffused throughout the whole of the apartment, also prove sufficiently powerful to destroy the general epidemic poison within the enclosure, and thereby increase the chances of immunity of the inmates, beyond others not so protected. Since that time, I have seen the use of the same means followed by the same results with regard to typhoid fever. I recollect of no case in which a second attack has happened in the same family under my management. In a conversation held with a medical student a short time since upon the subject, he observed that if what I said was true, it would curtail the whole amount of medical practice materially; that, by shortening the usual period of treating fevers more than one-half, and then preventing, in a good measure, some of the most

troublesome varieties from being formed, it would diminish the fever practice to a ruinous extent to the physician. Well, let it be so. The people, I suspect, would hardly be willing to suffer sickness merely to accommodate physicians, and then it will work no real loss to the practitioner himself, even to lose his calling entirely; for I hold it to be certain, that any man who has intelligence, judgment, and industry sufficient to constitute him a good physician, can make more *money* at almost any thing else to which he may choose to devote his energies. I have known many physicians who, after scuffling for years with poverty in the profession, at length turn their attention to something else, and make a rise in the world in a short time. But whether it increase or diminish the profits of the profession, will never operate as a reason for its rejection or adoption—this will be settled by the evidence of its *truth;* for to the honor of the profession be it said, it has never been backward in adopting every means which gave good promise of curtailing human misery. But I have somewhat wandered from the subject. I think I have given good reasons for believing that the continued action of the remote cause of fever does render that disease difficult to cure, and more liable to relapse. These reasons, in brief, are, that malarious fevers do not yield as readily to remedies while the patient remains in the infected district, as when removed to a locality where the poison does not exist, and that relapses are much more common

under the former than under the latter circumstances; that in hospital practice, where the apparent circumstances are much better than in pauper practice in a city, the mortality is generally larger, showing that the malaria generated by fever patients operates injuriously to the recovery of others, when a number are confined within the same walls. I will add, that even other than fever patients feel the influence of this malaria, when confined in the same atmosphere with a number of fever patients—ulcers are difficult to heal, bones unite tardily, the effects of capital surgical operations are more fatal, etc., etc.

And, further, that aborting febrile diseases, or breaking them up in their earlier stages, lessens the liability of those around them to take the disease.

HOW DOES QUININE CURE INTERMITTENTS?

It does it, either by destroying the malaria or by neutralizing its power, or by removing the condition of the system produced by the action of the malaria, from which the fever arises. That it does not act in either of the first ways mentioned is proven by the fact that it does not always succeed. For it is evident that if it is capable of destroying the malaria, or so changing its nature as to deprive it of that quality which impresses the system injuriously, it must do it always. Whether malaria be a poisonous gas or organized existence—whether vegetable, animal, or chemical—this would be the

case; any substance which unites with another chemically so as to neutralize it or change its nature, will always act thus; and any thing which will destroy the life of any particular species of animal or vegetable existence will do it every time, if given in sufficient quantity or applied in sufficient force. It follows, that if quinine destroyed the cause of fever, or was an antidote to the effects of that poison, it would never fail in its operation. But it does fail occasionally, under the best management; and even when it succeeds in breaking up the disease, it often returns again, and after the quinine is repeated a few times, it generally wholly loses its power; this would not be the case if it acted as a neutralizing chemical agent, or if it acted as a poison to the organic existences which compose malaria. It therefore does not act by destroying the malaria, or by neutralizing it. It remains, then, that it does act by removing the condition of the system produced by the malaria, out of which the fever arises, or in which it consists, for there is no other conceivable mode by which it can act. But the evidence of its acting in this way does not rest merely upon the fact that we cannot explain its *modus operandi* in any other way, but is based upon its perceptible effects upon the system, and the analogy of these effects with those produced by other remedies, the mode of action of which is well understood. Now what are the sensible effects of quinine? I once took five grains of this medicine, when in good health, on purpose

to ascertain, if possible, its mode of action on the system, and I found its sensible effect to be that of a pure nervous stimulant. It produced that state of the system, in a high degree, which we call *nervousness*, a sense of agitation, slight quiverings of the muscles, some intolerance of light, increased sensibility to sounds; and even imaginary sounds were heard, as of distant ringing of bells. Besides these, there was wakefulness, and a very uncomfortable restlessness; and these sensations remained in force for over twenty-four hours. The reason such a moderate dose produced so much effect was, that there was no nervous depression in my system to overcome, as there is in fever; on the contrary, I am naturally of a nervous temperament, and easily impressed by all nervous stimulants. I have not given this description of the sensations produced by quinine in my own person because they were peculiar, but because they are exactly what are described as arising from the influence of the medicine by others, who have taken it to break up a chill; showing that its effects upon the healthy orgasm are the very same which it produces in disease; and it may be noted, that if it fails to produce these, its natural effects, in some considerable degree, it also fails in effecting the object for which it is administered. But quinine not only acts like other nervous stimulants, but other nervous stimulants act like it, that is, they accomplish the same object. It is well known that opium will prevent a chill, with almost as much certainty as quinine;

so will ardent spirits, so will the *cold douche*, so will a powerful mental excitement. These, it is true, are not so permanent in their effects as quinine—the reason for which will be given presently—but, as far as their sensible effects are concerned, they act very similarly: all produce an agitation of the nervous system, muscular tremors, wakefulness, giddiness of the head, and an exaltation of the senses. I mean they do these when applied in a certain force—all may operate with such overwhelming power as to suspend sensation—but all when operating with moderate power, like quinine, increase nervous power, and exalt sensibility. In what, then, consists the superior power of quinine over other nervous stimulants, in breaking up intermittent fever? It is because it is not only a powerful nervous stimulant, but it is a permanent one, and nothing else. It is not evanescent in its effects, like the *cold douche* and mental excitement, nor does it possess, like opium and other medicines of that class, sedative or depressing properties, by which, after exciting, they depress nervous power, thereby increasing the nervous debility, which causes a chill: quinine, by bringing about a high state of nervous excitement, and continuing it for a considerable time, in consequence of the increased amount of nervous power which is sent out to the whole system, often causes such a perfect reäction in the capillary vessels, that they do not relapse again into torpor, and consequently a permanent cure of the fever is effected. But sometimes the excitement

produced by quinine amounts to actual nervous disturbance, succeeded by depression, in which case the capillary debility is increased, and then its use proves an entire failure. Taking this view of the subject, I am in the habit of giving but a moderate dose of quinine, just enough to impress the nervous system, say from four to six grains, and at the same time give my *fever syrup*, to keep down nervous disturbance, and to increase capillary action. In this way, I have generally succeeded in breaking up intermittent fever, with a single moderate dose of quinine; for, if the syrup is continued for several days after the chill is broken, there is little danger of its returning. This mode of treating intermittents has several advantages: it is more certain, it produces no unpleasant effects, and it is much cheaper.

MODUS OPERANDI OF TOPICAL REMEDIES.

Fordyce, (p. 304,) in speaking of the known benefits which often arise from cupping or leeching the temples, when a patient is suffering in the head, or in delirium during fever, observes, that it cannot be accounted for by the mere amount of blood drawn from the general circulation, or from any direct relief to the vessels immediately congested, as they are situated within the brain, and there is no very intimate connection between the internal and external circulation of the head; hence he adds: "The reason why such topical evacuations by bleeding carry off or diminish the delirium or pain in the

head, or even sometimes the whole fever, is consequently wholly unknown to the author." The same difficulty will present itself in accounting for the beneficial effects of other topical means—such as dry-cupping, sinapisms, stimulating liniments, blisters, etc. These often prove as beneficial as drawing a little blood by cups or leeches, and they undoubtedly operate upon the same principle—all act by producing a stimulant effect through the medium of nervous connection or association. Distant parts of the body are known to be closely associated by nervous influence—the *uterus* and the *mammæ*, the *parotid gland* and the *testes*, mucous surfaces and their external opening; and all internal parts with the external surface immediately opposite. Some of these associations, among which is the last, are not owing to *direct* nervous communication, but are dependent upon that connection which exists between the nerves of the associated parts in the brain, spinal marrow, or some other centre of nervous influence, as the great plexus and ganglia. Why there should be a close nervous association between the skin and the deep-seated parts immediately under it, we cannot tell, except that it was so arranged by our beneficent Creator for the express purpose of enabling us to produce remedial effects upon parts beyond our reach, by applications addressed to those parts within our reach. This appears to be a sufficient reason for the association, and we can perceive no other, as these parts are not at all concerned with each other

in performing their ordinary functions. The mode of operation of topical remedies appears, then, to be as follows: a blister, for example, is applied over the seat of an internal inflammation; it acts as a powerful stimulus to the cutaneous capillaries, and the impression, being conveyed by the nerves of organic life, with which the part is supplied, to the nervous centre from which they take their rise, is sent by reflex action along the same kind of nerves to the deep-seated part, and becomes a stimulus to its capillaries and arouses them to healthy action. I say nerves of *organic life* in the above connection, to distinguish them from nerves of sensation, for these appear to have little or nothing to do with the beneficial operation of a blister in relieving deep-seated inflammation or congestion. This is proven, first, by the fact that blisters often draw well without producing any sensation at all, and yet produce their full remedial effect; and, secondly, blisters often occasion intense sensation while producing but very slight capillary excitement in the part, and without any benefit to the internal inflammation or congestion. We are all familiar with examples of this kind in conditions of great capillary debility, connected with a high state of nervous sensibility, as in cholera, congestive chills, etc. My observations have led me to the conclusion, that the benefit derived from a blister, or any other stimulating topical remedy, is always in an inverse ratio to the suffering it occasions the patient; and acting upon this idea, I am in the habit, when I think it best to

employ them in a case in which the sensibility of the nerves of sensation is exalted, of subduing it as much as possible by opiates or anodynes; for it appears that the same kind of impression which is transmitted to the nervous centre, is sent, by reflex action, to the associating organ or part. Hence, if it be increased *sensation* which predominates, the only effect of the local application is to increase the *sensibility* of the associating part, and, consequently, augment the *suffering;* but if the matter is so managed that but little impression is made by the topical stimulant upon the nerves of sensation, and capillary excitement greatly preponderates, then the effect upon the internal inflammation is most strikingly beneficial; the capillaries of the part being roused to action, relieving the engorgement which occasioned the nervous suffering and other mischiefs referable to capillary distension, without any expense of suffering to the patient, or any danger of increasing the general morbid action by getting up an extensive nervous disturbance.

It was said above, that the same impression which is made upon any one part by a topical application, will by reflex action be also produced in the associating part. This will at once explain the operation of every kind of remedy of that class. It is in this way that a soothing poultice to the abdomen relieves pain or spasm in the bowels; in this way chloroform liniment, applied to the surface of limbs tortured with the aches of the first stage of fever, will allay the distress in the bones, sinews, and

muscles, as by enchantment; it is in this way that blisters or strong rubefacients, applied to the *breasts* of a female suffering from suppressed menstruation, will excite the capillaries of the *uterus* and cause it to perform its function; and in this way an anodyne poultice, applied to the same parts, will transmit its soothing effects to the womb, and quiet the pains attendant on difficult menstruation.

HOW DO SPIRITS OF TURPENTINE ACT BENEFICIALLY IN TYPHOID FEVER?

So many intelligent physicians have given their united testimony in favor of the use of turpentine in typhoid fever, that we are compelled to believe that it does possess some useful properties, suitable to control this particular form of diseased action, or at least some of its stages.

But writers differ widely as to the time of administering it, and the amount prescribed; but most of them agree in giving it in the second stage of the disease, when there is considerable redness of the tongue, irritability of the bowels, and tenderness of the abdomen, in small doses; and more freely in the third stage, when there is low delirium, subsultus tendinum, etc. My views of the nature of the *febrile movement* in general, and the condition of the nervous system in particular, in this disease, as well as my experience, would lead me to prescribe this remedy, under certain restrictions, in all its stages. Before adopting my present mode of practice, I relied more upon this than on any other remedy, and

yet occasionally prescribe it, especially when called to a case which has already run into the second stage; it therefore is pertinent that I inform the reader with what view I administer it, and what advantage I expect to draw from it; for I protest against giving any thing in disease for which I cannot assign a good reason.

In another part of this work, I have said that turpentine enters the circulation and acts as a direct stimulant to the capillaries in general, and to those of the kidneys in particular; but it does more than this; it is a powerful stimulus to the nervous centres, and greatly increases general innervation. In typhoid fever, nervous power is deficient, as well as capillary action too low; therefore we can see at once that turpentine may be used in it with great advantage. But it does still more: in this fever, there is always more or less intestinal irritation, and often inflammation; now we know, from microscopic observation, that the capillaries are torpid in these states of disease, and observation has proven that turpentine, as a local application, is an excellent excitant to bring up the action of the capillaries to the healthy standard; hence it has become a popular application to burns and sores. Turpentine, then, acts beneficially in typhoid fever in the following ways:

First. It tends to relieve the local irritation or inflammation in the alimentary canal. But for this purpose it should not be given undiluted, or even in water, as by not mixing with the water it will still

strike the walls of the stomach in its full strength, and injure its coats; it should always, therefore, be combined with syrup, or mucilage, or olive oil, so as to cover its acrimony.

The second indication for which it may be given, is to excite general capillary action. That it has this effect, in a very considerable degree, is evident from the improved appearance of the surface which often follows its exhibition in very low conditions of the system, and the general restoration of the secretory action which we also often see follow its administration, known by the tongue and mouth losing their dryness, the skin becoming more pliant, and by a more abundant flow of urine. For this purpose, it should be given in small doses, largely diluted with some bland material, which will favor its absorption; for if given in too large doses, and especially if undiluted, it will irritate the absorbent vessels so as to cause their mouths to contract and prevent absorption, or, if it enters the circulation in too much force, it may over-stimulate the kidneys, it being specifically directed to these organs. The indiscriminate and almost reckless use that has of late years been made of this remedy in typhoid fever and dysentery, has led to many cases of incurable debility in the kidneys, occasioned by over-stimulation; so that we should recollect, that while we are curing a man of an acute disease, we will not lay him under much obligation to us if we entail on him some irremediable injury, and especially when all the advantages of the remedy can be ob-

tained without any of its disadvantages. But turpentine, in the third place, acts upon the nervous system. That it does, is evident from its beneficial effects in low stages of fever, in quieting nervous restlessness, subduing subsultus tendinum, and relieving low, muttering delirium. That it is especially a *nerve stimulant* and not a *sedative*, is proven by its increasing nervous disturbance, headache, and delirium, when these arise from over-action or excitement. We, therefore, should be guarded in its administration; withholding it altogether when there is evidence of over-action in the nervous centres; and even in cases of under-action we should be careful not to impress the nervous centres too powerfully, or we may exhaust the vital impressibility and defeat our purpose, or we may produce so much nervous reäction as to cause nervous disturbance, which some, not being able to detect, have considered a good reason for pushing the remedy still further, until, if the patients survived the shock, they were permanently injured by having partial paralysis or other forms of nervous lesion entailed on them for life. I have thought it necessary to say this much upon this subject, to guard the younger members of the profession from running into an abuse of this excellent remedy, and thereby bringing it into odium, as some other valuable articles of the Materia Medica have been.

WHY DO MERCURIALS ACT INJURIOUSLY IN TYPHOID FEVER?

I answer, because they have a tendency to produce the *typhoid condition.*

Now in what does the typhoid condition consist? That is, what distinguishes typhoid from other fevers? It most assuredly is the want of nervous power, manifested in many ways not necessary here to enumerate, but in no way more strikingly than by a deficiency of fibrin in the blood. This material seems to be the most difficult to elaborate of any of the constituents of the system—the very highest effort of vital action is required to produce it; hence, in all cases in which nervous power is low, fibrin is found to be deficient in the blood; and in all cases in which fibrin is deficient, the repairing or healing process does not take place. Whether this is for want of fibrin, or from the cause which produces a want of fibrin, viz., a low state of vital action, is not known. From this cause, blisters drawn in an advanced stage of typhoid fever often refuse to heal; and from this cause ulcerations so often prove troublesome in the intestinal coats in typhoid fever. Dr. T. L. Maddin, of this city, read a paper before the State Medical Society last spring, (1856,) which was published in the May number of the Nashville Journal of Medicine and Surgery, in which he gave, as I believe, the true pathology of these ulcerations, viz.: that Peyers and the solitary glands of the in-

testines have a peculiar construction, being nothing more than minute vesicles formed under the epithelium of the bowels, having no orifices, and bursting when they become distended, and so causing a minute lesion of continuity in the epithelium; repaired, it is true, with very slight effort in health, but which, in the typhoid condition, when nervous power is low, and the reparative effort at zero, serves as a beginning-point for the destructive process of ulceration. And as these glands or vesicles, when they discharge their contents, become obliterated in the natural condition of the system, and others form in their place, to become full and burst in their turn, keeping up a regular succession, so in typhoid fever it is found that a like succession of ulcers is formed—*post mortems* having revealed the fact that ulcers in every stage of development were present from the size of a mustard-seed to that of a hen's egg, and fully perforating the coats of the bowel, the reparative process being entirely suspended. Now, what are the facts with regard to the influence of the constitutional effects of mercury? Does not observation prove that it does the same thing? The best writers acknowledge that it lessens nervous power; it also occasions a loss of fibrin in the blood, and cripples the recuperative powers so that the reparative process is suspended, and ulcers will not heal. It is true that, when the inflammatory process is set up in the salivary glands, an amount of excitement is conveyed to the nervous centres sufficient to *whip up* a forced state of nervous power ade-

quate to manufacture fibrin, but not out of the *raw material;* not from nutriment taken in, but by breaking down the tissues of the body and appropriating the fibrin which entered into them, throwing off the albumen and salts at the mouth. And sapient physicians have resorted to this *destructive* process to force a little fibrin into the circulation, but oftener killed their patients in trying. How much more philosophical it is to go to work with such means as will increase the nervous power and restore the capillary action, so that fibrin may be manufactured from appropriate food!

The inquiry will naturally arise, Why do so many really intelligent physicians persist in giving mercurials in typhoid fever? One reason is, that they were *taught* to give it—all the fathers gave it: Cullen, Rush, Chapman, Cooke, and all the lesser lights, relied on it as *the* remedy in fevers in general. Another reason is, that in most fevers it actually does do good under proper management, and physicians are slow to believe that a remedy which acts beneficially in other fevers will act the reverse in this.

Mercurials possess one property of action which, so far as it goes, renders them exceedingly appropriate in fevers: they do increase capillary action; given in the proper manner, no remedies can act finer in this respect; hence they excite all the secretions, and those who look no farther for the source of mischief in disease than the most obvious effects, naturally enough suppose that the suspension of secretions is the *disease*, and to restore *them* is to cure

it. But if they would recollect that it is the want of nervous power that has occasioned capillary debility, and this in turn the diminished secretion, they would never resort to a nervous debilitant to remove the difficulty, but would rather call into requisition such generators of nervous power as turpentine, camphor, ammonia, etc., but especially sassafras, piperin, and valerian. A distinguished physician who witnessed the happy manner in which these articles control typhoid fever, (having watched the progress of a case under my treatment,) inquired of me if it was not probable that the sassafras operated in the same way as turpentine, (he having great confidence in turpentine.) This conjecture was undoubtedly well founded: turpentine is a nervous stimulant, and so is sassafras; both increase nervous power, and in this way both are valuable remedies in typhoid fever; but the sassafras is preferable, because it is the best nervous stimulant, and because it never acts as a nervous irritant, as turpentine sometimes does, and never acts injuriously upon any of the organs, as turpentine occasionally does upon the kidneys; and then sassafras possesses the rare property of destroying or counteracting the effects of narcotics, and in this way prevents any further action of the remote cause, so that between it and turpentine and mercury the case stands this way:

Mercury increases capillary action and excites the secretions, but depresses nervous power, which is already deficient, and in this way increases the

deficiency of fibrin, and destroys the recuperative powers so that the reparative process cannot be set up, and the patient wears out, or destructive absorption perforates his bowels, and he dies suddenly from the contents entering the cavity of the abdomen.

Turpentine acts also as a capillary stimulant, and, in place of depressing, elevates nervous power, and thereby favors the production of fibrin, and aids the restorative efforts of the system, so that these lesions in the intestinal epithelium are healed, and the process of destructive absorption stayed. It however is liable to over-stimulate the nervous centres, get up nervous disturbance, produce over-action in the kidneys, and entail debility on those organs.

Sassafras does whatever the turpentine can do that is beneficial, and never does harm, so that it is to be preferred of the two, and, with the aid of piperin and valerian, will do pretty much all that is required to be done in typhoid or any other fever.

CHAPTER VIII.

PARTICULAR FEVERS.

PRELIMINARY OBSERVATIONS.

EVERY writer upon the subject of fevers has been more or less perplexed with regard to *diagnosis* and *nosology*. That there are different varieties of fever, and that each of these varieties has some diagnostic symptoms by which it can generally be distinguished from every other, is acknowledged by all; but these varieties present so many symptoms in common as to render it no easy matter to either name them correctly, or point out peculiarities by which each can be readily recognized, especially in the early stage, and by the young practitioner.

The reader who has followed me thus far will have discovered that all fevers consist mainly in the same departures from healthy action, and that they consequently can be successfully managed by the same means, which renders it of small moment

whether each kind of fever be known from every other kind or not. And were I writing alone for experienced practitioners, what has been said would be quite sufficient; for their knowledge of the laws of diseased action, and their acquaintance with remedial agents, would enable them to make such changes in the treatment, and call into use such collateral means, as would be found necessary to adapt my general plan of treatment to each variety of fever, and to each case. But, as I am not writing exclusively for these, but also for the *neophyte* in medicine, it becomes necessary that I should enter into particulars, and as far as possible point out the differences that exist between the varieties of fever, and especially the variations in treatment most appropriate to each.

Fevers have generally received names by different authors significant of what they suppose to be the nature of each variety, or the particular organ or structure apparently most involved; or of what they suppose to be the remote cause, or of the most prominent symptom presented in the course of the disease. The nosology of fever, founded upon the structure most essentially involved, has some advantages, as it leads to practical results; and the least objectionable classification upon this plan is by our talented countryman, Prof. H. F. Campbell, of Georgia. The reader may find his views somewhat in extenso in the published "Transactions of the American Medical Association, vol. ii., 1858. (Article:) Report on the Nervous System in Febrile

Diseases, and the Classification of Fevers by the Nervous System. By Henry Frazer Campbell, A.M., M.D., Professor of Anatomy in the Medical College of Georgia."

Professor Campbell, I think, very correctly classes fevers with the *neuroses*, and divides them as he thinks they have their origin in disturbance of the cerebro-spinal or the ganglionic system — terms answering to nerves of animal and nerves of organic life of Bichat. That the remote cause of some fevers does chiefly impress the cerebro-spinal system, and that the remote cause of other fevers spends its force mostly upon the ganglionic system, will be evident to every observer who has ever had his attention turned to the subject. For example, the most prominent symptoms of miasmatic fevers are those of disturbance of the nerves of animal life, while those of typhoid fever as plainly arise from disturbance in the nerves of organic life; but other forms of fever appear to about equally involve both systems. The eruptive fevers, for instance, which Campbell classes with the ganglionic neuroses, are attended with nearly the same amount of disturbance in the cerebro-spinal nerves, except showing a less marked periodicity, as the miasmatic fevers; and pneumonia, which he classes with the cerebro-spinal neuroses, sometimes is attended with so little manifestation of disturbance in the nerves belonging to that system, that neither the patient nor his friends become aware of any thing serious in the attack, until such *organic* changes have taken place as to

place the case beyond the hope of recovery. In fact, the *fever* attending pneumonia is often purely *typhoid*, while it is in other cases as plainly *bilious*, and not unfrequently decidedly *inflammatory*. The same observation may as truly be made with regard to dysentery; so that all that can be established upon this subject is, that some fevers mainly manifest themselves in disturbance of the nerves of animal life, while others principally show disturbance of the nerves of organic life, and others involve both systems about equally, or sometimes one principally and sometimes the other; so that we find Professor Campbell's system of nosology, though perhaps the best we have seen, yet, like all its predecessors, dividing things which should stand together, and grouping others that should stand apart.

But Professor Campbell has done much for the science of medicine. His treatise on the nerves should not only be read but studied by every physician, as it throws a flood of light upon many of the phenomena of diseased action, which have heretofore been perplexingly obscure to the practitioner. Though as a discoverer he cannot rank higher than Marshall Hall, yet his discoveries will lead to more important practical results, and his name will ever justly be associated with those who, in every age, have gone in advance of their generation in elucidating the great truths of medical science. Some persons are disposed to undervalue the labors of both Hall and Campbell, because what they have demonstrated to be true, had before been guessed

at, and more or less firmly believed, by others before they wrote; but all must agree that those great men have made plain what was before obscure, and proven that which was only presumed, and systematized those things which before appeared to be disconnected and without relationship.

Those who have read the preceding pages of this work, will have discovered that I distinctly claimed years ago that the remote cause of fever makes its first impression upon the nervous centres; but I did not claim this as original; but I do claim that I stated it more distinctly, and dwelt upon it as of more prominent importance, than any writer who preceded me had done. Neither does Prof. Campbell claim originality for the idea that the cause of fever first impresses the nervous system, but claims to point out the points of attack of different causes, and how the various parts of the great nervous system are associated together, so as to sympathize with each other, and give rise to the almost infinite variety of morbid manifestations with which we meet in febrile diseases. Now, by the way, I claim for the *capillary system* a degree of what I accord to Professor Campbell with regard to the *nervous system*, viz.: that I have drawn attention to it more pointedly than had before been done; and pointed out the essential part it plays in the development of every case of fever; and further, that I have made this knowledge of the nervous and capillary systems available not only for a clearer elucidation of the phenomena of fever, but also in devising

a better, because a simpler and more successful, treatment.

For the advantage of the uninitiated, perhaps it would be well to give a condensed statement of what is claimed as new with respect to a knowledge of the functions of the nervous system.

We had been taught long since, by Bichat and others, that the human system is supplied by two distinct classes of nerves: one class, having their origin in the brain and spinal marrow, puts us in connection with the external world, by giving us general sensibility, as well as supplying us with special senses—as sight, hearing, etc.; also giving voluntary muscular motion, by which the senses can be gratified and other wants of the system supplied: these are the *nerves of animal life* of Bichat—*cerebro-spinal nerves* of late writers. The other system of nerves have their origin in the ganglia or *little brains* situated within the body, and connected together by nervous matter so as to form a continuous chain in front of the spinal column, known as the great sympathetic nerve, and supply nervous power by which all those silent and delicate operations are performed by which life is preserved—as digestion, assimilation, circulation, etc., etc.; hence known as *nerves of organic life*, or ganglionic nerves. Now it had been long known that each of these great nervous classes contained lesser divisions—the cerebro-spinal nerves being naturally divided into nerves of motion, nerves of sensation, and nerves of special senses—and that the ganglionic

nerves divide themselves into nerves of nutrition, nerves of circulation, etc., and that each of these lesser divisions is susceptible of further subdivision; for example, the nerves of secretion contain as many varieties as there are different secretions, those of assimilation as many as there are constituents of the human body, etc. It was also known that besides all these there is another set of nerves whose office it is to connect the different classes, divisions, and varieties of nerves together, and hence called nerves of association; and it was believed that it is by these inosculating or connecting nerves that the various sympathies and nice relations that exist between distant parts of the system are maintained; and late investigations have tended to establish this belief as ascertained truth.

It has also been well understood that each nerve possesses fibrils whose office is different; that some convey impressions *to* the nervous centres, while others transmit power or influence *from* them to the part in which the nerve is distributed. Now what gave Marshall Hall so much celebrity is principally this, that he demonstrated the existence of a class of nerves whose office it is to convey impressions to the nervous centres where motive-power is manufactured, and cause influence to be sent out through the motor-nerves to the muscles to excite them to action. Example: a certain impression made upon the lining membrane of the nose is taken up by these nerves, and conveyed to the origin of the nerves which are distributed to the muscles con-

nected with respiration, which causes motive-power to be sent out which calls into action those muscles by which the complicated act of sneezing is performed; or a morbid impression is made upon the sentient lining of the stomach by some offending matter: intelligence is sent along these nerves to the spinal origin of the various muscles called into action in the effort of vomiting, and immediately motor-power is sent out, and those muscles obey the impulse; or an intense light impresses the sentient expansion of the optic nerve unpleasantly, and immediately intelligence is sent along those nerves to the origin of the muscle which supplies motor-power to the iris, and instantly that delicate curtain is drawn so as to admit less light, and so on throughout the countless examples where muscular motion is wanted irrespective of the will, often taking place in opposition to it, and still oftener unknown and unmarked by consciousness. The nerves performing these offices are the EXCITO-MOTORY NERVES of Marshall Hall. Since the excito-motory system was made known by Hall, our own countryman has added an equally brilliant chapter to our knowledge of nervous action.

Professor Campbell has clearly proven that a system of nerves exists which bears the same relation to *secretion* that the excito-motory system does to *muscular action*, and by him appropriately named the EXCITO-SECRETORY SYSTEM. A few examples will illustrate the working of this system of nerves. For instance, the odor of savory food

strikes the sense of smell of a hungry man, and the impression is instantly conveyed to the origin of the nerves concerned in the manufacture of saliva; the necessary nervous power is immediately dispatched to the salivary glands, and, in less time than I can tell it, the fellow's "mouth is watering." Then again, if the man obtains his desire, the moment the food pleasurably impresses his gustatory nerves, an impression is sent in a like manner to the centre from which the *gastric* nerves have their origin, which gives information that the *liquor gastricos* will shortly be needed, to act upon the material which is being masticated. And thus we might follow the morsel from the time that its pleasant odor first attracted the alimentive desire, throughout the entire history of its complete digestion, to the absorption and assimilation of its nutrient particles, and the ejection of its grosser constituents, and show that, in its progress, the excito-secretory nerves are faithful to their office, having a supply of the necessary secretions ready to meet it in its various stages. In the meantime the excito-motory nerves have not been idle, but have called for motive-power to move the material on, and at the exact time that a change of place was needed. In fact, a more beautiful and forcible illustration could not be given of the workings of both the excito-motory and excito-secretory nerves, than might be found in tracing digestion through its various stages.

Now, a knowledge of the nervous system, with

the lights which have been recently thrown upon it, becomes not only exceedingly interesting in the study of physiological phenomena, but of the highest importance to a correct understanding of those pathological developments constituting the symptoms of disease, and also of the *modus operandi* of the means we prescribe in its management.

For example, when the physician is called to examine a case of fever, and he finds his patient, besides presenting other evidences of a febrile attack, complaining of distressing pains or aching through the whole frame, with more or less soreness of the surface, he may know that the spinal origin of the nerves of general sensation is the seat of morbid excitement; which will at once draw his attention to the importance of making soothing or anæsthetic applications to the spinal column. If there be cramps or spasms, or other morbid muscular action, he knows that irritation exists in the origin of the motor-nerves, and he resorts to the above means, with the addition of antispasmodics. But he will further wish to ascertain whether this irritation be the result of primary disturbance in the cerebro-spinal centres, or whether the principal difficulty exists in some other part, as irritating ingesta, or worms, in the alimentary canal, which, through the medium of the excito-motory nerves, have gotten up the morbid phenomena; if so, he will, besides using the means suggested in order to obtain present relief, also direct his efforts to removing the principal cause. But suppose the prin-

cipal feature in the attack is a wasting diarrhœa; the practitioner will determine that, as all the secretions are performed by ganglionic influence, there is therefore morbid excitement in that system, and especially in the particular ganglia from which the nerves of the small intestines arise; and he will now consider whether this irritation be primary or secondary. If the patient be a child, and, on examination, he finds that a protruding tooth has produced considerable irritation in the gums or investing membrane, he will have reason to suppose that this has acted upon the ganglia involved, through the medium of the excito-secretory nerves, and will expect advantage from letting the tooth free by an incision; or, if the patient be an adult of delicate constitution, and living much within-doors, but has lately been exposed to wet or cold, or damp air, he may presume that the morbid impression made upon the surface has been conveyed by the excito-secretory nerves to the ganglia, as in the child, and a hot mustard bath, or some kindred appliance, will often arrest the diarrhœa without other medication.

But to point out all the instances in which a knowledge of the *excito-motory* and *excito-secretory* nerves can be of service in diagnosis and treatment, would fill a volume; what has been said must suffice as hints, which may serve to set the practitioner to thinking. I cannot leave the subject without suggesting that the labors of Hall and Campbell form but the first chapters in the exposi-

tion of a *general excitor nervous system;* that besides the excito-motory and excito-secretory, there will yet be added excito-circulatory, excito-nutrient, excito-thermal, and excito-sensatory systems of nerves, and perhaps others. I will very briefly sum up some of the evidence which presents itself of the existence of the last four named; and, first, excito-circulatory system. Examples of the physiological workings of this system may be found in all those sudden and partial excitations of the capillaries following impressions made upon some distant part or organ, as in blushing, engorgement of the erectile tissues, etc. These excito-circulatory nerves evidently also coöperate with the excito-secretory in causing a flow of blood to a gland during the period of active secretion. Pathological examples may be found in the flow of blood to the head from disturbance of the stomach, the hectic flush from pulmonary irritation, etc.

Of the existence of a system of excito-nutrient nerves we may find evidence enough. What else produces those physiological developments which take place at puberty and during pregnancy? Instances of the morbid action of these nerves may be seen in induration and scirrhus of the mammæ and uterus in the female, and enlargement of the prostrate in the male. Of the workings of the excito-thermal system we may find examples in all those cases of morbid sensation of heat unaccompanied with inflammatory action or even congestion, with which we so often meet in disease, and often

existing without any other manifestations of departure from healthy action. Of the existence of an excito-sensatory system of nerves, we also have abundant evidence: every ache or pain, or other morbid feeling, which we find located in a part not itself the seat of disease, is an example of its workings; as pain in the head from gastric or uterine irritation, pain in the extremity of the urethra from irritation in the kidneys, or colic from mental disturbance, etc.

Now I am not certain whether the above suggestions contain any thing new or not. I have not met with them before; but if new, I claim very little credit for what I have said, it being naturally suggested by the researches of those who have preceded me. Columbus was entitled to undying fame for his discovery of a New World; Americus was also entitled to much credit for pushing discovery farther; but after them, every navigator could make fresh explorations. These observations upon the nervous system may be a little out of place in a work of this kind, but I hope the reader will find them sufficiently instructing and suggestive to compensate him for the perusal.

Without further preliminaries, I will now take up the individual fevers, and point out the diagnostic symptoms of each, and the variations of treatment which I have found necessary in adapting my general plan of treatment to each variety and stage, and the various minor helps which serve to meet complications, remove annoyances, and increase

the efficacy of the principal remedies. And, as being the most important on account of its ranging over a wider territory, and being more intractable to the influence of the common remedies, I will first take up

TYPHOID FEVER.

The forming stage of this fever is generally more protracted than any other. For many days, or even weeks, the patient is sensible of not being quite as well as usual; has less energy; less vivacity; a more capricious appetite; has some irregularity of the bowels; sleep more disturbed, and not so refreshing as usual; finally, pains strike him of a peculiar kind, often assuming the form of crick in the neck, or aching of the head, and soreness about its junction with the neck. His feeling of discomfort gradually becomes more decided; and, although he is still often unable to refer his illness to any particular point, yet he is conscious of being very sick, but perhaps still mopes about for a few days longer, expecting every day to get better. Friends frequently look upon it as an attack of common cold, and administer sweating remedies to throw it off; but although he perspires copiously, he is not at all relieved, and they next usually conclude to give a purgative to work the cold off by the bowels. This also operates with unusual power, and the bowels continue to run off with watery discharges long after the medicine has spent its force. By this time the patient is decidedly worse, and his friends, not know-

ing what more to do, send for a physician. When the doctor arrives, he will generally find his patient, if a negro, closely covered up in bed, even should the weather be warm, and either asleep or in a stupid condition, which might be easily mistaken for sullenness. Arouse him and ask him what is the matter, and he will probably say, *Nothing:* ask him why he is in bed then? and he will say he feels too weak to work; if he complains, and you inquire where his misery is, he will answer, All over. If a white man, he will probably be found sitting with the family, and will apologize for sending for you upon so trivial an occasion; observes that he has felt "out of sorts" for several days, and thought best to send for you to tell him what is the matter. Upon examination you will find his pulse a little more frequent than natural, and the surface, especially of the body, a trifle too hot, and the extremities rather too cool; his countenance looks tired and expressionless; tongue furred, and unusually white, as though it lacked blood, or presenting the hue of venous blood. Ask him if he has tenderness of the abdomen, and he will commonly say No; but upon making pressure just above the umbilicus, or a little to the right, you will discover that he will flinch; pressure along the spine will commonly detect several points of tenderness; his chief complaint however is of *weakness*, which he attributes to the operation of the purgative, if he has taken one.

If the disease is suffered to progress, and you examine him a few days later, you will probably find

that the giddiness of the head has been changed for severe pain, delirium, or stupor as from intoxication. There is now decided tenderness the whole length of the spine; the tenderness of the abdomen is also considerably increased, attended with more or less fulness; the heat of the trunk is augmented, and the pulse accelerated, small and wiry under the touch; tongue thickly coated in the middle, but rather clean at the edges, and of a deeper red; in putting it out it assumes a pointed form with a slight elevation of the edges, and it will often quiver like a leaf in the wind. The tongue is not however red and pointed in every case; sometimes it is unusually white, as though it lacked blood, and is spread out much broader than natural. There is usually some diarrhœa; at least, if any purgative has been given, it has run off with watery discharges. The heat, though not generally so great in this fever as in some others, is more persistent, varying but little for many days together.

You who have ever seen a case of typhoid fever, will readily recognize the likeness I have drawn. Now what kind of lesion does this group of symptoms indicate? Do they not all point unmistakably to derangement of the nervous system, forcing the conclusion that this disease has its primary foundation in the disturbance of the nervous centres? Every characteristic manifestation of this fever from first to last indicate this, viz., weariness, lassitude, cramping pains, giddiness, the wry neck, the trembling tongue, the peculiar fretful pulse, the fitful al-

ternations of heat and cold, and subsequently the flashes of fiery heat, the delirium, subsultus tendinum; and even the red tongue, the irritable bowels, and the rose-colored eruption, as I will show hereafter, all grow out of disturbance of the nervous centres, which disturbance is evidently the result of the action of the remote cause, as they precede local disturbances, and therefore cannot be *produced* by them. Hence this disease has often been known by the term *nervous fever*.

The capillary debility, and consequent engorgement, being more slowly produced in this than any other fever, the reäction is also more tardy in taking place, but, when set up, is proportionally more persistent, and the disease has therefore been called the "continued fever." The red tongue and intestinal irritation have claimed the attention of others, and they, taking these symptoms as indicating the seat of the disease to be in the stomach and bowels, have named it "enteric fever," or gastro-enteritis.

But these are by no means the first symptoms of the disease, but come on in its progress, and are therefore an effect of some other lesion: they are not even peculiar to this disease, for we find them fully as prominent in other diseases known to be the result of great nervous disturbance, such as fever from dentition, from intestinal worms, from painful or difficult menstruation, from conception, etc. Every practitioner of experience has seen the red tongue, the irritable stomach and bowels, and the great impressibility of the system to the influence

of medicine, as fully developed in these and kindred cases, known to arise from nervous disturbance, as they ever have in typhoid fever.

In the progress of this disease there often appears a peculiar eruption on the surface about the epigastrium, and also internally in the neighborhood of Peyers glands, which has induced many to class it among the eruptive diseases, such as scarlatina and measles, and that, like them, it is contagious and self-limited. But we have many other examples of peculiar eruptions originating from great nervous disturbances, such as arise from over-doses of narcotic stimulants, poisonous fish, lobsters, mushrooms, etc.; and it is not unphilosophical to suppose that the eruption attending the true specific contagious diseases is the result of a similar cause, viz., that the remote cause of these diseases, whether animalcule or gaseous, is of the nature of a narcotic stimulant or nerve-poison, and the eruption is the consequence of the nervous disturbance. It will be perceived, therefore, that I do not deny that typhoid fever is an eruptive disease, or that it is contagious; all I contend for is, that it is the result of a narcotic poison, either organic or gaseous, acting upon the nervous centres.

Very little appears to be decided in the profession with respect to this disease, either with regard to its nature or its treatment.

In order to show the unsettled condition of the medical mind upon this subject, I will give an abstract of the present mode of treating this disease,

taken from writers living in various parts of our own extensive country and in different European states, which I think will show that more light was needed upon this subject. I will first condense from the published Transactions of the American Medical Association some abstracts from reports of committees on epidemic diseases, etc.:

Dr. Barbour pursued the expectant plan; bled if reäctions ran high; gave opium to restrain diarrhœa, quiet delirium, and procure sleep; moved the bowels with castor-oil and turpentine. Mercurials proved injurious; quinine was also injurious in every case of real typhoid fever. Dr. Adair agrees with this.

Dr. Hart relied upon aperients, diuretics, diaphoretics, sedatives, and stimulants.

Dr. M'Nelly gives blue-mass, ipecac, and citrate of potass, in a state of effervescence; restrains diarrhœa with paregoric and tinc. catechu; has frequently given quinine, but without benefit.

Dr. B. F. Stephens gives chlorine-water for the purpose of introducing oxygen, so as to favor the production of fibrin. His patients recovered in about three weeks.

Dr. Sutton, of Georgetown, Ky., supposes there is generally a daily obscure chill in typhoid fever, and gives quinine, he says, with great advantage.

Dr. Desha relies on quinine and blue-mass as an antidote to the diarrhœa in typhoid fever, or quinine alone.

Dr. Geno gives quinine in typhoid fever to quiet restlessness, and equalize the circulation.

The great objection to quinine by Dr. Grant is, that it always increases the restlessness, sleeplessness, tinnitus aurium, (ringing in the ears,) and the deafness symptomatic of the disease.

Dr. Grant and Dr. Hawkins meet the crisis with diffusible stimulants.

Dr. S. Kersey, Lewisville, Henry county, treated three cases of typhoid fever with rhubarb, blue pills, camphor, valerian, quinine, and anodynes, with cold sponging. One died. One case treated with anodynes, quinine, turpentine, and cold sponging, recovered.

I will now give some extracts from those who have written books upon that subject:

Dr. Jackson commences with an emetic of tartarized antimony, and follows it with an active cathartic. If this does not afford decided relief, he then uses venesection, letting the blood flow until an *impression* is made; then gives tartar emetic in broken doses every two hours until decided nausea is produced, restraining its action on the bowels when necessary with opium. Formerly gave calomel in frequently-repeated small doses, so as to produce salivation, but finally abandoned the measure as useless if not injurious.

Dr. Nathan Smith has never seen any remedy shorten the duration of the disease; in mild cases gives no medicine, but keeps the patient quiet; gives

mild drinks and nourishment. In severer cases uses such means as will mitigate the most prominent symptoms; looks upon emetic tartar as "an inappropriate and unsafe remedy;" has known it convert a mild case into a severe one.

Chomal, of Paris, follows the expectant plan, giving only mild drinks; sponging with cold water; fomentations, mucillaginous injections, etc.; and in the latter stage gives tonics and stimulants; bleeds if the symptoms indicate it.

Loves pursues pretty much the same plan, but resorts to bleeding oftener. He says that "by the judicious use of the three principal means—bleeding, sweating, and tonics—the disease may be shortened a little—a day or two at least," and "hopes that a more successful treatment of this disease will yet be discovered."

Bouillaud bleeds freely and repeatedly, and cups and leeches in the intervals; does nothing else.

De Laroque begins with an emetic, and then gives daily brisk purgatives throughout the entire course of the disease.

Bartlett recommends the eclectic plan in a mild way, merely combating symptoms and taking care of the strength as much as possible, to enable the patient to bear up under the tedious course usually run by this disease.

Wood commences with a mild purgative; then bleeds, if arterial action is high; gives refrigerants to moderate pain; and in the second stage gives mercury and turpentine to combat inflammation.

By increasing the number of quotations from authors and writers in medical periodicals, the variations in treatment could be extended to an indefinite amount. In the South, physicians agree in hardly any thing in the treatment of this disease but in giving turpentine.

It is truly painful to think of, and humiliating in the extreme to our professional pride, that this, the most extensively spread febrile disease known to the human family, and therefore having been seen and studied by every medical inquirer in the civilized world, should yet be so little understood that not only no settled treatment has been agreed on, but remedies the most opposite in their operation are relied on with equal confidence, and, so far as appears, with about equal success—no one even pretending that the disease can be materially shortened. Surely, in view of this, the profession ought to be willing to candidly investigate the claims of a plan which proposes to do what all others have failed to accomplish, especially when a trial involves no risk of life, the remedies being mild in their operation, and involving no waste of the resources of the system, and their employment indicated by a rational view of the condition of the system in the disease.

I can offer very little upon the treatment of this disease, except what could be gathered from the observations which may be found upon this subject in the preceding pages; however, to bring the subject directly before the reader in a more condensed form, and enable him to understand my *manner* of using

the principal remedies, and the particular *helps* which should be called in by way of meeting the various difficulties which present themselves, from time to time, in the progress of a case of typhoid fever, I will now proceed to take up a case representing its most usual form, and give a kind of *programme* of my method of procedure.

All fevers, we have before observed, are very similar in their inception; therefore, when I am called to see a case in the forming stage, I think it best not to decide what form I may have to treat. If interrogated upon the subject, I answer that the patient has either taken cold or is about to have some form of fever, and that I cannot *name* it until it is further developed. In this stage, it is usually best to do very little; rest, mild drinks, and a gentle purgative are enough to prescribe. It is true, a full dose or two of the comp. syrup, before described, will usually cut the disease short at once, and it will never develop itself at all; but this is not always to be desired, for then you would never know whether you had aborted a case of fever or not; and even if you should be satisfied upon this point, the patient and his friends will not be, for however threatening may have been the symptoms of a dangerous attack of fever, yet if, on your return on the morrow, you find your patient clear of diseased action, you will also find that he thinks you were needlessly called in, and will not only refuse to award you any credit for your skill, but pay your pittance of a fee with reluctance. It is best,

therefore, to temporize until you have an opportunity of diagnosing the fever, and the patient becomes certain that he is sick "sure enough." But if the patient be of a delicate constitution, or is laboring under broken health from other causes, or is a female who is *enciente*, it is best to give the *syrup* at once, lest there might be risk in delay. Therefore, when called to see a patient whom I find complaining of some headache, general muscular weakness, alternate sensations of cold and heat quickly succeeding each other, loss of appetite, etc., and I think best to abort the disease at once, I prescribe a tablespoonful of the syrup every two hours to an adult, and in proportion for a child; a warm mustard bath to the feet, and chloroform liniment or camphor and laudanum to the spine, also to the stomach and bowels if there is any distress in that region, and direct quietness and the recumbent position. When I call the next day, I expect to find my patient sitting up, and, if a female, attending to her amusements or domestic duties, and to be greeted with, "Why, Doctor, you've come too late; I've got well; I was more scared than hurt; I feel as well as common to-day," etc. Now, you need not try to convince her that yesterday she was within the grasp of a powerful disease, and that you had made it loose its hold; she will not believe a word of it, and you will be in danger of making yourself appear ridiculous by trying. I know that physicians have as much disinterested benevolence as any people under the sun, but even they would

rather their favors were appreciated. Therefore, when the constitution is sound, wait a while. People love to take *physic* when they are sick, and it is best to indulge them. If you have any doubt about the case being typhoid fever, give a dose of calomel, and that will decide for you by the next day. On inquiry, you will be told that the *physic* "worked powerfully, and brought away more bile than you ever saw;" but upon making closer inquiry, or, what is better, by seeing the dejections, you will find that the *bile* is nothing more than yellow water, presenting the appearance of an egg stirred up in a gallon of very thin gruel. You will also find, too, that the patient feels generally worse: his headache has increased; more pain in the back; skin hotter; tongue covered with fine white fur, and is broader than natural; abdomen is more tumid, and slightly tender to pressure; pulse more frequent; heat persistent. Well, if you are satisfied that you have a real case of typhoid fever before you, just put him upon the treatment that was suggested for yesterday, with the addition of a poultice to the bowels; but if you still have doubts, temporize a while longer, but don't repeat the calomel, as it may make the disease worse than you would desire. Give Dover's powders if you like, and order castor oil or a seidlitz powder, or any thing else that will not do much harm; even quinine, if you think it indicated; and see him again in twenty-four hours. You will probably find that the purgative has not operated at all, or it has purged him profusely; there is

more heat of the skin; greater restlessness; headache unabated; confusion of ideas; talks wildly when dozing; tongue more coated in the middle, a little red at the edges and slightly contracted, and, on protruding it, you may perceive a little quivering, as though its fibres were acting "one at a time."

If you will delay another twenty-four hours, these symptoms will be still further developed; the tongue will be decidedly pointed, and a deeper red at the edges, and will perhaps tremble with a palsied motion or look rigid, with its tip turned upward. You now have a case which will do to record in your note-book, "A case of typhoid fever of five days' standing;" for though you have only been treating it three days, yet it is certain that it existed two days anterior to your first visit. Well, I should commence the treatment now, just as I would have done at the first, so far as giving the syrup is concerned. It would be *syrup* every two hours, liniment to the spine, poultice to the bowels, cold applications to the head, and sponge the limbs with warm soap-suds. On my next visit I should expect to find but little change of symptoms, for when this fever gets five days the start, it wont yield in a few hours; it now seems to have the tenacity of life of the *snapping-turtle*, and, like it, will hold on to its grasp some time after its vitals are crushed. I should therefore feel contented to find my patient no worse: perhaps less headache; not quite so restless, and bowels not so excitable, etc. A very

gradual, hardly perceptible improvement may be expected until the third or fourth day of treatment, when I should expect to find general evidence of improvement: the pulse, for example, about 90; skin much cooler; headache gone; sleep undisturbed; tongue not so red and pointed; but still no appetite, and no signs of increasing strength; perhaps the patient feels weaker, and will not believe that he is really any better. But I tell him he will be better to-morrow, and suggest some light diet, which I direct to be taken with the punctuality of medicine. On the next day I expect to find a decided improvement in his appearance: his countenance looking bright; his skin filled out, and looking lively and glossy; his eye brighter; his tongue having lost some of its redness and assuming its natural shape, but yet trembling. He now inquires what he may be allowed to eat, and likely suggests some article that strikes his fancy. If the tone of the bowels has not been injured before I have taken charge of the case, and it has not been of more than five days' continuance, I usually permit the patient to have a little of any diet he may fancy, even fried chicken or broiled bacon. But if purgatives have been injudiciously used, or the fever is in its second week when I first see it, I know that the bowels have been so injured that the utmost care will be required during convalescence.

My usual form of administering the *syrup* is, to give it in twice its volume of water or milk. It should always be diluted with milk for infants and

small children; and this is generally the best vehicle for giving it in fevers for all ages, the milk furnishing the means of sustaining the system, and the syrup securing its proper digestion and assimilation, and thus preventing a loss of tone in the stomach or much prostration of general strength. But sometimes the syrup, given in any way, becomes repulsive to the stomach, which renders it imperative to devise some other form in which to administer the remedies. The following pill constitutes a good substitute: Recipe—Pulverized rhubarb 30 grains; piperin, valerianated zinc, and extract of liquorice, each 10 grains; oil of sassafras, 20 drops; make 30 *pills*, each of which will be about equal to a tablespoonful of the *syrup*, and may be given at the same intervals. I often find it convenient and politic to substitute the pills for the syrup, even when the latter is taken kindly, so as to prevent the patient or his friends from thinking that too little is doing. It is generally best to give the pills in the night, and the syrup during the day. When the medicine is given in the form of pills exclusively, I have thought its effect in breaking up the febrile movement was not quite so potent as when given, at least part of the time, in the form of syrup. I never have a patient roused out of a quiet sleep to take medicine. As soon as convalescence is fairly set up, I make the intervals longer between the times of giving the medicine, making them three, four, and six hours, and finally

ordering only a teaspoonful to be taken directly *after* eating each meal.

When called to take charge of a patient in an advanced stage of the disease, after active inflammation has been set up, I meet this complication with additional treatment. If of the brain, I apply cold applications to the head, and a blister to the back of the neck, letting it extend some way down between the shoulders. If of the bowels, I first poultice, or pour a stream of warm water for an hour or two upon the exposed surface, and then poultice; and if these means fail, I then apply a blister: I prefer a large one; the irritation produced by a small blister is usually transferred to the greater one beneath, making the disease worse by adding to it.

I pay no attention to the secretions of the liver, knowing that, when the remedies have set up general capillary action, those of the liver will also participate in the reäction, and healthy bile will be secreted; and also knowing that a forced effort of the liver, under the goadings of mercurials, will in no way aid me in breaking up the febrile movement. I therefore only watch for the return of healthy biliary secretions, with the same solicitude that I do other secretions; viewing all of them not as a *cause*, but a *consequence* of commencing convalescence. If called in at an advanced period of typhoid fever, I can make no promise as to the time which the disease may then run; but if there are no serious inflammatory complications, I usu-

ally, even then, break up the febrile movement in four or five days, but convalescence is tedious.

I will now further illustrate my mode of managing typhoid fever, by detailing a number of cases treated by myself and others:

Case 1. August 30th, 1853.—Capt. Wm. B. Walton's negro boy Allen. Has had constant fever thirty hours. Great sense of weariness, much distress of the head, tenderness of the epigastrium, pulse full and throbbing, tongue covered with very short white fur and slightly red at the tip. Prescription: Bathe the abdomen and spine with liniment, and give a tablespoonful of syrup every two hours.

31st.—Had a slight chill last night and some fever, but no headache or other distress. Added five gr. quinine with four ounces syrup, and ordered a tablespoonful every four hours.

September 1st.—Appears clear of disease. Ordered the medicine to be given three times a day for a few days.

Case 2. September 1st, 1853.—John Stone, clerk at the Verandah. Has for several days suffered from a feeling of prostration, and a sense of heat and chilliness alternately; during the last night, sweat profusely and had copious discharges of yellow water from the bowels; abdomen tumid and sore, tongue furred and rather dry, substance of it rather dark-red, inclined to doze, and dreams half waking, pulse quite compressible and variable, constant alternations of cold and hot sensation.

Prescription: liniment to the spine and abdomen, tablespoonful of syrup every two hours.

Evening.—Pulse tolerably full and regular, skin warm and moist, abdomen less tense and tender. Continued treatment.

2d.—Same general improvement. Continued treatment.

3d.—Slowly improving; has had no passage from the bowels. Ordered two blue-mass pills, and continued syrup.

4th.—Found general improvement, consistent discharges, tongue cleaning, some appetite. Syrup every four hours.

5th.—Left his room and rode out.

Case 3.—September 18th, 1854.—Mr. Jackson, College street, boat-hand. Symptoms: dull, heavy headache, with disposition to sleep, mutters when dozing, skin hot on the body, extremities rather cool, abdomen tumid and tender to pressure, considerable diarrhœa, tongue furred in the middle, but clean and red at the edges, and becomes pointed and trembles on protruding it.

Treatment.—A poultice of corn-meal and mustard to the abdomen, liniment to the spine, and a tablespoonful of syrup every two hours.

19th.—Does not mutter so much in his sleep, bowels quiet, abdomen less tense and tender. Continued treatment.

20th.—General improvement. Discontinued treatment, except syrup every two hours.

Case 4. January 1st, 1855.—Son of Mrs. Tay-

lor, Cedar street, aged about twelve, small of his age and slightly formed, but usually enjoyed good health and was sprightly. His mother informed me that for about a week before I was called in, he had appeared unusually dull, and was inclined to sit about the house, and when directed to go on errands, went reluctantly; finally complained unusually of cold whenever he left the fire; lost his appetite, had headache, and soreness of the abdomen; diarrhœa at last set in, which for the last two days had been very severe, producing great prostration. I found him laboring under considerable headache, intolerance of light, abdomen tense, tumid and tender to pressure, considerable heat of the head and trunk, but extremities cool, tongue thickly coated with short white fur in the middle, but clean and shining red at the edges; in thrusting it out it became extremely narrow and pointed, and trembled exceedingly.

Treatment.—A poultice of corn-meal and mustard to the abdomen, hot bricks to the feet, liniment to the spine, and a tablespoonful of syrup every two hours.

2d.—General improvement; headache less distressing, diarrhœa checked, stools consistent, abdomen less tense and tender, tongue not quite so deep-red at the edges nor so pointed, but still trembles. Continued treatment, giving the syrup every three hours.

3d.—Is still improving. Discontinued all treatment, except the syrup every four hours.

4th.—Is clear of all evidence of disease, appetite returned. Directed the syrup three times a day for a few days, and dismissed the case.

Case 5. By Isaac N. Croom, M. D., Henderson county, Tenn.

"Mr. C., aged 16, was taken December 29th, 1858, with a chill; fever continued, and I was called in December 31st. Found him restless; pain in the head, back, and limbs; skin hot and dry; tongue furred in the middle and dry, red tip and edges, pointed and tremulous; bowels tender, and a disposition to diarrhœa; pulse 120.

"January 1st.—Symptoms about the same.

"2d.—Pulse 100; slept well; said he was nearly easy, but no appetite.

"3d.—Decidedly convalescent, and recovered speedily.

"The above case was treated upon your plan for typhoid fever entirely. A number of other cases could be given, which have been managed with equal success."

Case 6. By J. E. Fulton, M. D., Big Oak, Miss.

"On the 20th September, 1858, I was called to see Mrs. W., aged about 30, good constitution, former health very good. I found her laboring under slight fever; some headache; pain in the back; slight tympanites of the bowels; some soreness in the right iliac region; slight diarrhœa; tongue long and peaked and very tremulous, edges red, and black scurf down the middle. She informed me that she had been ill-disposed for four or five days,

but thought it unnecessary to send for a doctor, and, said she, 'I don't think I am much sick now.' Being satisfied of what was the matter, according to your directions, I put her on the comp. syrup of valerian, a tablespoonful every four hours; and on the 21st visited her again: but little improvement, probably not so much pain in the back. On the 22d I found her with less headache, and diarrhœa diminished; continued the syrup. On the 23d, considerable improvement of all the symptoms; and on the 24th, I found her sitting up in the bed eating; and on the 25th, I found her sitting on a chair rocking the babe. I dismissed the case, but ordered the syrup to be continued for a few days. The recovery was rapid and complete in a few days.

"Such has been the termination of all such cases since I came in possession of your most excellent book."

Case 7. By D. C. A. Moses, M. D., Eldridge, Ala.

"I was called to see W. G. W. H., on the 20th of April, 1857; and upon inquiry, learned that for the week previous he had been complaining of loss of appetite; general lassitude; inability to confine his mind to his business, (which was that of merchant;) dull pain in the head; general feeling of soreness in the muscles; chilly sensations alternating with heat; occasionally cold clammy sweat, morbid vigilance, etc.; and at the time of visit, found all these symptoms aggravated, with considerable febrile excitement; pulse about 110; considerable thirst; tongue

dry and pointed, tremulous upon protrusion; bowels slightly tympanitic, and tendency to diarrhœa; and during sleep constant muttering. Diagnosed, typhoid fever eight days' standing.

"Commenced treatment by sponging body (which was dry and hot) with warm whiskey, afterwards applied anodyne liniment to the spine and abdomen; tablespoonful of comp. syrup valerian every two hours; warm poultice to bowels, etc.; and without giving you the treatment for each day, which was nearly the same, suffice it to say, in six days he was convalescent. From imprudence, on the eighth day after convalescence, (having resumed his business and exposed himself for several hours in a cool draught of wind,) he relapsed, and when called to see him, I found him in the following condition: Perfect prostration, constant muttering, almost impossible to arouse him sufficiently to give medicine, hemorrhage from bowels, subsultus tendinum, impossible to protrude the tongue, bowels tympanitic, picking at imaginary objects, etc. But with the comp. syrup valerian and some auxiliaries, he was again convalescent on the ninth day, and has enjoyed uninterrupted health from that time to the present."

Case 8. By W. J. Miller, M. D., Gas Factory, Tenn.

"July 9th I was called to see James W., aged 17, sound constitution, had never had any serious sickness; had been complaining, for a week previous to my visit, of soreness of the flesh; aching of his

bones; chilly sensations, hot and cold flushes; when he would cover up, would become too hot, and when removed, he would soon feel chilly; tongue coated with white fur. His father had given him a dose of castor oil, which had acted freely; considerable fever; pulse 100. I prescribed six grains blue-mass, neutral mixture, and sponging the body. On the 10th I found all the symptoms increased, drowsiness and headache added to the above symptoms. I thought I had now a case of typhoid fever, and commenced your plan of treatment at once. It is useless to describe the treatment, as it was just as you direct. On the 11th, not much alteration any way. The 12th, the pulse had come down 12 beats, with some modification of skin and tongue. 13th, considerably better. 14th, so much better that I did not visit him any more. His fever had all gone, tongue about clean, headache gone, and soreness gone."

Case 9. Dr. T. G. Underwood, of Orange, Ga., writes that during the fall of 1858 he had an opportunity of treating many cases of typhoid fever, as that disease was epidemic in that region of country. He gives the symptoms as follows:

"Want of appetite; giddiness and sometimes nausea; countenance dejected, dull heavy sensation in the head, and a general feeling of weariness; general debility; no inclination to mental or corporeal action; tongue coated with a white fur. These are the premonitory symptoms: after which slight chills in the majority of cases, alternating

with heat of the body; entire disgust for any kind of food; pulse quick and small; mind confused; dull, heavy pain in the head; general mental and physical depression; pain in the back and limbs. These symptoms would last for a short time; after which the febrile heat would increase; the face become flushed; the pulse rise in strength; the skin become dry and the lips parched; great thirst for cool drinks; tongue coated now with a brown fur; pain in the head, back, and limbs; increased tension and tenderness in the hypochondria; the brain more disturbed; hearing obtuse, and more or less delirium. Other symptoms were occasionally present.

"All such cases treated by other physicians, not in possession of your plan, ran the usual course. I treated mine upon the plan laid down in your book, and the average time of treatment before convalescence was from six to ten days; owing, though, a good deal, to what stage of the disease I was called in; if in the first stage six days, and if not, ten days.

"There was but one death, and that patient relapsed from eating pork and turnips after she was able to walk about the room."

The following letter, though not from a physician, contains so much good sense that I am induced to give it entire.

SUNFLOWER LANDING, COAHOMA COUNTY, MISS.,
March 12, 1859.

R. THOMPSON, M.D.:

DEAR SIR:—Not being a physician, but only a planter, it did not seem important that I should

give my experience with your mode of treating fever; but courtesy to you, and also a wish to add my testimony as to its value, induce me to address you.

I have used your plan constantly since the early part of 1857, and in no case without beneficial results. Besides its use in pneumonia and typhoid fever as prescribed in your book, I have used your fever syrup advantageously as a general stimulant for feeble persons; and in this our damp and miasmatic climate, I am satisfied it is very valuable in such cases, affording a resisting power to the constitution. In a few instances (that of my wife amongst others) the recurrence of chills has been prevented by it. But I generally first break the chill with quinine, and then give the syrup to insure convalescence.

In our climate the constitution seems to have very little reäcting power, even after disease has entirely disappeared; in other words, we get well slowly; and this depressing tendency of the atmosphere to keep us down when once it gets us down, is to be overcome by stimulation. No other stimulant appears to be equal to the fever syrup for this purpose. The syrup has been used on the plantation of my sister-in-law, also in this vicinity, by the overseer, whether intelligently or not I am not able to say; still, neither there nor on my own place has death ensued in a single case in which it has been used; in other words, we have not lost a single case of pneumonia or typhoid fever since I became ac-

quainted with your plan of treatment, and we have had to contend with some severe ones. It is proper to say that I have also used quinine, ipecac, Dover's powder, and blue-mass, very moderately in the earlier stages, and also blistered; but have invariably employed your syrup in a manner fully up to your directions, relying chiefly upon it.

One of my neighbors thinks I saved the life of a valuable negro of his, a month or two since, by sending him a bottle of the syrup with directions.

I ought to add that I have given it in cases of ordinary bilious fever, remittent and intermittent, but could not see any marked advantage until after the disease had been broken up by other usual remedies; then, as a stimulant to aid convalescence, it is of much value.

Very respectfully yours,

H. C. CHAMBERS.

D. C. A. Moses, M. D., of Eldridge, Ala., makes the following suggestions:

"I have not confined myself strictly to your plan, but have slightly altered it to suit the peculiarities of different cases, and have also called in auxiliaries. For instance, when there has been great arterial excitement, with a tendency to cerebral derangement, I have used as auxiliaries the saline bath and the veratrum viride, and have also substituted the oil of valerian for the alcoholic tincture. Again, when there has been great depression of the vital forces, with a tendency to coagulation or stagnation

of the fluids, I have substituted the ammoniated tincture of valerian, because of the known power of ammonia to hold in solution the protein compounds of the blood," etc.

Now I think well of the above suggestions, except the giving of veratrum viride, which I know to be a potent nervous depressor, and therefore in the end is likely to do harm. A distinguished physician has said that veratrum viride will control inflammatory action in pneumonia, but that the patient would not get well. I would much rather lessen arterial action by giving sub-nauseating doses of emetic tartar.

J. D. Steel, M. D., of Charleston, Miss., informs me that he has treated about seventy cases of typhoid fever upon the plan laid down in this work, without a death, and that convalescence was set up in from three to five days. Many other physicians have furnished evidence that typhoid fever can be aborted by my plan of treatment; only one states that he has failed, and I am forced to the conclusion that the materials he used could not have been genuine.

It is well known that there are two kinds of sassafras, the red and the white; now the oil obtained from the white is nearly inert, it has little pungency, and will not destroy insects. It may be known from the red by having a whiter color.

One physician inquires of me if the syrup is contra-indicated in gastritis connected with fever. I have already said that it is, but in mere excitement

of the stomach it may be freely given, as it serves to allay it; and much experience by myself and others has proven that it may be given not only with impunity, but with signal advantage when there is inflammation either in the small or large intestines. In inflammation of the stomach it makes too strong an impression, and must be suspended. For such cases, besides poultices and fomentations, I give the following:

Emulsion of Gum Arabic, ℥iv.
Sul. morphia, gr. iv.
Spt. turpentine, ℥j.
Comp. spt. lavender, ℥ij.
Mix.

Give a teaspoonful every two hours. A free application of the chloroform liniment should also be made to the spine and epigastrium.

A number of practitioners have complained of the great pungency of the syrup, which renders it difficult to take. This must be owing to some fault in its preparation. No medicine is taken with less trouble by my patients; in fact, it is often spoken of as being peculiarly grateful, and if the directions already given (of rubbing the sassafras, piperin, and sup. carb. of soda first in a mortar, and then adding the syr. of rhubarb, again rubbing it, and lastly adding the tinc. val.) are observed, it will never be unpleasantly pungent. Druggists will not attend to these particulars unless especially charged upon the subject; but if the syrup is still too pungent, order it taken in sweet milk or emulsion of slippery elm or gum arabic.

BILIOUS OR MIASMATIC FEVER.

This disease presents a wide diversity of grades and symptoms, as it is seen in different latitudes, or in different individuals in the same region.

The leading symptoms as clearly indicate primary disturbance in the cerebro-spinal centres, as those of typhoid fever do a morbid condition of the ganglionic system. But as the latter system must become considerably disturbed before capillary inaction can be produced, which, we have seen, is an essential part of every case of fever; and as we see the secretions all more or less depraved, which also are under the control of the ganglionic nerves; we need not be surprised that in many cases it is difficult to decide, especially in the early stages, whether a case be typhoid or bilious. In elevated countries and in vigorous constitutions, if this disease occurs at all, it is usually characterized by a high grade of action, simulating inflammatory fever; whereas, in the far South, and in the West, in recently reclaimed alluvial districts, vital energy is often so completely exhausted by a continued action of the cause of this disease, that very slight reäction follows the stage of depression, and in some instances there is no reäction at all, but the patient dies in the first stage. In other cases the reäction is considerable, but is followed by very slight remissions, merely a lessening of the most urgent symptoms in the morning, as in continued fever; then, again, we see well-marked remissions,

and in others complete intermissions, so perfect, that one unused to the disease would never expect a recurrence.

Now it has been a matter of inquiry for many thousand years, what is the nature of the morbific agent which produces this disease, and what are the circumstances necessary for its elaboration. At one time the medical mind had decided that it originates in the decomposition of vegetable matter, but this idea is now generally abandoned; and it has been equally authoritatively decided, that the only circumstance necessary for its production is high temperature, acting upon a soil which has been saturated with moisture. But however produced, or whether organized or unorganized, it is certainly a narcotic poison; the symptoms which precede an attack indicate this; as giddiness of the head, erratic pains, muscular weakness, pallor, and a dirty hue of the surface—just what we often see following an intemperate use of tobacco, opium, and other narcotics—and in very grave attacks, produced by sudden exposure to the poison in a highly concentrated form, the symptoms are precisely those of intense narcotism, occasionally causing death without even the nervous reäction constituting a chill.

Now this inquiry into the nature of the poison producing bilious fever is not merely interesting, but is of the first importance to a correct understanding of my plan of curing it. For a long time the cinchona and its preparations have been chiefly

relied on in the treatment of intermittent fever, but all agree that, notwithstanding the disease is very uniformly arrested by the use of quinine, yet it is proverbially prone to return, often making it necessary to cure it many times before it will finally stay away. Now this is accounted for by the fact that quinine cures intermittents by acting as a nervous stimulant, producing a state of nervous power incompatible with that attending a chill; but the effect soon subsiding, it is not wonderful, therefore, that the disease returns. Now the oil of sassafras evidently neutralizes this narcotic poison; and hence, when it is combined with quinine, the disease very rarely returns; but more of this presently.

I will now take up a case of bilious fever, and run it through its course, in order to illustrate my mode of management.

Suppose, for example, I am called to see a case presenting all the common symptoms of the forming stage of fever, and almost a counterpart of the one which we have considered, which turned out to be typhoid fever, but still I am not warranted in deciding that this will prove to be one; perhaps there have been other attacks lately in the family, or the immediate vicinity, which have shown themselves in their progress to be malarious, and I reasonably presume that this is of the same kind. But if I have determined to abort it, any speculations as to whether it is typhoid or miasmatic are uncalled for, as that point will never be ascertained;

for putting the patient under the same treatment proposed in the forming stage of typhoid fever, will so effectually break up the diseased action, that no trace will be left which would indicate what way the disease would have run.

But suppose I try a dose of calomel, given in the evening, and visit the patient next morning, and find him much improved—the headache gone, the skin naturally moist, consistent bilious discharges, etc., etc.—I settle the case in my mind at once, that I had treated *bilious* fever in its forming stage. But have I aborted it? Perhaps not, the favorable appearances notwithstanding; and if I do not guard against it, by the time I have made my round of calls and returned to my office, I will probably find a pressing message to repair in haste to see my patient, as he is much worse. Well, I judge at once that he has had a chill; and so I find on my arrival. He is now in the stage of reäction, and his headache violent; heat of the surface pungent; thirst great; stomach irritable; pulse full and throbbing, etc., etc. Now what are the indications? Clearly to quiet the nervous excitement, and open the capillaries so as to admit the blood to flow on freely from the arteries, and thereby relieve the excitement of the heart; and I will do this by giving a large dose at once—two or three tablespoonfuls of comp. syrup valerian in a glass of cold water—and at the same time bathe the head, neck, breast, and arms with cold water; in less than half an hour I will have my patient quite comfortable.

I then leave him the following prescription: Sul. quinine, piperin, blue-mass, each 10 gr.; oil sassafras, 5 drops; make 10 pills; give one every two hours until they are all taken; then give a table-spoonful of the syrup, at the same interval, in half a glass of cold water, and direct some additional purgative to be taken *after the time of the chill has passed*, provided the bowels are not pretty freely moved without. I usually prefer Epsom salts. But I very often, yes, generally, leave out the mercurial entirely, and treat the case with the syrup alone, except a single five grain dose of quinine given soon after the hot stage begins to abate. The liver will commonly act very well without any assistance, except the general capillary excitement produced by the syrup; another formula often depended on is comp. syrup valerian, ℥ij; sul. quinine, 10 gr.: a dessert-spoonful every 3 hours. If the chill should be prolonged, and the reäction imperfect, showing that the fever is of the type known as congestive, I still give nothing but the *syrup*, but increase the dose largely; have given two or three ounces at a time, and repeated it in an hour. This, with external warmth, and frictions with some stimulating liniment, will generally soon bring about a fine reäction. I then treat it as a common case of chills, except that I think it safer to increase the quantity of quinine. To those who have been in the habit of resorting to active depletion and free evacuations, in order to subdue the over-action in high grades of *bilious* fever, the idea

of bringing down the excitement by the use of piperin, oil of sassafras, etc., will appear quite unphilosophical. But if they will recollect that the *condition* of the system in the lowest grades is precisely the same which exists in the highest, and when they recollect what that condition is, they will cease to be astonished that the same capillary stimulant will meet both cases.

I have had many of the high grades of bilious fever under my management within the last few years, which I formerly thought called for the use of the lancet, tartar emetic, etc., but which have yielded kindly to my present mode of practice, and left the strength of the patient but very little impaired, so that convalescence was rapid. But still, if I were called to a case in which active inflammation had been set up in some vital organ, I should certainly bleed; but my experience leads me to believe that in cases treated upon my present plan, no inflammatory complications will probably ever arise; they have not in my practice.

Now, if there be no distinct remission, I make one by sponging the body with cold or tepid water, or applying the wet-sheet or the half-pack of the hydropathists, that is, a large wet towel laid upon the stomach and bowels, and removed as occasion requires. At the same time I order the following:

℞—Comp. Syr. Val., ℥ij
Dov. Powder, gr. x.
Mix.

Dose—a dessert-spoonful every two hours.

I will further illustrate the treatment of bilious fever, by giving observations extracted from letters received from intelligent physicians in different States.

J. W. Steel, of Charleston, Miss., says: "The year 1858 will be remembered as the most sickly year within the recollection of the oldest inhabitant in this country. I prescribed for fully seven hundred cases of bilious fever, and as I made your book my guide, I lost no case, and had no protracted case. I very seldom prescribed any thing but the syrup, as it generally fulfilled every indication."

Dr. Steel says nothing about adding quinine to the syrup.

Dr. D. C. A. Moses, Eldridge, Ala., says, "The prevailing fever with us is remittent, of a low adynamic type. I have attended within the last year some three or four hundred cases, and have never failed to arrest or abort it in from twelve to thirty-six hours. I follow your book with hardly any deviations."

Dr. Moses is silent on the use of quinine as an adjunct.

Case by A. D. Cutter, M.D., of Dresden, Tenn.:

"I was called to a gentleman taken with a chill which had lasted twenty-four hours before I saw him—had a congestive chill, I would say. Gave him calomel in twenty-grain doses, until four doses were given, which procured several copious green evacuations the next day. At my first visit I had none of the syrup with me. At my second visit, as the calomel had acted freely without apparent benefit, I resolved to try the syrup. He was still restless, with

pains in the head and abdomen, sick stomach, tongue white, pulse one hundred and forty. Gave nothing but syrup, one tablespoonful every two hours in half a tumbler of cold water. Four ounces completed the cure in five days. It acted like a charm."

PNEUMONIA.

If in addition to all the common symptoms characterizing the first stage of fever, there is added unusual shortness of breathing, a hacking cough, with more or less expectoration of a tenacious mucus, if there is some pain in the chest, and the countenance looks flushed and mottled, I suspect at once that I have a case of pneumonia, and immediately examine for the *physical* signs, and if these are present, I then know that I have this troublesome disease to contend with. My observations have convinced me that *pneumonia* is nothing more than inflammation of the lungs, added to an ordinary attack of fever, and that this fever may be typhoid, miasmatic, or common inflammatory, from the influence of cold, etc. This being the case, the general treatment will be precisely the same as in a like case of fever uncomplicated, with the addition of such means as will most directly reach the local difficulty in the lungs. The most efficient of these, both for subduing inflammatory action in the lungs, and for relieving engorgement by setting up free expectoration, is large flannel cloths wrung out of very warm water, and made to envelop the chest, and renewed every few minutes. But as example is more striking

than precept, I will present the reader with the history of the treatment of a very violent case of this disease, which I managed during the winter of 1854. The patient was a Mr. T. of this city, a young man of intemperate habits, and who had been, for some weeks previous to the attack, most of the time intoxicated. This being the case, his friends looked upon his attack as nothing more than a *cold*, and neglected to apply for medical aid until the disease had progressed a number of days. When I saw him, the powers of life were very nearly overwhelmed. His breathing was exceedingly hurried, attended with an incessant hacking cough, and no expectoration whatever; his pulse was frequent, (about one hundred and fifty,) but had very little force or volume; no respiratory sounds whatever could be heard over the lower half of the chest; his eyes were deeply injected, and his face flushed, with livid blotches; the under side of his face, and even of his arms and body, presenting the appearance usually seen some hours after death, occasioned by the blood settling in the capillaries of the dependent parts; his extremities were cold and shrunken, and altogether he presented a state of the system in which there can be but little hope of a successful reäction. But I resolved on making an effort, and accordingly ordered hot water prepared, and a number of bricks to be warmed. For, although I did not presume that the prolongation of the life of the patient would *pay* for the smallest outlay of skill, yet, as a test of the powers of the mode of treatment, it might be of

immense value. I accordingly devoted as much time to seeing that the plan of treatment was duly carried out as was compatible with other duties, and thus saw that the directions were fully obeyed, viz., that the whole chest should be enveloped with the cloths wrung out of warm water, and repeated every ten or fifteen minutes; that the extremities be kept warmed by the imparted heat of hot bricks, etc., etc. I gave him a tablespoonful of the syrup every hour, in half a glass of water, and allowed him no other drink. After the lapse of twelve hours I discovered the following changes: his cough was not so incessant, but when he did cough, it was attended with a free expectoration of exceedingly offensive matter, such as evidently had been accumulating in his lungs for days; his pulse was less frequent, and his skin and tongue showed a little capillary action. On listening for sounds in the chest, I now discovered a distinct *tubular sound*. The same treatment was continued for another twelve hours, when I thought best to discontinue the use of the hot cloths, and cover the whole chest with a fly-blister. At this time, in addition to the tubular sound, I discovered some *crepitus*, and a little afterwards the real *mucous rale*, showing that the air was having access into not only the smaller bronchial tubes, but also their minute ramifications; twelve hours later, the blister had drawn well, and as a result of this, and the general stimulant effect of the syrup, the breathing was much easier, the expectoration freer, causing little effort, the pulse had sunk

to one hundred, the countenance looked more intelligent and lifelike, the skin had lost its mottled hue, and the lungs produced a sensible *respiratory murmur* over a part of their surface; the tongue was yet red and pointed, and trembled as it usually does in the second week of typhoid fever, the strength was exceedingly little, and mental effort almost nothing, the patient seeming, though rational, to have the mind of an infant. The blister, as the sensibility was very low, caused no suffering of consequence, but produced a most happy influence upon the *irritability* of the capillaries, causing those of the lungs to act more vigorously by *direct association*, and those of the general system to feel the impression through the nervous centres; the result was, that there was a *general amendment.*

The blister, however, soon seemed to lose its effect, and the hot cloths were obliged to be again resumed, to keep up that amount of action in the pulmonary capillaries which is indispensable to the performance of the function of the lungs sufficiently to continue life at all. But by persevering in the use of these means as occasion required, and a regular use of the syrup, the system was kept in that condition which enabled the recuperative powers to act with such efficiency that eventually a healthy action was established, and the patient slowly recovered. No medicine was given internally during the entire term of treatment but the syrup; being resolved, as the patient's life was probably of no other advantage, to make it of value in establishing

the fact that this medicine, with the aid of external means, was capable of meeting the very lowest condition of vital action compatible with life. Mr. T. has regained his usual health, and has been able to stand the effects of many drunken sprees since then.

By relying so exclusively on the syrup in the above case, I did not suppose I was at all jeopardizing the patient's life for the sake of *experimenting*, for I had treated many cases of pneumonia previously upon the same plan, with entire and prompt success, but have not always confined myself to that alone. In cases in which there is a good deal of spasm in the lungs, and in such as manifest much active inflammation, I give emetic tartar in sub-nauseating doses, until these symptoms are abated. I have not resorted to the lancet, even in pleuro-pneumonia, for several years, except in one single instance, and then I thought my patient was damaged by it. Since the above case I have managed many others upon the same plan, with uniform and prompt success, having had no cases of tedious convalescence, and having none to die except one, which I will now advert to, for I think best to state the *whole truth*.

I very lately was called to treat four cases of typhoid pneumonia, all attacked near the same time. Three yielded kindly to treatment, being entirely relieved in a few days. The fourth also promised well for the first two days of treatment, but on the third assumed a more intractable form.

This was the occasion: it was Christmas day, and many of the *bucks* had become over-jolly; one had fancied himself insulted by another, and drawing a pistol threatened to shoot; the other made his escape, and the infuriated inebriate, in search of him, burst into the room in which my patient lay, (a timid girl,) exhibiting the rage of a demon, and so affrighted her that on my return I found her in a high state of nervous disturbance, which continued to increase in spite of all my efforts, until it presented many of the characteristics of true *tetanus*. I had Professor Bowling called in consultation, but no efforts were availing, and she died on the seventh day of treatment.

If any other apology were necessary for the loss of this patient, it might be found in this: she was of a highly nervous temperament, the disease was excited by exposure to wet and cold during the catamenial flow, causing suppression; she had been treated by her mother as for a cold, with stews, etc., for eight days before I was consulted; complete hepitization had at that time been produced in more than half of the left lung; still I believe I would have saved her but for the scare.

J. E. Fulton, M. D., Big Oak, Miss., says:

"During this winter (1858) I have had seven or eight cases of typhoid pneumonia, in the treatment of which I have confined myself entirely to your practice. *No deaths;* the recovery from eight to twelve days; and upon the whole my experience goes to warrant me in the conclusion that if your

plan is assiduously followed out in typhoid cases, the result will be universal success."

J. W. Steele, M. D., Charleston, Miss., writes:

"We have had a great deal of pneumonia this winter, (1858.) Most of the cases that I have had I believe would have been typhoid had I not aborted them. I have had but one case to last over five days, and that was an old man near seventy years old; he was down about two weeks. I seldom ever give any thing except the syrup and nauseants. The syrup meets every indication. Other doctors lose about one-third of their patients."

W. Kirk, M. D., Buckhorn, Miss., writes:

"Typhoid pneumonia has been prevalent here this winter, (1858,) and I have succeeded in curing every case so far. I have always used your syrup when the cases have been ushered in with a chilly sensation, and I know of nothing equal to it in producing a reäction."

YELLOW FEVER.

Never having seen this disease, it may be considered rather presumptuous to attempt to form any judgment concerning it differing from that of those who have studied it at the bedside. But during the excitement of an epidemic, physicians are usually not in the best frame of mind to philosophize upon the facts presented by the suffering and the dying. It is only after the battle is over that a cool survey can be made of the battle-field, and just estimates formed of the positions which the foe assumed, and

the relative advantages of different means which may have been taken for repelling or circumventing him; and one who took no part in the contest, and, consequently, has not his own actions to justify, will be more likely to perceive where an error had been committed or an advantage neglected, than those who were identified with the conflict. I have carefully studied the history of this dreadful disease as given by those who have contended with it, and impartially compared the various modes of treating it which have been adopted by those who have been most successful, so as to be able to determine, if possible, upon what the success of each depended, and wherein all fall short of meeting the requirements in the case, and will now give the reader the result of my reflections.

The best description I have seen of this disease, and the most rational mode of treatment, attended with the largest success, I find in the Proceedings of the National Medical Association for 1854, by Dr. Fenner, of New Orleans.

Dr. Fenner agrees with most other writers who have closely observed the commencement and spread of the disease, that it has a local origin, and spreads from the point of commencement, not by being carried by *persons*, but by an extension of the infected atmosphere; and that, when once originated, nothing but a reduction of temperature will arrest its progress. Dr. Caldwell, upon this subject, says: "The fact, as here stated, is true, and the problem it presents is difficult of solution: when an epidemic

yellow fever has begun its career in one of our large commercial cities, nothing but a termination of warm weather can arrest it. Local nuisances may be removed, the inhabitants of the city may fly, man may erect all his artificial barriers, currents of water may be made to flow along the gutters, rains may fall and wash the entire streets, and the winds may blow and change the atmosphere of the place, but all to no purpose. If the temperature of the atmosphere continues high, the epidemic mocks at resistance, until it expires under a regular change of season." Now, were this poison a gas, evolved by the decomposition of animal or vegetable matter, would not removing the filth stay its production? Would not rains wash it away, or winds disperse it? But as none of these things are so, then the poison is not a gas, but must be something which has in itself the power of increase, which can retain its position by its own powers in opposition to wind and currents of water, and which can direct its own course, so that when once it has obtained an existence, it multiplies independent of the original source, and travels "whithersoever it will," independent of currents of commerce or currents of air. It must then be an organized existence, having a will, or being directed by instinct, and increasing by natural propagation.

The cause of yellow fever therefore is, as I believe, poisonous animalculæ. The effect produced upon the system by this poison is much allied to that arising from the malaria which causes bilious fever.

The symptoms of the first stage of yellow fever, as described by Dr. Fenner, are precisely such as we see in the same stage of high grades of bilious fever. The patient has felt chilly, with headache and pain in the back, etc. Went to bed, and covering up warm, a high state of excitement was soon developed, viz.: violent pains in the head and back; skin burning; countenance flushed and swollen; eyes red; pulse frequent, full, and bounding; thirst intense; stomach uneasy, sometimes nausea and vomiting; restlessness, etc. He says, it is evident the symptoms plainly indicate *super-excitement of the whole system*, and the *principle* to be acted on is, "*to restore the equilibrum of the circulation and the excitement.*" Now there is some mistake here; if there was really super-excitement of the *whole* system, there would be *harmony of action*, and there could be no necessity for efforts to restore the *equilibrium*. It is exactly because there is not a *general* over-excitement, that we are called on to interfere. There is great *partial* super-excitement, but it is not general, or the patient would be no worse off than when taking vigorous exercise in obedience to some strong mental impulse. The history of the case up to this time is as follows: a morbid agent has entered the system, and has produced a powerful impression upon the nervous system; so powerful that it has been for the time overwhelmed, and the usual amount of nervous influence not sent out to keep up the various operations of life; hence the chilliness and inclination to go to bed, etc. But

presently the nervous system reäcts, and a powerful impulse is sent to every part. The heart and arteries act with unusual power, and the blood, which had been accumulating in the large veins, is sent with force throughout the system; and did it not meet with *resistance*, if there were no *obstruction* anywhere, an equilibrium would unquestionably soon be established. But there is an obstruction; the capillaries have been debilitated by the same cause which irritated the nerves, and they refuse to pass the blood on with the rapidity which would be in harmony with the action of the heart and arteries. Hence a distension of the capillaries takes place, giving rise to the suspension of the secretions, swelling and flushing of the countenance, redness of the eyes, restlessness, etc. Now what does this view of the condition of the patient indicate should be done? The doctor says, truly, that "the overheated skin must be cooled, the accelerated pulse must be retarded, the distressing pains, thirst, and restlessness, must be relieved. These desirable results may doubtless be effected by different means, but that is the best method which accomplishes the object most promptly, effectually, and *safely*." All those indications I believe would be met by my *fever syrup*, *promptly*, *effectually*, and *safely*. By soothing the excited nerves, and restoring the action of the capillaries, the whole of the phenomena would cease at once. Why this should be so is plain enough: the obstruction in the capillaries to the onward flow of the blood is unquestionably the

cause of the protracted increased action of the heart; the dilatation of the capillaries also causes the pain and restlessness, by compressing the fine nervous expansion in their coats, and in the interstices between these minute vessels; the heat is also occasioned by this congestion, so that restoring the action of the capillaries will necessarily remove all febrile phenomena. But in this, as in other forms of fever, if the case were obstinate, I should call to my aid every other appliance which could be made to operate in unison with the principal remedy. The nervous excitement could be greatly controlled by stimulating anodyne liniments; the heat of the surface by sponging with cold or tepid water. But I should never think of bleeding, unless there were actual inflammation. Mere over-action and super-excitement can always be subdued by the means I have stated, without lessening the fountain of life upon which we must depend for the final restoration of health; and if the cause be a poison still floating in the blood, and by its continual action keeping up the disturbance, taking away a part of that fluid will not lessen the proportion that will be left in what remains, or materially reduce the whole quantity, unless it should be carried to the extent of the case named by Dr. Fenner, when near a hundred ounces had been abstracted, (one gallon;) in this case the poison must have been pretty well eliminated from the system, as the man could have had but a small quantity of blood left to contain it. But I should by no means have been sur-

prised, as was Dr. Fenner, by his dying of hemorrhage in the form of *black vomit*. Such a loss of blood and vitality ought to produce a state of relaxation in the capillaries, which would allow the blood to pass unchecked through all the mucous membranes.

I would suggest to any one disposed to test this mode of treatment, that, in consideration of the great irritability of the stomach, which is often present, it would be well to first apply a cloth saturated with chloroform liniment to the epigastrium, and as soon as it begins to burn considerably, give the syrup in a decided dose—say two ounces. If this be rejected, repeat it immediately, even to the third time; but when a dose has been retained, give no more for two hours, withholding all drinks; by this time the medicine will have produced its peculiar effects, and drinks may be allowed as desired.

Believing, as I do, that the cause of yellow fever, though not the same, is allied in its nature to that which produces bilious fever, and that the condition of the system in both is much the same, I would expect to arrest it by much the same means. The course of treatment which has been suggested to my mind by the symptoms, as described by accurate observers, is as follows:

I should bathe the spine and epigastrium with *chloroform liniment*, or laudanum, camphor, and turpentine; give the comp. syrup of valerian in such

quantity as the condition of the system would indicate; if vital action seemed to be very low, I should give it as in congestive fever, two or three ounces at a time; I would, at the same time, give quinine and blue-mass in the form of pills; apply an emollient poultice to the abdomen, sinapisms to the feet, and cold applications to the head.

Under the influence of this treatment, I should expect all my patients to recover very rapidly, unless the constitution had been previously much injured by other causes, or this disease had been suffered to run until serious lesions had been produced in vital organs. I hope some one may be influenced to give this plan of treatment a trial, should this dreaded disease again visit our land.

DYSENTERY.

As Dysentery, as well as Cholera Infantum, are only serious diseases when attended with fever, I have thought best to include them as sections under this chapter.

Many persons have expressed a wish that I should reproduce an article on Dysentery published in the March number of the Nashville Journal of Medicine and Surgery, 1853. As that article contains much important truth, and has a freshness of thought and accuracy of description, the result of a strong impression which the disease had then recently made upon the author's mind, he has concluded to copy it entire:

PROF. BOWLING:—In the 2d volume (p. 188) of your Journal, you published a note from me upon the use of peach leaf in dysentery. Since then I have had a pretty large experience in that disease, and as I found it necessary to vary my treatment somewhat, I will now, with your permission, offer a few additional remarks upon it.

In July last the flux made its appearance on the southern border of my practice; and although it was confined to a few families, yet for a while it gave me a good deal of trouble. It did not yield readily to the influence of my favorite remedy; a main reason was, that there was great gastric distress, with frequent vomiting or inclination to vomit, so that the tea could not be retained long enough to be of any benefit.

The tongue was usually thickly coated in the middle with white fur, and red at the edges; pulse small and corded; surface covered with a disagreeable perspiration, sometimes profuse and general, sometimes partial and alternating with a dry, hot surface; extremities generally rather cool, often quite cold; almost all complained of a burning sensation in the region of the sternum, commonly about its middle; the discharges from the bowels were generally small, and composed of blood and mucus, and attended with tenesmus; the worst case, however, had no tenesmus, and the discharges were more copious, and consisted of a reddish jelly. Finding I had a more formidable enemy to contend with than I had usually encountered, I had to draw

upon other resources to enable me to overcome it. In order to make the peach leaf available, it was necessary to devise a form which would be retained by the stomach. I accordingly prepared it as follows, after reducing the tea to a fluid extract: I added a sufficient amount of sugar to make a syrup; to this I added sup. carb. soda, until it became slightly alkaline, and piperine enough to give it a pleasant pungent taste. I also added some of the oil of sassafras, for the purpose of preventing gangrene of the bowels, which I believed to be imminently threatened.

I have long used the sassafras as an antiseptic, and believe it superior to any other known. I learned this property of the sassafras more than twenty years ago, while living in McConnelsville, on the Muskingum river, in Ohio, there being many salt works in the vicinity, which gave employment to several hundred hands, many of whom were dissipated and reckless: shocking bad scalds were of frequent occurrence, by having an arm or a leg immersed in the boiling brine; dangerous sloughing was often the consequence.

In the management of these cases, I found the sassafras poultice more effectual in arresting gangrene and restoring vitality to the parts than any other means, and have used it ever since in all cases of gangrene coming under my treatment, with the most happy effect. I also used it, and found it a valuable means of fulfilling several other indications, one of which is to prevent narcotic

stimulants from producing injurious effects upon the brain. It is invaluable for this purpose; its greatest power is exerted over hyoscyamus and tobacco.

I found the above compound to answer my purpose finely. It was not only well retained, but quieted the irritation, both of the stomach and bowels; brought about a more uniform temperature; dried up the cold sweat when present; in short, answered every purpose for which it was compounded. I kept the abdomen, most of the time, enveloped in a peach leaf poultice, and gave injections of the tea, thickened with flour, to which I added pulverized opium in preference to laudanum: I found it less stimulating and more astringent.

I have long been in the habit of exposing a piece of opium to the air to dry, on purpose to be used in the bowel complaints of children. It is much less offensive to the stomach than the moist article, and is less narcotic and more astringent. It of course must be given in greater quantity. This injection usually gave the patient three or four hours rest from the tearing discharges, much to his comfort and advantage.

I occasionally, however, met with a case in which the irritability of the bowel was so great, and its involuntary contraction so strong, that mechanical assistance had to be rendered to enable the patient to retain the injection. This was afforded by making firm pressure upon the hip. After the lapse of ten or fifteen minutes, the bowel usually

ceased to make any expulsive effort, and the pressure was then withdrawn. I have also met with cases the very reverse of the above. There was no tenesmus, no pain attending the discharges, which were tolerably copious, and composed of red jelly, having no fœtid smell, but a peculiar odor, something like that arising from water in which fresh meat has been washed, and were voided almost involuntarily.

On examination, I found the sphincter so relaxed as to bring several inches of the bowel within view, which presented the appearance of raw macerated beef. In these the surface was always more or less cold, and covered with a dewy perspiration; great thirst and precordial uneasiness. I treated these cases by throwing up cold sugar of lead water into the bowel, and repeating it every few minutes, until I saw some contraction take place; then gave a small injection as above, and made it stay by pressure. I found it best to make my injections very small, not over half a gill. This quantity would be retained without difficulty, when a larger, owing to the stimulus of distention, could not be made to stay a moment.

By the first of August the disease had disappeared in my practice, but still prevailed south and east. In the neighborhood of Lancaster, and east of the Caney Fork, extending into a part of Jackson county, the disease was attended with unusual mortality. At the solicitation of a Mr. Owens of our neighborhood, I visited his son's family, east of

the Caney Fork, and found all of them very sick, except himself. After prescribing for them and a few others in the same vicinity, I went to Lancaster to see my friend, Dr. Rose, who I learned was prostrated with this disease.

Here I met with Dr. Sypert, who lived a few miles distant, both of whom strongly solicited me to remain, and try to stay the destruction of human life, which was then progressing at a fearful pace. More than a hundred deaths had been reported as taking place within an area of ten or fifteen miles square in a few weeks.

As there was no sickness of any kind in my neighborhood, I consented to stay, and for about six weeks was unremittingly employed in visiting the sick, except while paying a few hasty visits to my family.

Some perhaps may be ready to surmise that the disease had abated in malignity about the time I commenced treating it, but that was not so, as was shown by the fact that patients continued to die beyond the circle of my practice. Only a few days before I left the neighborhood, I was called some ten or twelve miles to visit a family, where I found one already a corpse, another dying, a third beyond the influence of medicine, and died in eight hours, and a fourth badly attacked, who recovered. This family had been attended by two intelligent physicians, who, as far as I could learn, had skilfully administered the usual remedies. Dr. Sypert adopted my method of treatment, and was equally success-

ful with myself. Dr. Rose, unfortunately, never regained his health sufficiently to resume the practice, until the disease had subsided. I hereby acknowledge the obligation I am under to both of these physicians, for their gentlemanly bearing toward me while practicing in their vicinity. It was greatly owing to their influence that I obtained access to so large a practice.

While in the neighborhood of Lancaster, I prescribed for more than one hundred and fifty cases of dysentery, many of which were attacks of that disease in its worst forms, and yet but two of my patients died, both of which cases I think were rendered fatal by other causes. One was a negro boy belonging to Wm. Lancaster, about seven years old; had had the whooping-cough very badly for ten or twelve days before taking the flux; found him also full of worms—more than twenty large ones were expelled, and their expulsion produced so much prostration that he was unable to cough efficiently, and died of engorgement of the lungs; the flux seemed to have but little agency in his death.

The other case is remarkable as showing the power of the imagination in determining the issue of a disease. The patient, Miss White, of perhaps thirty years of age, had suffered under a complication of female diseases many years, among which dyspmenorrhœa was prominent. Her mother had died a few days before I was called to the family; three others were simultaneously attacked. I mention these facts in order to show that causes ex-

isted calculated to produce a high state of mental excitement. On the second night after the three had been taken sick, she dreamed that she also took the disease, and died on the sixth day. Before night she had the usual symptoms, and was so fully impressed with the belief that the disease would terminate according to her dream, that it was with difficulty I could induce her to submit to treatment; in fact, it was only in my presence that she did. I arrested the disease several times, but found her as bad as ever on the next visit. And, true to her belief, she died at the precise time she had dreamed.

Before going further into an exposition of my method of treatment in dysentery, I think best to make a few observations on its cause and nature. Every physician to some extent conforms his treatment to his theory, and thus errors in theory beget errors in practice, which I believe has been the case to a most lamentable extent with respect to the disease under consideration. Before, however, undertaking to show what I believe to be its true cause and nature, I will try to remove some mischievous errors upon the subject, the most important of which I consider the opinion that it is of miasmatic origin, and, of course, must be treated as a mere modification of bilious fever. Hence the free exhibition of calomel and quinine, which I am forced to believe have aided the flux in sending more persons prematurely to their final reckoning, than would at all comport with the honor of the profession to be fully known.

For a few years past, this disease has been the source of a greater outlet of human life in the United States than perhaps any other; which, I think, ought not to have been the case, and consider it highly important that the medical profession should look into this matter, and see if there has not been a needless amount of mortality. That dysentery has not a common origin with bilious fevers, I infer from the following considerations:

First. It does not necessarily originate under the same circumstances, or in the same localities. I am aware that I have come in conflict with a host of very high authorities, backed by an array of seeming facts, which I cannot now undertake to investigate, but will honestly and fairly state what I have seen and observed upon the subject, on which my own opinions have been founded, and leave it for others, more competent, to investigate the whole ground.

If I can only be the instrument of eliciting a thorough review of the subject, I shall have accomplished much. I reiterate the assertion, that flux does not necessarily originate under the same circumstances or in the same localities with bilious fevers; that it often does, I admit; and that it is often complicated with them, I also admit; which I presume has been the foundation of the mistake with regard to their being of the same family.

Large and densely populated cities are known to be almost exempt from miasmatic fevers. Can this be said with regard to dysentery? I think not.

In many localities, which are annually visited by miasmatic fevers, dysentery only appears occasionally, and in others not at all. Poor, elevated situations, such as the barrens of Warren, White, and Jackson counties, where bilious fevers have hardly ever been seen, are as subject to dysentery as the Mississippi bottom. During the two past years these counties have been severely scourged.

Second. Dysentery has a different manner of spreading from miasmatic fevers. The latter attack individuals over the whole extent of a malarious district, pretty much simultaneously; but dysentery usually commences at a point, and radiates more or less extensively—often stops within very narrow limits, often spreads in one or two directions, leaving intermediate neighborhoods, similarly circumstanced with regard to the production of malaria, and equally subject to malarious diseases, entirely untouched.

I had an unusually favorable opportunity of observing the manner in which the disease extended itself in the Caney Fork country the present season. The *ridge* runs for many miles almost parallel with the river, at a distance of about eight or ten miles; hollows put out from the river, and run almost directly to the ridge, divided from each other by high sharp spurs, making off from the main ridges, thus forming very marked natural boundaries to each neighborhood. In order to be better understood in what I shall say upon this subject, I will name the several hollows as they are familiarly known in the

vicinity. The lowest one on the river, which I shall mention in this connection, is Dillard Hollow; branching off from the same mouth, is Beller Hollow; about a mile above, is Smith's Hollow; and something like the same distance above this, is Fitch Hollow; two miles farther up, we find Rock Spring Branch; and three miles above that again, we find Wolf Creek, which is as high as my observation extended. I will premise here, that the flux commenced on the west side of the river, where the country is not so well defined by natural boundaries, and where its manner of spreading could not be so well designated; but it soon crossed the river at several points, and we will commence by noting its progress after crossing opposite the mouth of Dillard Hollow. It took up this branch, attacking a few families living in the very lowest situations, but as it ascended, it spread up into the side hollows, and visited nearly all the poor families that had made a lodgment between the hills. It thus continued to travel upward until it reached the main ridge, and immediately opposite the head of Dillard Hollow. From here a detachment took down the ridge westwardly, visiting several families, until it reached a break which makes down toward what is called Hell Bend, down which it travelled, and visited almost every family in the bend. Another detachment took along the ridge eastwardly several miles, and sent off a force down a break, making into Snow Creek, along which I learned it travelled to its mouth; but the main body seemed

to keep along the ridge about a mile farther, where it turned down a break into Smith's Hollow, attacking nearly every family, and more than half the individuals living in it. Now, Beller Hollow lies between this and Dillard Hollow, and is an almost exact counterpart of them, and yet it remained entirely untouched throughout. About the time it crossed the river into Dillard Hollow, it also crossed over and took up Rock Spring Branch, passing by Fitch Hollow, in which there never occurred a single case, except two families living immediately on the river; but it travelled up Rock Spring Branch to its head, not escaping a single family, and assailed a family living on the ridge directly opposite its termination. From here it spread over into Jackson county, beyond the sphere of my observations. Near the same time, or a little earlier, it crossed over and visited Wolf Creek, again skipping over Indian Creek, which remained unmolested many weeks. It assailed every family for about four miles, when it suddenly stopped, and the force diverged; one part taking up a side hollow to the right, and spreading into the flat woods, and the other taking up a branch to the left, passing over a gap into Buffalo Valley, which communicates with Indian Creek about three miles from the mouth of the latter. In Buffalo Valley it lingered many weeks, few families, or even individuals, escaping; finally it extended down to where it communicates with Indian Creek, and spread both up

and down it throughout its whole extent, with rather more than its usual malignity.

I must here say something more about Buffalo Valley, which presents some very striking peculiarities. It is about ten miles long, and wide enough to afford many very fine farms; is the only outlet for the water which falls on some sixty or eighty square miles, and yet it affords no branch, not even a gully can be found, to indicate that water ever runs through it, even in the wet seasons. This is explained by the fact that the subsoil is composed of very coarse gravel, overlaid by a stratum of rich loam and gravel, through which water finds its way as fast as it falls, and is doubtless conveyed to the river through a subterranean passage. Now is this a likely locality for generating malarious diseases? I learned that none had ever been supposed to originate there, and yet here the flux made one of its heaviest sets. I argue, therefore, that it is not of miasmatic origin.

The above is a correct history of the manner of the spread of this disease in the above region of country, as I observed it myself, and learned from those living in each locality. I will now ask, Is this the usual mode of progression observed by malarious fevers? No one will say that it is. The inhabitants of the above country are well acquainted with the habits of bilious fevers. They commence on the river, and extend occasionally some distance up the hollows, but never climb

the *hills*, much less ascend to the summit of the *ridge*. Now if flux arises from the same cause, how can we account for the fact that notwithstanding miasmatic fevers have prevailed along the river more or less every year, yet a case of dysentery was never seen in that country until the past season?

It has now been made evident that flux does not necessarily prevail in the same manner; and I now say, in the third place, it is not ushered in by the same symptoms. There is a great similarity in the first stage, or what are denominated the premonitory symptoms of all general fevers; such as lassitude, weariness, loss of appetite, disturbance of the head, aching or soreness of the limbs, etc.; few cases are ushered in without being preceded by some or all of the above symptoms, which is not the case with flux. It most usually has no premonitory symptoms; the first indication of the approach of this disease is the disease itself, viz.: the peculiar dysenteric discharge. Sometimes a little muco-serous discharge precedes it; but in the great majority of cases which have come under my observation, the very first intimation of an attack was a muco-sanguineous or gelato-sanguineous discharge, attended by more or less tenesmus, a sense of heat and general uneasiness in the lower part of the abdomen, the individual feeling otherwise perfectly well; appetite and digestion good as usual; no disinclination to muscular exertion; and, unconscious of their danger, many continue to eat and

work as usual for a day or two after the attack; general disturbance of the system seldom took place before the second or third day of the disease, and, if proper treatment was instituted in time, very seldom took place at all, which was the case in an attack which I had myself. After partaking of my breakfast with usual appetite, I retired, as is my custom, and had a natural alvine discharge, without any thing peculiar about it whatever; but in half an hour after, in attending to another call, I passed a considerable amount of gelato-sanguineous matter, entirely unmixed with any of the usual contents of the bowels. It was preceded by some slight pains in the lower part of the abdomen, and attended with tenesmus and burning sensation in the lower bowel. As this was the usual mode of attack in the worst cases, I lost no time in resorting to my usual remedies; refused to ride until in the latter part of the day, when, finding but little uneasiness, I commenced my circuit again, felt no more of the disease, except some uneasiness in the lower part of the abdomen, which continued several days. Many other cases could be given similar to this, which served to convince me that the flux is purely a local disease at its commencement, and that the fever which follows is entirely secondary. But with regard to premonitory symptoms, it is true that some cases are preceded by more or less indisposition; but as I could discover no regularity or similarity in such cases, I was led to look upon them as purely accidental, and not necessarily con-

nected with the subsequent attack. I saw a few cases preceded by the common symptoms of bilious fever, and several were evidently complicated with enteric fever. But dysentery is not only not ushered in by the usual symptoms which constitute the first stage of miasmatic fevers, but, in the fourth place, does not primarily or usually affect the same organs. For example, the liver and spleen seem to bear the onus of attack in bilious fever; in flux they are seldom involved, and never, I believe, except when complicated with the former disease. No physician would risk his reputation by pronouncing a case an attack of bilious fever, in which there was no derangement in the functions of the liver; the universality of hepatic derangement in miasmatic diseases has given the great emulgent, calomel, such a world-wide reputation in their treatment; and a natural bilious discharge is as confidently expected to be followed by an improvement in these diseases, as an effect is expected to follow its cause. But in flux, the liver usually performs its office as in health, and a natural bilious discharge, so far from affording relief, invariably aggravates the symptoms. So uniformly has this been the case in every instance coming under my observation, that had it not been necessary for the general health of the patient that the liver should continue its accustomed action, I would have wished its secretions suspended during the continuance of the flux. This satisfactorily accounts for the pernicious effects of calomel in the disease. We all know the grip-

ing quality of what are called *calomel* discharges. They often give much torture in passing through the bowels in a healthy condition; how could we, therefore, expect any thing less than insupportable tormina to be the consequence of their passage over the abraded and inflamed mucous surface in dysentery, and a consequent aggravation of all the morbid associations which have been set up in other organs?

The nervous system often suffers an amount of prostration from this kind of over-stimulation, such as to lead the administrator to conjure up a *congestive chill*, and to continue the drama by the administration of quinine, until its finale is made tragical indeed.

I have said that the spleen is another organ which principally suffers in miasmatic fevers; hence the efficacy of quinine in this treatment, which it is now ascertained acts specifically upon that organ; but it also acts powerfully upon the brain and great nervous centres, perhaps by direct stimulation through the medium of the circulation, producing a most happy effect in the depressed stages of miasmatic fevers, but a most disastrous one in the depression occasioned by excessive irritation in dysentery.

Another organ primarily affected in miasmatic fevers is the stomach: loss of appetite, loathing of food, nausea, etc., are among the best diagnostic symptoms of an attack, which is not the case in flux. The stomach is rarely at all affected at the commencement of this disease; food is taken with

as good a relish as usual, and digestion is performed as well as common. It is only after the local disorder has continued with a sufficient intensity long enough to get up spinal irritation, that the stomach becomes involved, after which it often suffers severely.

RECAPITULATION:—Dysentery does not necessarily prevail under the same circumstances or in the same localities with miasmatic fevers; has a different mode of progression; is not ushered in by the same symptoms; does not primarily or necessarily affect the same organs, and is not relieved by the same remedies; which reasons appear to me sufficiently conclusive that they have not a common cause. Others have supposed dysentery to be the effect of atmospheric vicissitudes; but that any agency operating so universally and continually as these should be the cause of a disease appearing only occasionally and within limited boundaries, appears to me to be very improbable. Neither do I believe it to be *contagious* in any proper sense of that term. I never could trace it from individual to individual. Those who visited the sick did not seem to be more subject to an attack than others who did not. It visited families that lived remote from any others, and it often attacked those members of a family first who had never been from home.

The inhabitants of the two hollows mentioned above, which escaped the disease to my certain knowledge, frequently visited the adjoining affected districts. The only theory which will at all agree

with the history of this disease with regard to its erratic mode of appearing and extending itself is, that it is caused by the poisonous influence of a species of animalculæ, whose habitudes lead them to make a lodgment in the mucous membrane of the lower bowel, which is selected by them as a suitable nidus into which they can burrow and hatch another brood.

It is now believed, if it has not been positively proven, that cholera is caused by an insect; and there are many points of resemblance between it and dysentery, both with regard to their manner of spreading, mode of attack, and the manner in which they affect the system. They differ, however, in the part of the alimentary canal in which the onset is made; the cholera choosing the stomach and upper bowels, and the flux the lower bowel alone. These and some other dissimilarities are the result of the different instincts or habitudes of different species of animalculæ.

It was when on Caney Fork, contending with the flux, and observing its manner of spreading and mode of attack, that I first conceived the idea of its being of animalcular origin; since which time I discover I was anticipated, this opinion having been formed and expressed by others.

No one viewing the disease under the circumstances which I did could resist the above conclusion, after once turning his thoughts in that direction. Its wayward and whimsical mode of progression is consistent alone with the fickleness of insect

migration. The most prominent characteristics of the disease are perfectly consistent with this theory, and may be beautifully explained in unison with it. The first discoverable symptom is that which might arise from the stimulation produced by the titillation of the mucous membrane by the little intruders on first taking possession of the part, viz.: an increase of the natural secretion; but presently they make an entry into the substance of the membrane, destroying its texture and exposing the open mouths of the capillaries, irritate the nervous extremities with which the part is so abundantly supplied, and, of course, get up a state of high morbid excitement—the result of all which is general uneasiness of the parts, discharge of mucus mixed with the blood exuding from the exposed capillaries, and a constant feeling as if something was wanted to be expelled, with spasmodic efforts of the bowel in making these expulsive efforts, etc. This continual teasing and irritation of the sentient nervous extremities, finally convey sufficient morbid impression to the spinal column to get up morbid excitement in it and the other great nervous centres, which is soon conveyed by the nerves to every part of the organic structure, and general disease is the consequence; each organ and tissue complaining according to its peculiar mode: the brain by disturbance of ideas, giddiness, vertigo, stupor, etc.; the stomach by burning, nausea, vomiting, etc.; the kidneys by increased, depraved, or suspended secretions; the skin by con-

striction, dryness, morbid exhalation, etc.; and the whole system by a sense of ill-being. This state of things continues until the enemy is either destroyed, expelled, or has gone through its period of incubation, and is expelled with the mucous membrane: provided, however, that the patient should escape gangrene, or the system should not sink under the pressure of so much morbid action, before the disease has run its course.

That flux is not merely ordinary inflammation of the mucous coat of the lower bowel, I infer from the fact that the common symptoms of inflammation are not sufficiently prominent; they are rather those of irritation, and are not relieved by the means which usually subdue common inflammation. An abstraction of an equal amount of blood, commonly discharged in dysentery, from the capillaries of an inflamed surface, would, of itself, arrest any ordinary inflammation. Blisters, which exert such a powerful control over inflammation, are without benefit, if they are not positively injurious, in this disease. It is therefore evident that the disease of the bowel in flux is of a specific nature, gotten up by the agency of a specific cause; and that that cause is the presence of poisonous animalculæ, is rendered still more probable, from the fact that those means which are known to be destructive to insect life are most effectual in arresting this disease.

This is eminently true with regard to the sassafras. I have experimented pretty fully with this

article with reference to this subject, and have found it to be immediately fatal to all insects to which I have had an opportunity of applying it. It is perfectly amusing to witness its application to a woolly puppy, or a little negro's head. It will clear them of inhabitants in less time than I can tell it. A little applied to my own person has often made an army of *bed-bugs* retreat more precipitately than Bragg's battery ever did the Mexicans.

During the winter, I have carefully examined every author within my reach, and have overhauled old files of medical journals, and have found that the operation of every remedy which I can find to have been used with any considerable degree of success in dysentery, may be rationally explained in view of the above theory. They all seem to perform their beneficial effect in one of the three following ways: either by destroying the animalculæ, expelling them mechanically, or by counteracting their injurious effects, enabling the system to sustain the injury until they run their race.

The most efficient means which has hitherto been generally used, calculated to fulfil the first indication, that is, to destroy the animalculæ, is spirits of turpentine. Many physicians have tested its beneficial operation in flux. But its use is attended by a very serious objection. Every practitioner who has had much experience in this disease, has observed that dysuria is a very common and troublesome attendant, owing to the close sympathy existing between

the lower bowel and the kidneys and bladder. The use of turpentine serves to aggravate this difficulty, and many cases of permanent injury to the kidneys have come under my observation occasioned by its use, both in flux and typhoid fever. As this has become a very fashionable remedy, it would be well for the profession to look a little closer to the result of its improper exhibition. I have had unpleasant effects produced even by its external application, both on the kidneys and stomach, and was led to devise some means of avoiding the difficulty without giving up the use of so valuable a curative means, and succeeded by combining it with nearly an equal quantity of ammonia. In this form it is not absorbed, and its efficacy as a topical application is very much increased. If any one still doubts cuticular absorption, let him try the experiment of making friction on some part of his person with a flannel cloth, saturated with warm turpentine, and if he fails to detect the violet odor on his urine, it will be because he has a *bad cold.*

The peach tree and sassafras are not only more effectual than the turpentine in destroying the animalculæ, but are less offensive to the stomach, and, in place of increasing dysuria, operate as a preventive. None happened in my practice.

Another article which has been much relied on in private practice, and is favorably mentioned by several authors, is sulphur, which, no doubt, owes what advantage it possesses in the cure of flux to

its property of destroying insects. The same is true of camphor, and some other articles of less note. The second indication, that is, to expel the enemy *per force*, is often accomplished by active purgation.

Many of our best writers agree as to its advantage, but differ widely in the use of the means employed. The objection to the use of drastic purgatives in this disease is, that if they fail in expelling the enemy, they do not fail in lessening the powers of the system to bear up under its subsequent depredations.

The third indication, that of enabling the system to sustain the injury until the disease runs its course, will include much the largest class of means which have been used with advantage in this disease; prominent among them are the different preparations of opium, which act beneficially by lessening the irritation in the bowels, subduing the excessive general nervous irritability, and breaking up morbid associations. Next in efficiency to opium are poultices and fomentations, which serve to equalize temperature, and appease the over-excited nervous extremities. An occasional mild purgative is also beneficial, by preventing any accumulation of offending matter in the upper bowels. Many other means have been resorted to, with more or less advantage in fulfilling this indication, but I shall not pursue the subject farther, but will now give my own method of treatment in a condensed and connected form, so as to be better comprehended.

And first, I will give my manner of preparing what I call *compound syrup of peach leaf*. I take as many peach leaves as will fill a ten-gallon kettle, and weight them to prevent their rising, and add water until they are covered, bring it to a boiling heat, and keep it at a simmer for two hours; take out the leaves; strain and settle the infusion; then evaporate by a moderate heat, until it begins to thicken; there will now be about one quart; to this I add five pounds of sugar, and, when it is melted, sup. carb. of soda until it ceases to effervesce; then add one drachm of piperine, finely powdered, and one ounce of oil of sassafras; after stirring them together effectually, and the syrup has considerably cooled, I then add half a pint of brandy. I have usually made use of a good article of American brandy for this purpose. Of this syrup, I give a tablespoonful every hour, until the disease yields. If it produces any unpleasant excitement, I lessen the dose, and always discontinue it during sleep. With regard to injections, what I have said in the first part of this paper is sufficiently specific, and will not be repeated. In most cases, by adding a little morphine to the syrup, a necessity for the use of injections is superseded; the abdomen should, most of the time, be enveloped in a peach leaf poultice, or flannel saturated with the infusion. It is generally necessary once in twenty-four hours to administer a mild purgative, in order to obtain a fecal discharge. I prefer epsom salts to any other for this purpose.

Occasional symptoms must of course be combated by such appropriate remedies as the judgment of the practitioner may decide to be indicated in each case.

I have not found it necessary to be very strict with regard to diet; in most cases the powers of the stomach are but little crippled, and notwithstanding there may be considerable nervous irritability of that organ, yet it generally performs its office of digestion very well, and I found it best to give it employment. I consequently allow mild digestible food to be taken quite freely; even a small piece of fried chicken has often been allowed, much to the gratification of the patient, by removing the nausea, and appeasing the craving of the stomach. No unpleasant consequence ever followed the indulgence. Other physicians often attributed the death of their patients to some improper indulgence in food. Fatal relapses were often said to be produced by this cause, and perhaps justly. But I had no relapses, and allowed my patients great latitude in eating. My course of treatment, by sustaining the tone of the stomach and preserving the general strength of the system, prevented any considerable debility ensuing, and, consequently, convalescence was neither tedious nor precarious. I will here give a list of a hundred cases, taken in connection from my day-book, showing the time each patient required my attendance. Many were visited for a day or two after convalescence, and all were directed to continue the remedies for some days after decided

convalescence, in order to insure its stability. The time embraced in the following list is that in which the patient was sick enough to require being visited. The protracted cases were all complicated with other diseases:

29	cases were	visited only	1 day.
37	"	"	2 days.
14	"	"	3 "
7	"	"	4 "
9	"	"	6 "
3	"	"	9 "
1	"	"	12 "

One chief cause why so few visits were necessary in the above cases, may be found in the manner in which I conducted my visits: my habit was, when called to a new case, if it was a bad one, to tarry with my patient until I had arrested or materially controlled the disease, even if it required several hours' detention; then, by leaving full and specific directions, the case was often managed without another call. In this way I was enabled to attend to many more cases than I possibly could have done by paying flying visits, and repeating them daily.

And though this kind of slow movement often kept new calls waiting until patience was well-nigh exhausted, yet I found that they only had to suffer a few additional hours, as the disease, when once fully formed, usually became no harder to manage for several days.

Since the above period, I have kept myself posted

as to the prevalence of dysentery in that region of country, and I find that a few scattering cases have occurred every summer, within the boundary of its former ravages; but that every year it has prevailed epidemically, beginning at one or more points near the margin of the space over which it had travelled the previous season. During the early part of the present summer it prevailed over a considerable space of country, commencing some miles west of the Caney Fork, and extending in the direction of Nashville. But few escaped an attack, and as it was attended by physicians who obstinately adhered to the use of calomel treatment, the mortality was fearful: over twenty deaths occurred in two families. This disposition of epidemic dysentery to commence one season at a point near which it had stopped the previous year, has been observed and spoken of by others; several instances are given in reports on epidemics in the "Transactions of the American Medical Association;" but as they are not at hand just now, I cannot refer to the page.

I might here close my remarks on dysentery, but think best to make a *resumé*, so as to leave the reader's mind distinctly impressed by the most important points, especially in the treatment. It is much to be lamented that there is still no improvement in the management of this disease throughout the United States; the yearly bills of mortality show this to be so; and physicians generally have not yet learned that the exhibition of mercurials is very generally asso-

ciated with death in typhoid dysentery. There is still no approach to uniformity of treatment, even in recognized varieties, except so far as giving mercurials is universal.

Perhaps no disease is more variously treated in the United States than this. This want of uniformity in its management is no doubt partly owing to a want of correct knowledge of the nature of the malady, and partly arises from the diversity of character which it assumes in different localities and at different times. The particular disease known as dysentery, or *flux*, is undoubtedly a local affection, consisting in a certain amount of irritation or inflammation of the mucous lining of the lower bowel; but this local disease cannot exist long or intensely without involving the general system, manifested in the form of *fever;* and the fever will take on the particular form or type to which the system has been predisposed by atmospheric or epidemic influences; or the fever may have been *first* set up, and the dysenteric symptoms superadded afterward, or both may be developed at the same time by the same exciting cause. Hence, we find flux existing as an independent disease, unaccompanied with any fever; and we find it associated with the different grades of miasmatic fever and typhoid fever; and we see it arising in the course of these fevers; and see these fevers developed in the progress of independent cases of flux. The only sensible way, therefore, of viewing dysentery, is to first consider it as a distinct local disease, and

to contemplate the associating fever also as an independent affection; and then consider in what manner each influences the other.

Dysentery proper consists of a peculiar kind of irritation in the mucous membrane of the lower bowel, exciting a more abundant discharge of the natural mucus of the bowels, attended with a sense of uneasiness, and more or less burning pain and increased contraction of the lower bowel, producing a prolonged disposition to remain at stool, with more or less tenesmus or straining. As the disease advances, the mucus is generally exchanged for jelly mixed with blood. All this may take place, and yet the individual continue in nearly usual general health; having no chills, no fever, no loss of appetite; and, except when suffering paroxysms of tenesmus, no feeling of indisposition of any kind, showing that the disease is as yet purely local. In this form, and in this stage, it is very easily managed. For twenty years I have relieved all such cases in a short time by simply ordering *peach leaf tea* and injections of starch and laudanum. Any part of the peach tree will answer for making the tea, as well as the leaves. In the winter I use the twigs. The dose is about a third of a teacupfull every hour or two; an injection of half a gill of starch or flour gruel, with twenty to forty drops of laudanum, or three or four grains of denarcotized opium, should be given after each discharge. If there should not be fecal discharges produced by the tea, I give epsom salts or Seidlitz powders. If

there is considerable tenderness of the abdomen, I direct a poultice of peach leaves, or, in their absence, a tea of the bark or twigs, thickened with wheat bran. But if the flux is complicated with fever, more than these means, which are all directed to the local affection, becomes necessary to be done. The fever must be met by such treatment as will control it; and a reason why so great a mortality has attended this disease in many parts of the United States, of late years, is doubtless because it was complicated with *typhoid fever*, a disease which, under the ordinary treatment, runs a tedious course; a disease which is attended in its progress with ulcerations of the upper bowels, which, by concert of action, will keep up the dysenteric inflammation of the lower bowel, and both, combined with the depressing nature of the fever, become more than the system can endure, and it sinks under it; and more especially if the depressing influence of mercurials is added. Human nature was not designed to bear up under all these, "and it won't." By the aid of my plan of aborting typhoid fever, this form of dysentery becomes as manageable as any other. All that is necessary in the case is, to add a tablespoonful of the comp. syrup of valerian to a cup of the peach leaf tea, and give a third of it every hour. As the comp. syrup of peach leaf contains the essential properties of the fever syrup, it alone will control it, or they may be given alternately. In a few days the *fever* will be broken up; and even should the dysenteric symptoms continue

a few days longer, the patient will steadily improve in general health. But if the fever should be of the bilious kind, I still do nothing more, as the same treatment will break that up with equal facility, except there should be chills, and then the addition of a few grains of quinine will meet that feature.

The following extracts will show what success has attended my plan of treating dysentery in the hands of others:

Dr. J. E. Fulton, Big Oak, Miss., writes:

"I have had an opportunity of trying your plan of treatment in dysentery, and in my hands it has come fully up to your representations."

Dr. A. M. Walls, Madison Cross Roads, Ala., says:

"I have treated a few cases of dysentery with the peach leaf tea, etc., in all of which it proved successful."

Dr. W. Shapard, Scottville, Ky., writes:

"I have used your treatment for dysentery in my practice with great success."

Dr. W. M. Cook, Silver Springs, Tenn., says:

"I have used your treatment for flux several years, and find it just what you represent it."

Dr. R. Smith, Thebes, Ill., says:

"I am much pleased with the effect of peach leaf tea in dysentery."

Dr. John K. Cypert, Monterey, Tenn., writes:

"I am highly pleased with peach leaf tea in the treatment of dysentery. I have derived great advantage from it, but do not consider it a specific."

Dr. J. G. Boyd, Shady Grove, Tenn., writes:

"Your plan of treating dysentery has been entirely successful in my hands."

CHOLERA INFANTUM, OR SUMMER COMPLAINT.

Children during the second summer often have a hard struggle for existence. The food which nature furnishes for early life being withheld, or proving inadequate to their sustenance, they are forced to depend upon other kinds not so readily assimilated; and not having the teeth or digestive powers to manage properly the ordinary diet taken by older members of the family, and possessing a higher degree of irritability than those of maturer years, the half-digested food often acts as a direct irritant to the coats of the bowels, causing increased secretion and increased peristaltic motion, constituting diarrhœa. Thus the food, instead of being appropriated to the nourishment of the system, weakens it by acting as an irritant, causing the child, as it decreases in strength, to increase in nervous excitability, until some exciting cause, as change of temperature, or the irritation of a protruding tooth, so impresses the nervous centres as to get up general nervous disturbance, which, connected with capillary debility, we have seen constitutes the first stage of fever. Reäction takes place as in other fevers; and as the stomach and bowels in this case are already in a state of morbid excitement, they constitute the weak point, and the onus of the disease is thrown upon them. The indications to be

met in this disease are, therefore, to remove the excessive irritability of the stomach and bowels, to quiet nervous disturbance, restore capillary action, and then improve the tone of the digestive organs so as to guard against relapse.

If the case be taken in time, before *fever* is set up, the treatment is very simple. The diet of the child should be restricted to such articles as are most digestible, least irritating, or subject to ferment—such as boiled milk and stale bread, rice, sago, etc. To improve digestion and restore the tone of the stomach and bowels, I prefer the following formula above any I have tried, viz.: good French brandy, half a pint; pulverized cinnamon bark and nutmegs, each, two drachms; piperin, ten grains; loaf-sugar, a fourth of a pound. Dose, one or two teaspoonfuls, combined with four times its volume of milk, repeated three times a day, or oftener if required; or it may be added to a cup of boiled milk at meals. If the disease be suffered to continue until fever is set up, and there is vomiting, swelling and tenderness of the abdomen, etc., I direct a *lye poultice* to the bowels, and a grain of *calomel* every three or four hours, until bilious discharges are obtained, and then commence the treatment as before directed. Sometimes, however, the fever is truly *malarious*, and, besides the above remedies, requires a few grains of quinine.

Sometimes the thirst in this disease is perfectly intolerable; in which case, though the surface may be cool, we may know there is great internal heat.

I manage such cases by applying a cold wet cloth to the abdomen, renewing it as it becomes hot; for, notwithstanding the cool surface, the cloth will soon absorb the heat from the overcharged internal surfaces; at the same time I administer internally a weak solution of epsom salts, given in small quantities, and frequently repeated. I usually add some sup. carb. soda and a little Dover's powder. My formula is, epsom salts, three drachms; sup. carb. soda, one drachm; Dover's powder, twenty grains, dissolved in twelve ounces of water, of which I direct a tablespoonful every hour. This will be drunk with avidity by the little parched sufferer, and should be continued until the vomiting ceases and the thirst is partially allayed, even should it appear to increase the purging. No other drink, or any thing else, should be taken on the stomach until the vomiting subsides.

The interval should now be extended, and a tablespoonful of new *cream* given between each dose. The cream will not coagulate, and is exceedingly soothing to the irritated lining of the stomach, and affords the most readily assimilated nutriment that can be selected, which the famishing child so much needs. After the patient has become relieved by the above means, the *anodyne carminative*, or brandy mixture, should be commenced, for the purpose of restoring the vital powers. Sometimes the constitutional disturbance attending this disease is not sufficient to produce fever; the child keeps up, retains some appetite, though morbid, occasionally

vomiting, and once, or oftener, in the twenty-four hours, has an attack of diarrhœa; which, from its violence, seems as though it should produce speedy prostration, but it stops of itself, and the little fellow falls asleep, and presently awakes to cry for food. But emaciation goes steadily on, the abdomen becomes tumid, the face and hands puff, and, if the disease is not arrested, the child must die ere long. In these cases I begin with the saline mixture recommended above, and regulate the diet as before directed, usually giving three or four grains of *Hyd. cum Creta* every night. This treatment should be continued three or four days, when it will usually be safe to commence the *anodyne carminative*. A multitude of poor little shrivelled specimens of humanity, whom to wish dead seemed to be benevolent, have by this means been restored to perfect soundness and plumpness.

Dr. J. S. Burford, Whiteville, Tenn., writes:

"I have a beautiful little daughter, now in the enjoyment of the most perfect health, who was brought down to the verge of the grave by that great *baby-destroyer*, 'cholera infantum,' upon whom every means were employed without avail, until treated according to the plan you recommend. It acted 'like a charm.' I feel that I am indebted to you for her life."

CHAPTER IX.

ERUPTIVE FEVERS.

VARIOLA, OR SMALL-POX.

Notwithstanding the discovery of Jenner, which justly immortalized him, through the inattention of the people this dreadful disease almost yearly makes its appearance in our large cities, and along the great thoroughfares. Hence it becomes necessary that everybody, and especially every physician, should be able to recognize the disease, and give it the best treatment that the light of the present age affords.

The forming stage of this disease cannot be distinguished from that of measles, scarlatina, or, in fact, any other fever; it is only when the eruption begins to appear that it can be identified; and here I will hazard a statement, which will run foul of authority—that is, that this disease cannot be communicated until after the eruption is pretty well formed. Much inquiry and close observation have fully convinced me that this is the case; and by

knowing this to be true, much alarm and uneasiness may often be prevented. I am aware the contrary is true of measles and scarlatina, the most contagious period of which is during the preliminary fever. If this were so of small-pox, the disease would, every time it makes its appearance, spread among all the friends and visitors.

When the eruption does make its appearance, it can be immediately recognized by any one who will preserve the type in his mind, it being unlike any other. The first appearance is a few very small and exceedingly red points, scattered over the breast, and next appearing on the face. These points are smaller and of a deeper scarlet red than any other eruption for which it could be mistaken. Twenty-four hours subsequent you will find the number of points increased, sometimes very greatly, and now are more numerous on the face: hence, I suppose, the mistake of authors that it always begins on the face, the few that first appeared upon the breast having been overlooked. By this time the first points that made their appearance have begun to fill with a clear limpid secretion, and have lost their peculiar redness. As these vesicles enlarge, they become surrounded by a circle more or less red, according to the amount of fever which is present.

These vesicles soon acquire a slight depression in the centre; but in the course of four or five days they again assume the rounded form, owing to the lymph giving place to suppuration. They now

are much enlarged, and have acquired an opaque yellowish appearance.

In three or four days more, the pustules begin again to sink in the centre, and often part of the matter oozes out, yielding an exceedingly unpleasant odor, and so peculiar as to be always readily recognized afterward. In five or six days more the scabs fall off, leaving deeply discolored spots, and often unsightly pits.

The disease as described above is known as *distinct* small-pox. Sometimes the pustules, particularly on the face, are so numerous that they run together, making large continuous patches; this is known as *confluent* small-pox. Then, again, the pustules may merely touch without running together, which is called *semi-confluent.*

The stages of this disease are nearly as follows: Period of incubation, or time between receiving the contagion and accession of fever, twelve days; period of fever before the eruption appears, three days; pustular stage, four days; drying stage, four to six days. This is true with regard to the eruption on the face and breast; that upon the body and extremities, by appearing later, will also be behind in all their stages.

From the time that the fever commences until the eruption is fairly out, the patient is commonly quite sick; complains of headache, nausea, pains in the bones, considerable fever, etc.; but when the eruption is fairly out, all these symptoms generally give way very suddenly, and the patient complains

but little, except of the surface, until the suppurative stage commences; secondary fever is now set up, which often runs very high, and in confluent small-pox often causes death.

TREATMENT.—The treatment of small-pox was once perfectly murderous—giving hot teas and stews, keeping the patient shut up in a close room, and forbidding water.

This same treatment is yet followed by some of the ignorant pretenders to whom the management of this disease is sometimes committed. At the present time the received practice is palliative; viz., in the first stage, while there is much fever, cooling drinks and very light nourishment, with occasional spongings of the surface with tepid water, and keeping the bowels gently open with Seidlitz, or a little epsom salts or cream of tartar, and a few grains of Dover's powder at bedtime, to allay restlessness and procure sleep.

When the pustules commence maturating, if the fever is of rather a low type, and the pustules fill tardily, nourishing soups and wine should be given. At every stage of the eruption the patient should be anointed with olive oil at least once a day; and when the matter commences oozing from the pustules, the surface should be thoroughly cleansed with soapsuds, and then oiled. This treatment is generally successful; still, deaths are not unfrequent, and deformity by pitting quite common.

Since adopting my present plan of treating fever, I had rather desired an opportunity of testing it in

small-pox. My opinion was that it would prevent secondary fever, and, by maintaining a vigorous condition of the capillaries, prevent the destructive ulceration of the true skin, which occasions the pitting.

Last winter my desires were gratified. I was called to treat four cases of genuine variola, and a number of cases of varioloid. I felt so confident that I would prevent pits, that I assured the friends that I would do it, if they would attend to my directions. During the vesicular stage I gave my fever syrup in teaspoonful doses every two hours; allowed plenty of cold water, with cream of tartar sufficient to render it pleasantly sour; sponged the surface when hot, and oiled it when they complained of itching. In a few days all constitutional disturbance subsided, and my patients sat up, were cheerful, and had good appetite, which was rather liberally indulged. When the pustular stage set in, I ordered the syrup to be given in half tablespoonful doses, and had the surface well washed with tepid soapsuds every day, and then oiled. No secondary fever occurred, and hardly any inflammatory action; so that the areola was quite indistinct, the pustules, when in their full rounded stage, looking like little balls of cotton sticking on a sound surface; and, as I expected, when the scabs scaled off, there was not a mark left, except three on one little fellow's forehead, who repeatedly rubbed off the scabs.

Now these cases were not naturally milder than common. A negro man who contracted the disease

at the same time and from the same person, and who was not considered worse than my cases, was treated by another physician, and died with secondary fever. One of my cases was partly confluent; the pustules over all the face and breast slightly touched each other, and in several places on the face ran together, making patches as large as a twenty-five cent piece; and yet, when these scaled off, it was perceived that the true skin had not sustained the slightest injury.

I visited the family (Mr. Edmunds', of the lower suburbs of Nashville) about a month since, and found all the children as clear of evidence of having had small-pox as they were before having it, except the little fellow who rubbed his forehead, and who presented three slight irregular marks not at all characteristic of variola. While attending these cases I saw my friend Professor Bowling, and informed him that I intended to cure them without leaving marks. Some time since he informed me that, being in the vicinity, he had a curiosity to see if I had succeeded, and found that I had completely.

SCARLATINA.

Scarlet fever still continues to be the terror of households, and yearly furnishes its quota of bereaved parents. Sometimes its ravages are fearful. Only last winter there lived in an adjoining county a gentleman and lady whose home was made glad by six lovely children; but the destroyer came in

the shape of scarlatina, and in less than ten days they were childless! O! how desolate was that hearth!

This disease has been divided into three grades: *Scarlatina Simplex*, *Scarlatina Anginosa*, and *Scarlatina Maligna*. And though these divisions are rather arbitrary—the different varieties often running into each other and blending in every possible manner—yet, as we often do meet with different cases of this disease presenting true types of each of the grades named, and as these cases must necessarily be treated differently, it is well to preserve these distinctions.

Symptoms.—*Scarlatina Simplex*, or simple scarlet fever.

In this form of the disease there is open reäctionary fever, with an abundant scarlet eruption; skin hot, and pulse full, strong, and rapid. The skin seems to be the principal seat of the disease, the throat not much affected.

The reverse of this is true of *Scarlatina Anginosa:* the throat here is the point of suffering, often swelling enormously, which, with a croupy exudation, which is frequently considerable, and exceedingly tenacious, often produces absolute suffocation.

In this variety, the eruption is rather later in making its appearance, and is not so regularly diffused. Sometimes no eruption whatever takes place, though the disease might appear in the same

family, a part of which had the eruption without the sore-throat.

In *Scarlatina Maligna*, there is profound nervous prostration and great capillary torpor, shown by the purple hue of the surface. The fever is of a low type and very little eruption, and that coming and going as the capillary system varies in activity.

The throat is but little swelled, but is often the seat of foul sloughing ulcers; sometimes the little patient dies in the first stage, assuming many of the symptoms of collapse in Asiatic cholera.

TREATMENT.—In simple scarlet fever, my treatment is exceedingly simple, and uniformly successful.

In the forming stage, which usually lasts two days, this disease cannot be distinguished from the same stage of other fevers, and I treat it accordingly; rubbing the spine with chloroform liniment, and giving the fever syrup in moderate doses, say a teaspoonful every two hours in a wine-glass of sweet milk. I expect by this treatment to allay the restlessness, and get up such a good state of capillary action, that should the disease threaten to assume the anginose or malignant form, it may be converted into the simple variety. It is the business of the nerves and capillaries to manufacture the eruption, and I presume if they are supported so as to perform their office, the disease will always be open and simple.

When the eruption makes its appearance, if it is very profuse, and the heat of the surface great, I

have the surface sponged with tepid water and then greased: a piece of smoked bacon rind is thought to answer the best for this greasing—perhaps on account of the empyreumatic oil which it has imbibed—but any mild oil will answer pretty well.

If there is great thirst, I order small and frequently repeated doses of epsom salts, say what you can lift on the end of a teaspoon-handle, given in as much cold water as the child will drink at one time. If this should operate too freely upon the bowels, it must be discontinued, and two or three grains of Dover's powder given. The eruption will begin to fade in four or five days, and if there have been no complications, the child will speedily recover.

In *Scarlatina Anginosa*, besides the treatment already given, prompt attention must be paid to the throat; the inflammation must be assuaged, and the swelling of the glands reduced, or suffocation may take place very speedily. For the first, I use weak pepper tea, sweetened to the taste, (honey should be used if it can be obtained,) with the addition of two grains of ipecac to a pint of tea: recollect that the tea must be weak; many are in the habit of giving it strong, but this has a tendency to produce distress of the throat by checking secretion, whereas the weak tea merely excites sufficiently to promote secretion. I usually use the cold infusion, made by putting a few grains of cayenne or a pod of common red pepper in a glass of cold water, removing the pod whenever it has imparted a slight pungency

to the water, putting it in another glass of water, to be ready for use by the time the first is used; this drink will be taken kindly, and may be used freely. The throat should be enveloped with flannel, saturated with vinegar which has had as much common salt added to it as it will dissolve. This should be applied as hot as can well be borne, and often renewed; a dry cloth, well oiled or greased, should be applied over the wet one, to prevent cold from evaporation. A teaspoonful of the syrup should be given every two hours in sweet milk, or emulsion of slippery-elm or gum-arabic, to cover its acrimony. As soon as the inflammation is partly subdued, it is best to discontinue the wet cloth, and use some strong stimulating liniment. Either of the following which may be most conveniently obtained will answer: Chloroform liniment; vol. lin.; aqua ammonia and spirits of turpentine, equal parts; or strong vinegar and tincture of capsicum. The bowels should be kept open with small doses of epsom salts, but no active purgative should be given. Under the best management, one or more of the glands often suppurate; when this is found to be inevitable, it is best to hasten the process by applying a lye poultice. After the abscess is opened, a slippery-elm poultice is best; this should always be covered with a greased cloth, to prevent its adhering.

In *Scarlatina Maligna*, the general treatment must be somewhat varied, and local applications wholly different from those recommended for the last variety.

Here we have to contend with deficiency of vital energy, and every thing that is done must have reference to the vital necessity that exists of arousing nervous energy and increasing capillary action. The fever syrup should be given in tablespoonful doses; and if this should disagree with the stomach, carb. of ammonia must be substituted; a good formula is, Emulsion of slippery-elm or of gum arabic, six ounces; carb. am., one drachm; give a tablespoonful every hour until an effect is produced. As a gargle, use tinc. capsicum, one ounce; mucilage, half a pint, and if the patient swallow it, all the better; or chloroform liniment may be substituted for the capsicum. The throat should be enveloped with flannel saturated with cold water—ice water is preferable. If no decided change takes place for the better in a few hours, give the patient freely of brandy and water, and use the *wet sheet*. My habit is to give the brandy until some arterial excitement is gotten up, and then envelop the patient in a wet sheet, cover with one or two blankets, and apply hot bricks to the feet and about the extremities. As soon as reäction has fairly taken place, remove the sheet, and lay the patient between two dry blankets. Recollect we are striving after reäction, and do not want perspiration, and on this account the sheet should be removed as soon as it has accomplished this important office. If the blankets become annoying, change them for sheets. It is very fashionable to cauterize the throat in malignant scarlet fever with nitrate of silver, but I never do it until

after ulcers are formed; it then proves very beneficial, by giving a more healthy character to the ulcers.

Now my experience is, that unless the case be one of those terrible ones in which the system at once collapses under the power of the poison, a free use of the fever syrup in the early stage of this disease will convert a case that would have proved to be malignant, into the simple reäctionary form. I have had no case of malignant scarlet fever for many years, in which I was early consulted, and I suppose I should have had many but for the treatment, as they have occurred yearly in this city and vicinity in the practice of others, some of which died in less than twelve hours. On the contrary, a plain case of simple scarlet fever may certainly be converted into the malignant form by improper treatment. A case in point occurred in my own practice many years ago, which made an impression upon my mind never to be effaced, which, as it presents several points of interest, will here be given.

The patient was my only child, a sprightly daughter of about six years.

The undisputed doctrine then was to stimulate in the eruptive stage, in order to obtain a plentiful crop. She accordingly was covered up, and encouraged to drink freely of warm teas. Under this regimen there was soon rather a surplus crop of eruption, and the heat and restlessness were so great that I was forced to allay them by covering her

lightly, and rubbing her all over with wheat flour. Having obtained a period of comparative quiet, and considering the case doing very well, I withdrew my attention for about two hours. On again examining her, I found that a change had come over the case: her breathing was hurried and anxious, considerable stupor, the eruption rather purple; and on looking into her mouth, a sight was presented calculated to shock parental sensibility to the utmost. The whole lining membrane of the mouth was of a mahogany color, deepening as it approached the fauces, where it had become entirely black, showing that decomposition had already commenced. For a moment I was past thinking; but rallying, I began to cast about for some means which might give a chance. As I had never seen a parallel case recover, I had no satisfactory experience to fall back upon; but the object to be effected was very evident—general reäction must be established, and the gangrene arrested, or death must ensue in a few hours. After administering some unadulterated brandy, and perceiving that sensibility of the glottis and fauces was destroyed, I ventured to put a few drops of undiluted aqua ammonia on the back part of her tongue, which was swallowed with the ease of water; I repeated it every few minutes, each time increasing the dose, until a full teaspoonful was swallowed at once—taking the precaution to give freely of cold water immediately preceding each dose, so as to prevent its irritating the stomach, at the same time that the

full stimulating effect might be received by the dying tissues. In half an hour I perceived a salutary change: general arterial action was improved, capillary action better, manifested by a more florid color of the eruption; and, on examining her mouth and throat, I discovered that the mahogany color had faded considerably, so as to show a well-defined outline to the area of the mortification. I now discontinued the ammonia, and substituted strong pepper tea; this was discontinued in an hour or two, as the returned sensibility of the mouth and tongue caused its pungency to annoy her, and also because reäction was now quite satisfactory. A drink of the juice of green sage, diluted with water and sweetened with honey, was now given.

On the next day I had the satisfaction to see the blackened covering of the fauces cast off, exposing a very healthy but very irritable mucous membrane. Recovery was now rapid and without accident.

The points of interest in this case are: first, that it certainly was *Scarlatina Simplex* at the beginning, but was converted into *Scarlatina Maligna* by improper stimulation, producing excessive eruption, and this causing so much nervous excitement as to produce nervous and capillary prostration. Second, that by powerful stimulation the malignant form was made to yield to the original type, it terminating in the ordinary happy manner of *Scarlatina Simplex*. The third is, that it affords encouragement to persevere under difficulties, and not

give up a case because heretofore we have been unsuccessful. The fourth is, that it illustrates the fact that desperate conditions often warrant the use of what, under ordinary circumstances, would be desperate remedies. At present I would not in a like case depend so exclusively upon ammonia, and should not then, but for the fact that there was nothing else suitable at hand. I had often used the oil of sassafras alone, and in combination with ammonia, for arresting external gangrene, but there was then none in reach. At present I would rely upon the chloroform liniment. I will take the present occasion, as perhaps as suitable as any other that will occur, for saying something more upon the danger of excessive external stimulation producing fatal prostration. We meet with examples of it in all the eruptive diseases; from excessive blistering of young children; from extensive though very slight burns; and among the children of the poor or careless inhabitants of our hot and well-supplied mosquito city, I have frequently been called to visit babes who had been suffered to lie naked in order to keep cool, who presented very alarming symptoms of prostration of vital energy, and after careful examination and inquiry I could find no sufficient cause, except in the fact that the surface was densely covered with mosquito bites. An anodyne internally, with a little brandy as a soothing wash for the surface, have always proved sufficient to relieve such cases. In order to impress this subject upon the mind of my readers, I will

give a short history of a fatal case that I witnessed some years ago.

A gentleman, a mile or two from the village in which I then lived, called on me one evening, saying that there was a case at his house that he did not understand, and came to state it, that I might decide whether any thing was wrong. He informed me that on the previous day a little negro girl had her calico slip to take fire; but as he was near at hand, he succeeded in tearing it off before she was seriously burned; that after the first half hour she ceased to make any complaint of the burn, but, what was singular, had gone to sleep and could not be aroused. I told him that his negro would certainly die, which surprised him exceedingly; he insisted, however, that I should see her, and I did so.

I found that the burning garment had slightly scorched the whole surface, but only in a few small places sufficiently to excite vesication; her pulse was scarcely perceptible, and the whole surface cool; respiration very slow and languid; coma complete. I made some vigorous efforts to stimulate, but to no purpose—the system would not respond, and in two hours she died. Some persons may imagine that my teaching and practice upon the subject of stimulating in the eruptive stage of scarlet fever are contradictory—that I prescribe the fever syrup, which is a powerful stimulant. But I would have such to understand that the comp. syr. of val. is not a *general* stimulant, especially not an

arterial stimulant. Its effects are to quiet the nervous system and excite the capillaries, thereby preventing that kind of morbid excitement which produces excessive eruption. It is true I have said that it is the business of the nerves and capillaries to manufacture the eruption; but if they are preserved in sound condition, they will do this as it should be done, neither half doing nor overdoing their task, and it is the natural effect of this medicine to preserve or put them in proper working order. The operation of the fever syrup in high febrile and inflammatory diseases is graphically and truthfully set forth in a communication from a very intelligent correspondent. After stating its beneficial effects in the treatment of a number of active inflammatory diseases, he states:

"The only inflammation in which your fever syrup is contradicted is gastritis; here it acts as a direct irritant; but by the time it reaches the bowels, it is so modified as not to act injuriously; on the contrary, I have derived great advantage from it in chronic dysentery and inflammation of the bowels; and when it reaches the capillaries, it there sets every thing right again; the pain, irritation, and heat subside, the engorgement disappears, and all move on naturally and pleasantly again."

Now, if it would act as above in active inflammation, is it probable that it would increase febrile excitement? The very opposite is its legitimate effect.

SEQUŒLA OF SCARLATINA.—The diseases which

sometimes follow scarlet fever are often more formidable than the disease itself, especially as they are most apt to follow the mildest cases.

This has been supposed to be owing to the poison not having been properly eliminated from the system, which I think is a rather groundless conjecture. A much more plausible reason is this: the disturbance in a very mild case is not sufficient to produce reäction sufficient to restore activity to the capillaries; and though capillary debility seems to be slight, yet it remains, and as the patient is not sick enough to be kept in-doors, some exposure renders the capillary debility more intense, and it runs into active inflammation, or the debility becomes profound, and produces such a state of relaxation as will admit the watery part of the blood to exude into the cellular structure, or upon the serous surfaces, causing general or partial dropsy. With this view of the subject, the means of relief naturally suggest themselves. Give the fever syrup freely in watermelon seed tea, and promote the secretion of the bowels and kidneys with salts and cream of tartar. Under this treatment the dropsy will speedily disappear.

D. C. A. Moses, M. D., Eldridge, Ala., writes:

"You say nothing in your work of using your fever syrup in dropsy. Now, if you never have used it in such cases, give it a trial—it will not disappoint you. The cases that I have used it in are such as are recognized by the profession generally as incurable, cases of long standing in the old and

feeble, and which depend upon general atony of the whole system."

The dropsy following scarlatina, we have seen, depends essentially upon debility; therefore the above is in point.

As to the inflammations, glandular swellings, etc., which sometimes follow *scarlatina*, if they be treated upon the general principles laid down in this work, or according to enlightened common sense, there will be but little difficulty in managing them; though patience may become a virtue before a complete cure is obtained. After the prominent disease has been pretty well managed, chalybeates are often of great service.

RUBEOLA.

Much that has been said under the head of scarlatina is equally applicable to measles, and need not be repeated. Its diagnostic symptoms are cough, suffusion of the eyes, irritation of the mucous membrane of the nose, fauces, and air-passages, causing sneezing, running at the nose, cough, etc. These are attended with more decided symptoms of fever than ordinarily accompany common catarrh; but nothing positive can be known until the eruption appears, which will be about the fourth day of the fever.

The eruption appears in form of small red points, mostly grouped into circles or semicircles, of about a line in diameter; these gradually coalesce, forming strawberry-colored blotches, slightly elevated above

the surrounding surface; they mostly appear first on the face, extending to the breast, arms, body, and lower extremities, in order; they also invade the mouth, fauces, and, at times, the whole respiratory tubes. The blotches are usually entirely distinct, having more or less surface between them of a natural color, but sometimes they become confluent on the face, breast, and upper part of the arms. After the eruption fully appears, it relieves, in a great measure, the sickness, which at first is very annoying, but does not give much relief to the pulmonary disturbance, and the fever has no tendency now to abate, as in small-pox. In five or six days, both the fever and eruption begin to decline; the tongue, which had been intensely red, begins to assume a more natural appearance, the appetite returns, and the surface casts off its epithelium in the form of scales.

Diarrhœa is a frequent attendant on this disease, and, if not very severe, proves salutary.

Treatment.—In most cases of measles, very little should be done; bland drinks, unstimulating teas, such as balm, wild sage, etc., with two or three teaspoonfuls of the fever syrup in the course of the day, are all that is required. Nothing should be given to "strike out the measles;" no efforts will succeed until the proper time arrives, and then you cannot keep them back if you try. But injudicious efforts to bring out the eruption, often do much harm, by increasing the crop to a dangerous extent.

If the fever be very high, and there be great heat

of the surface, tepid sponging may be resorted to, and two or three grains of ipecac. added to a glass of cold water or mild tea, and the patient directed to take a swallow at short intervals. If the patient, on the contrary, is rather cool and restless, a few pretty full doses of the fever syrup, with a few grains of Dover's powder, will bring them to the right point. After the eruption is out, unless there are complications, I do nothing except to give the fever syrup in small doses; the best manner of exhibition is to add a teaspoonful to a glass of cold water, and allow them to drink that quantity every two or three hours. The syrup undoubtedly moderates the fever, and prevents all accidents from damp or cold, which are so often met with, and which are often serious; given in the above form, it enables the patient to drink a sufficient amount of water to supply the wants of the system without offence to the stomach, or injury any way. If the cough is very troublesome, and pulmonary distress considerable, add two grains of ipecac. and five of Dover's powder to a glass of mucilage, and give a tablespoonful every half hour.

Occasionally the brain becomes deeply involved in this disease, and requires very prompt attention. In slight cases, a cold wet cloth to the forehead will be sufficient; but I had a case, a few years since, which required the whole head to be enveloped with pounded ice, which had to be continually applied for nearly forty-eight hours; the disease then yielded, leaving no unpleasant results.

To allay excessive itching and burning of the surface, wash and grease as directed for scarlatina. Great care should be taken, during convalescence, to avoid cold and exposure. If any unpleasant sequœla follow, they must be treated upon general principles.

RECAPITULATION:—Time after the contagion is received until the disease begins to manifest itself, about fourteen days; forming stage, four days; eruptive stage, ordinarily, seven days; under the use of the fever syrup, from three to four days.

Can the contagion be prevented from taking effect? I think it can.

For nearly thirty years I have been in the habit of using a means for this purpose which, so far as ever came to my knowledge, has never failed; this is the odor of turpentine. The manner in which I use it is to saturate a woollen cord, or, what is better, a slip of buckskin, and apply it around the neck, so as to fall pretty low down, resting upon the under garment and being hidden by the outer; it must not touch the skin, or it will irritate the sensitive surface of a child excessively.

The first time I saw this remedy tried was on a crowded steamboat on the Ohio river. A case of measles occurred, and there were many unprotected persons aboard. I remained with the most of these long enough to know that the disease was not contracted. Since then, I have always recommended mothers to use this precaution when taking their children to camp-meetings and other public assem-

blages, when the disease was known to be in the vicinity; and so far as negative evidence can prove any thing, trials enough have been made to settle the question as to its power.

CHAPTER X.

CONCLUDING REMARKS ON FEVER.

THERE are many observations which the author would like to make, which did not seem appropriate to come under any of the preceding chapters, and others which, though appropriate, were forgotten at the proper time, all of which he proposes to bring out under the above latitudinous heading, and perhaps will contrive to group the most of them, if not all, under a single section, giving a condensed view of the various steps by which the author gradually departed from the strict antiphlogistic doctrines in which he was educated, and arrived finally to adopt the theory and practice set forth in this work.

He thinks he can make this subject interesting as well as instructive to his readers, as it will give him occasion to bring into notice many curative means which may yet be found of advantage to fall back on for sake of variety, or when the articles now most relied on are not at hand, or are inadmissible from idiosyncracy, or some other cause.

In Harrison county, Ohio, where the author was

brought up, and in a large part of that section of the State, almost the only form of fever from which the inhabitants suffered was what was known as continued bilious fever; sometimes a case of inflammatory fever occurred, but never *chills and fever;* there were some chills on the Ohio river, and they were frequent among the marshes of Still Water and some other sluggish tributaries of the Muskingum river, but the author had lived to study medicine without ever having witnessed the phenomenon of a chill. Now this bilious fever was a very formidable disease, and carried off a quota of our neighbors every year; it was ushered in with great headache, distressing sickness of the stomach, restlessness, etc., soon followed by an exceedingly hot skin and full, rapid pulse. The fever would slightly abate towards morning, but there was never any thing like a well-marked remission; it usually continued with little variation for several weeks, if the patient lived that long.

The subjects were mostly persons in the vigor of life, and principally young men. The prevailing practice of the day was to bleed freely at the start, then a vomit of emetic tartar, followed by free purgative doses of "calomel and jalap." On the next day the patient was usually again bled, and means at once put in requisition to produce salivation, as it was thought that no case could recover that was not mercurialized; but this was no easy matter to accomplish, as the secretions seemed all to be locked up, which, with deprivation of water, (which was

thought to be exceedingly dangerous to partake of,) often caused a dryness of the mouth and cracking of the tongue awfully distressing. Yet the doctor persevered under difficulties to push the mercurials, occasionally bleeding, often blistering until but little whole skin was retained by the sufferer, and vainly trying to allay the raging thirst and excite moisture, by giving sp. nit. and broken doses of emetic tartar. Sometimes the mercury did produce a local effect upon the mouth and salivary glands, producing a *dry salivation*, as it was called, which generally pretty soon eased the patient's sufferings in death; or, if he survived, terminated in deep sloughing ulcers, often ending in permanent mutilation. Well, this could not be avoided, said the doctor, as the mercury could not be brought to produce its legitimate effect. But generally, after this system of practice had been pursued until the patient was emaciated, prostrated, and apparently about to succumb, the fever abated, moisture returned to the mouth, and pretty soon a profuse salivation followed, with all its untold horrors; but then, had this not been produced, the patient would certainly have died; so said the physician, and so thought the people; therefore there were no hard thoughts if the patient, after a tedious convalescence, got about with a permanently shattered constitution.

This was the form of fever and mode of treatment with which I was familiar in my youth, and so was I taught to practice by Cullen and other writers, while studying my profession; and thus did

the popular physicians practice under whom I studied.

I was practicing in the neighborhood in which I was raised in the year 1830. This fever was very prevalent that year. I had a large practice, and treated my patients upon the above general plan, but contriving many small means of comfort, which of themselves were but trifles, yet served much toward lessening the patient's sufferings, and increasing the chances of the main remedy, calomel, to produce its legitimate effect; consequently, I was rather unusually successful, but still occasionally lost a case; and it continually occurred to me that there must be something wrong in the practice that would suffer the strong man to die. About this time, two men in different sections of my practice were attacked with this fever in its usual form; and as they had lost some of their friends under the management of physicians, and some others suffered more than death from blistering or morbid salivation, they resolved to trust to the efforts of nature, and take no medicine at all. As both were well known to the author, and lived in the line of his practice, along which he passed almost daily, he had a curiosity to watch the cases through their progress, and consequently often called; the disease pursued the usual course, lasted the usual time, and then terminated by a return of secretion, just as all others did that recovered. I observed that they suffered much less than those put through the usual course of treatment; they did

not become so emaciated, and retained more strength, and had a speedier convalescence. The only means used in these cases was plenty of cold water drank at will, often applied to the head, and occasionally, when the heat was great, to the whole body. I was now fully convinced that all my skill amounted to nothing, and that nothing was better than all I had been in the habit of doing. But should I abandon my profession, or should I seek to improve it? I resolved to aim at the latter. I saw that the course pursued was too exhaustive, and assisted the disease to wear out the patient's energies. Now I thought if I could find means that would accomplish the various effects which we all aimed to produce, that would not of themselves produce debility, I would be able to assist nature in sustaining the force of the disease, and aid in throwing it off. My knowledge of the virtues of our indigenous plants was now of service to me. This knowledge had not been originally acquired from books, but from observation and experience. In my early boyhood, doctors were "few and far between," and our mothers from necessity turned doctors. The author very early manifested some capacity and showed an inclination to wait upon the sick, and was soon installed as nurse in the family, and frequently acted as such among his near neighbors, always following the instructions of his mother, in whose skill he had great confidence—who, by the way, was a woman of much good natural sense, great prudence, and untiring energy;

was, in short, in the proper sense of the term, a "strong-minded woman," and yet enjoys a green old age of over four-score and ten.

In this way the author acquired a pretty good acquaintance with all the most useful domestic remedies, and it was now to this source that he turned in search of means by which he hoped to manage fever without causing exhaustion.

Among our indigenous medicinal plants, the boneset (*Eupatorium Perfoliatum*) was deservedly held in high repute; I knew it would act as an emetic, when given in form of a strong infusion, and administered freely; I also knew that it afterward usually produced active perspiration; having also tonic properties, it left the system invigorated. So the next attack of fever I was called to treat I managed as follows: Finding a full pulse, throbbing carotids, great headache, etc., I thought I dare not omit bleeding, and perhaps it was best not to; after abstracting twelve or fifteen ounces of blood, I administered half a pint of a very strong infusion of the boneset, and repeated in fifteen minutes; the second draught was soon followed with free emesis, throwing off considerable bile; a weaker infusion was now given, until I supposed all the bile then in the stomach was discharged; near a tablespoonful of Bateman's drops was now administered, for the purpose of quieting the stomach, and exciting perspiration. I also gave ten grains of calomel to carry off the bile from the bowels, and be ready to act by the time the sweating process should be over.

Perspiration was kept up pretty actively several hours, by frequent draughts of infusion of *wild sage*, with a grain of ipecac. to the pint.

After getting my patient fairly under the sweating process, and directing a tablespoonful of epsom salts administered in the morning to work off the calomel, I left quite pleased with the plan thus far. On my visit next day I found my patient much better, but there was still considerable fever; but the headache and restlessness were much less. I directed a moderately strong infusion of the boneset, with two grains of ipecac. to the pint, a tablespoonful every hour until the patient sweat or vomited, and then to be followed by the Bateman's drops, calomel, and wild sage tea, as on the day before. On my third visit I found my patient quite clear of any manifestations of disease; there was very little prostration, appetite returning; directed a bottle to be half filled with equal quantities of dogwood, wild cherry, and yellow poplar bark, and then filled with rye whiskey, (it being made in the neighborhood, it could be always obtained genuine,) the patient to take a wineglassful three times a day, to prevent relapse. This I looked upon as a great achievement—a case cured in forty-eight hours, which under my former treatment would probably have lasted two or three weeks at least; and this without a sore mouth, and with a whole skin.

My after experience was equally satisfactory. Some cases would linger a few days longer, and some required the aid of other means; but whatever

was done, the main point was kept in view, not to exhaust the patient's energies. Nothing was said about this new practice, and, therefore, the author escaped the odium of an innovator, and the opposition of his professional brethren. About all that was said was, that "Thompson was very successful, and didn't give much strong medicine." The author pursued this practice for about six years, when his health failed from excessive labor, so that he thought it advisable to seek a milder climate, and accordingly came to Tennessee. In the course of a few months his health so far improved as to warrant him again to commence practicing in a small way. Here he met with cases of the same grade of fever with which he had contended in Ohio; but there was a much greater tendency to chills; but as quinine had now been fully introduced, he found but little difficulty in the management of that feature.

But he was quite at a loss concerning his favorite remedy, the boneset; it could not be found in the vicinity, and the dried article obtained at the druggists proved to be quite inert; so he was forced to seek a substitute, which he found in the vervine, a plant growing in almost every yard; he found it a more stimulating emetic than the boneset, but not quite so active; therefore a few grains of ipecac. were added; he also found in the scabish, a white-blossomed weed infesting small grain and clover fields, a most valuable sudorific. It is equally as powerful as the boneset, and much pleasanter, and

less apt to nauseate. I now rarely bled, but still thought it essential to vomit, and after the vomiting my habit was to give ten grains of calomel with three of Dover's powder, and then commence with the infusion of scabish, adding a teaspoonful of sp. nit. dulc. to each draught if there were much heat of the skin; this was continued for several hours, and then followed with the infusion of wild sage, which I could always find. I also became acquainted with another exceedingly mild and pleasant sweating remedy—the queen of the meadow. This is a most noble-looking plant, growing often fifteen feet high, with a stem an inch and a half in diameter; but it is rather a rare plant, being only found in rich bottoms untrodden by cattle; but, as the stem was the part I used, an inch of which being sufficient for a pint of boiling water, a single stalk would suffice for a year's practice. The tea made from this plant has hardly any taste, and when cold, is taken by patients freely during the highest fever, when any of the other remedies named would be contra-indicated, or too repulsive to be borne by the stomach. The tea of this plant, though destitute of taste, and apparently of medicinal virtues, yet often causes the fever to gradually abate, and a pleasant moisture to succeed. Now all this time the author had no theory upon the subject, further than that fever is a depressing agent, and requires to be met by means which will sustain the vital energies. About this time, what is known as congestive fever was first seen by the author, the

phenomena of which impressed him still more forcibly with the truth, that the cause of fever acts as a depressing agent. The same mode of treatment during the febrile stage, and large doses of quinine, after a remission had been obtained, enabled him to succeed very satisfactorily with this fever. But a few years later, he met with another form of fever, which brought him to a stand-still. He had heard, for a year or two, of a fever prevailing in a distant neighborhood, which the physicians could not manage; it was said to go through whole families, and that very few recovered. One gentleman, who had a wife and several grown children with him, besides quite a number of negroes, was in the course of a year left alone, except a few old servants. Finally, I was called to a case which at first showed nothing peculiar, except that though the symptoms appeared to be quite mild, and the patient complained but little, yet my usual remedies did no good, and the disease went on as though nothing had been done. I soon found that my treatment was doing positive harm—the sweat was exhaustive, and ended in prostrating diarrhœa. I abandoned sudorifics and resorted to blisters and calomel. At one time I thought I had succeeded: the liver acted well, and there was some appetite. Soon another case occurred, a brother of the first; I commenced treating him with broken doses of calomel, and occasionally a teaspoonful of salts. Both went on, alternately giving good hope of success, and then arousing all my fears, until death put an end to the strife.

Now it was clearly evident, upon a quiet retrospect of the cases, that I had done no good, and I believed had done harm. I therefore resolved, had I other cases, to do nothing, and wait for light. Some five or six other cases occurred in the family, which, upon the do-nothing system, all finally recovered.

I said I did nothing, but that is not strictly true. I had the surface sponged with tepid soapsuds whenever it was too hot, and the bowels poulticed when they were painful, suppositories of opium used when there was diarrhœa, and broken doses of epsom salts when they were costive. I never improved upon this treatment until about ten years ago. During the long rides I frequently took between my home in Wilson and the Caney Fork country in Smith, where I was contending with epidemic dysentery, I carefully reinvestigated the whole subject of fever, and sought to analyze it, so as to ascertain what was essential, and what secondary or incidental. By this process, I ascertained that there were but two things which were always present; the absence of either was also the absence of fever; these are, as my readers know, nervous disturbance and capillary debility. How all the other symptoms of fever grow out of these, and how my present mode of treatment was naturally suggested by this theory, has been fully explained in the preceding pages.

This theory was often investigated, and the treatment proven by varied experience to be perfectly reliable, and of general application; for as it at-

tacks fever by removing the condition in which it consists, causing the febrile movement to subside, as water ceases to flow when the supply is cut off, I expected that *typhoid fever*, taken at a stage before inflammatory complications were formed, would yield to its power just as other fevers, and so it did, every case yielding in a few days. Now it is very improbable that I should have been mistaken in diagnosing the cases—I, who for many years had, with great care, noted the first symptoms of the disease, in order to avoid doing mischief by improper medication, and thus had become able to detect the disease in its very earliest stage, with very rare exceptions. I was not mistaken. Others have not been mistaken, and have succeeded as fully as I claim to have done. A few others, it is true, have not been so successful, but these only form the exceptions to the rule, *that typhoid fever treated upon the plan laid down in this book, when taken in the incipient stage, or before inflammatory complications are set up, will yield in a few days, leaving the patient very little weakened or injured in any way.*

Heretofore the author had kept his peculiarities of practice so much out of view, as to have attracted little or no attention. He now felt it to be his imperative duty to give the benefit of his improved plan to the world, how much soever he might personally suffer. His conclusions were, that as thousands had voluntarily sacrificed even their lives to procure a supposed good for their country, he could afford to die a moral or rather a medical death,

especially with a strong hope of a speedy resurrection. He accordingly removed to this city, and immediately embraced every opportunity that offered for promulgating his doctrines, and he has been more fortunate than he expected; he has now enlisted a host of true and worthy friends in favor of his views, into whose hands their support and defence could be safely trusted, were he removed from the field, or retire to recuperate.

Now all the long years in which he quietly pursued the stimulating plan of treatment in fever, he considered himself as much alone in the world as did Elijah, when he had fled from the destroyer of the Lord's prophets; but, like him, he now finds that he was mistaken—that a remnant scattered all over our wide country have been, without concert, acting upon the same great principles, and adopting a practice corresponding to their views. True, none had gone beyond my conclusions arrived at thirty years ago, that fever was a depressing power, and had to be met by appropriate stimulation.

Dr. S. A. Cartwright, then of Natchez, avowed this doctrine, and advocated the use of the peppers, many years ago, though unknown to me; but even his great name did not shield him from an overwhelming torrent of ridicule, which deterred him from making another hopeless attempt to bring his brethren to a knowledge of the truth. But I commenced at a more fortunate era; typhoid fever had forced the more prudent part of the profession to abandon extra debilitating measures, at least in that

disease, so that, as I have said, this is a transition period, in which the tendency is to adopt the use of stimulants; and, very lately, this has shown itself among the high-priests of our profession in London and Paris. A controversy of considerable length, upon the propriety of bleeding, and other decided antiphlogistic remedies, in febrile and inflammatory diseases, was carried on some time since between the savans of London and Paris. The principal champion of the opposition to the antiphlogistic treatment was M. Lois, of Paris, and his strongest opponent, Dr. Alison, of London; but the opponents of the lancet, and other exhaustive measures, having truth on their side, soon discomfited their opponents, so that even Dr. Watson, who had taken an active part in the controversy, surrendered—reserving, though, in his capitulation, the position, that epidemic influences had so depressed the vital energies of the people, that their diseases are not now of the same grade as formerly, being of a lower type, and therefore less tolerant of venesection, etc.; but that when a case does occur of the former high grade, the same active depletion is called for as in former years.

But M. Lois would not leave even this plank for him to stand on, but showed most conclusively that diseases had not materially changed, and that exhaustive remedies had been all the time as injurious as they are at the present. But Dr. Watson not only yielded the main point, but, in doing so, has been guilty of such gross contradictions as would

sink a common fame; their effect upon him is, however, yet to appear. Hear him and judge. He says in the latest edition of his work, "Years have passed since I have seen any instance of the disease (pneumonia) which has required phlebotomy. I may say much the same of inflammatory diseases in general. They have all, as I firmly believe, been less tolerant of blood-letting since the cholera first swept over this country in 1832."

Now hear what Dr. Watson said only three years before, (1855:)

"The *great* instruments to be employed in the *treatment* of inflammation of the lungs, are the same which have so often been recommended by me in other inflammatory affections before—blood-letting, tartarized antimony, mercury. Of these, blood-letting is the chief."

"The late Dr. Gregory, of Edinburg, was in the habit of saying in his lectures, that, provided he was called *early* to a case of pneumonia, he would be contented to dispense with all other aids than those of a lancet and water gruel. I am far from desiring you to believe that blood-letting is the only expedient required; but certainly the amount of the best experience, ancient and modern, is strongly in favor of its *free* and, I might almost say, prodigal employment. Very lately, one most distinguished French writer, M. Lois, has endeavored to show that venesection has not much control over the progress or event of pneumonia; and I advert to his opinion on this subject merely to

caution you against being misled by it, as you might otherwise be, considering his well-merited reputation as an exact and faithful observer."

Now, every man has the right of changing his opinion upon evidence, and it is his privilege to avow this change, or say nothing about it, as prudence dictates; but for a teacher of medicine by whose authority many thousands of practitioners regulate their practice, and upon the soundness of whose teaching the safety of millions depends, for a long series of years to pursue one plan of treatment in his own practice, and at the same time instruct others to pursue a different treatment, and one, too, which he now publicly repudiates as dangerous, betrays a reckless disregard of human interest which amounts to nothing short of deep moral turpitude; or, if we choose to disbelieve what he says with regard to his practice in pneumonia and other inflammatory diseases, and put it upon the ground that he said this merely for the purpose of breaking the force of his fall, and for the purpose of claiming the credit of having seen and acted upon this supposed change much anterior to his compeers, then we must admit that he possesses small conscientiousness, and as little judgment, allowing himself to be betrayed by his vanity to make statements which his own writings so easily prove to be untrue; or that he has been knowingly a *false teacher*. But enough of this; it is unpleasant; but my conscience would not suffer me to say less, as to this man thousands of physicians in our own

country are still looking, as to "the law and the prophets," and having only the older editions of his work.

It is evident that opposition to exhaustive measures has become the popular doctrine in Great Britain and France at least.

Hear how some of England's most popular writers stand upon the subject. Bennet, as is well known, was one of the earliest and most efficient champions of the new practice; he maintains that "antiphlogistic remedies in general, and blood-letting in particular, are unsuitable, and even hurtful, in all acute inflammations." Tanner, in his deservedly popular work on the Practice of Medicine, talks thus: after enumerating the objects to be accomplished in successfully treating inflammations, he asks, "How are these desirable results to be obtained? For very many years but one answer has been given to this question; viz., by the adoption of the *antiphlogistic regimen;* which consists essentially in the use of low diet, blood-letting, counter-irritation, active purging, mercury, and antimony. It is to be feared that these remedies still find favor with some practitioners; but I cannot help thinking that the more closely disease is studied, the smaller will become the upholders of these antiphlogistic agents."

Authorities could be multiplied. The above are sufficient to show that authority is now in opposition to exhaustive treatment, even in the high latitude in which it originated. Of course it is still

worse borne in this land of excitement, where excessive nervous excitability is more common than vigorous vital energy; and especially in the South and West, where all are more or less brought under the depressing influence of *malaria*.

So now I am no innovator, and may perhaps shortly be denied even originality. New doctrines or new discoveries are generally scouted by the masses, but, if they work themselves into favor, somebody finds out that somebody else hinted at the same thing long ago, and presently the cry is, "It is but an old affair newly vamped up;" and many will even try to persuade themselves that they had all the time some such notions, and others will push themselves forward as its especial champions, treating the subject without allusion to the source from which they obtained their light, and thus all credit for originality may be filched from the rightful possessor. Well, it is the way of the world, and we all have to submit to its decisions.

But little more, however, has yet been accomplished upon this subject in Europe, than to "cease to do evil;" they have not yet "learned to do well:" have not yet given any satisfaction as to how the debilitating treatment does harm, nor have they introduced any efficient treatment in its stead. Now in this, at least, I claim to be ahead: I have shown, by plain, sound philosophical reasoning, why depressing agents do harm, and made it evident that they always did and always will do harm.

APPENDIX.

CHAPTER I.

SOME FEMALE DISEASES.

OF THE USE OF THE EXTRACT OF HYOSCYAMUS AND OIL OF SASSAFRAS IN SOME OF THE MISFORTUNES ATTENDANT ON PREGNANCY.

VERY shortly after commencing the practice of medicine, by a favorable state of circumstances, I got introduced into a pretty extensive practice of midwifery; and was soon led to regret the want of more efficient means for relieving many of the ills which are often associated with the pregnant condition than the medical science at that time furnished. Opiates, it is true, afforded alleviation; but generally at the expense of some other form of suffering or disadvantage, such as checking the natural secretions, disturbance of the head, etc. On one occasion, while waiting upon a tedious case of labor, I amused myself, along with the matrons

present, in the enjoyment of the pipe rather freely, and suffered a good deal of vertigo as a consequence. In the course of the conversation which this incident gave rise to, one of the company observed that the dry bark of sassafras, combined with tobacco, would effectually prevent its unpleasant effects upon the head. I laid this up in my mind, and on the first opportunity made the experiment, and found it eminently true; the sassafras not only preventing the injurious effects of tobacco, but speedily removing them when produced. I tested this repeatedly, by smoking in a strong pipe until my head was very disagreeably impressed, and then reloading with a mixture of sassafras bark; a few puffs of which invariably dispelled all unpleasant sensations. I had now satisfied myself that sassafras was an anti-narcotic, so far as tobacco was concerned, and resolved to test its powers upon some other narcotic stimulants, and first selected the hyoscyamus. I added a drop of oil of sassafras to every two grains of extract of hyoscyamus, and made it into pills by the assistance of flour, and tested it first upon myself. Being very susceptible to the influence of nervous stimulants, I began by taking one common-sized pill, and increased the dose until I took five at once, without producing any other effect than a most delightful sleep—such as I had not enjoyed since, when a child, I used to fall down under the shade of a tree when tired at play. I now believed I had obtained the desideratum for which I had been wishing; and experience

fully verified my anticipations. It acted like a charm in soothing the excited nerves, and saved my parturient patients of nearly all suffering, except the necessary throes of labor. For some time I was uncertain whether the narcotic property of the henbane was wholly counteracted, or only lessened by the sassafras; but a mischievous little girl solved this question for me. Her mother being pregnant, and suffering much from costiveness and erratic pains, I made a syrup of butternut, to which I added sixty grains of hyoscyamus and thirty drops of oil of sassafras to the half pint, and directed a table-spoonful to be taken often enough to keep her comfortable. Her little daughter, seeing her take it frequently, supposed it was, of course, something good, and, in the absence of the rest of the family, managed to get hold of the bottle, and finding it sweet, drank all that remained, which was over a gill, and contained at least thirty grains of hyoscyamus. Her mother was at first frightened, and sent to the field for a boy to go after me, but, seeing no immediate unpleasant effects from the medicine, concluded to wait a while. The child, after a little, got into a crib and fell asleep, and slept quietly and naturally for about three hours, when the cathartic effects of the butternut aroused her. No injurious effects followed. I was now fully convinced that the sassafras rendered the hyoscyamus entirely innocent, and have ever since given it in just such quantity as to secure an immunity from suffering. I never pushed it to the extent that the

child did but on one occasion. In 1835, when the cholera was prevailing so fatally on Round Lick Creek, in Smith county, while at Mrs. Hearn's, where there were two corpses in the house, and one in the kitchen, and several more in a dying condition, a stout negro man, while walking across the yard with a spade on his shoulder intended to be used in preparing graves, was attacked with such violent cramps as to cause him to scream out. The spasm was of the clonic kind, making his lower extremities and body as rigid as if frozen. I happened to have a bottle of the above syrup in my pocket, which I had prepared for a lady who had been confined the previous evening, and whom I intended visiting in my round, and immediately gave him about four ounces, containing at least forty grains of hyoscyamus. In a few minutes the spasm relaxed, and he assisted all day in burying the dead. I don't give this as a case of cholera; I did not consider it as such; and yet it doubtless was produced by the joint influence of cholera and fear. I did give it, though, in genuine cases of cholera, and always with the effect of relieving the spasm, provided the stomach retained it a few minutes; but, unfortunately, it was often rejected before it had time to produce any impression.

Having ascertained the controlling power which the remedy was capable of exerting over many forms of disease arising from morbid innervation, and looking upon most cases of abortion and premature labor as originating from that cause, I ex-

pected it to prove valuable in their treatment, and was not disappointed when I brought it to the test of experience. I have now used it in all cases of this kind happening in a pretty large practice for about twenty-five years, having more calls than usual in the same amount of general practice, my success having given me some notoriety in that line; and I recollect of no case of failure, where I was called in previous to the occurrence of considerable expulsive uterine contraction. But as *cases* are more impressive than mere general observations, I will, in a very concise manner, give a few of the most prominent which have been treated with this remedy.

Case 1.—1835, Smith county, Rev. Charles Ledbetter's negro woman, aged thirty; commenced bearing children at fifteen; had five in quick succession, after which she aborted five times between the sixth and seventh month. Commenced giving the medicine, viz., the hyoscyamus and sassafras, at the middle of the sixth month, and continued it six weeks. She went to the full period, but had a dead child; after which she had four living children.

Case 2.—1840, S. Berdine's negro woman, aged thirty-five; had two children when very young, and had aborted nearly every year since, between the third and fourth month. Commenced giving the medicine as soon as she was known to be pregnant, and gave it until the period of quickening. She went her full time; had a fine living child. A

year after, again became pregnant; gave the medicine with the same result, after which she ceased breeding.

Case 3.—1843, Mrs. S—— married at fifteen; had two living children, then three miscarriages in succession about the sixth month. Commenced the medicine a while before the catastrophe was expected; continued it four weeks; did well, and continued afterward to bear living children.

Case 4.—1845, Mrs. D——, Wilson county, married young; had a living child, which died; after which she aborted every year for seven or eight, about the third month. Commenced the medicine as soon as pregnancy was known, and continued until after quickening; had a fine child at the full time; three years after, had another; both still living.

Case 5.—Mrs. C—— married at fourteen; had two children; after which she had severe flooding about every six months for four or five years. She did not consider them abortions; said they came on a week or two after she ought to have been *unwell*. I requested her, in case of another attack, to save all that passed until I could be sent for; which she did. It presented the appearance of blood only, but upon putting it into a vessel of water, I discovered something like an organized mass, and placing this in a fresh basin of water, I had presented the finest specimen of an embryo I ever saw. It was about the size of the chick on the eighth or ninth day of incubation, and very much resembled

one. There was an umbilical cord of about two inches in length and the size of a wheat straw, and a placenta, perhaps two and a half inches across the disc, presenting on the maternal surface a most beautiful flocculent appearance while floating in the water. Gestation in this case could not have existed more than thirty-five days. I gave her a supply of the medicine, and directed her to commence taking it immediately after missing a menstrual period. She did so, and next year had a living child.

Case 6.—Thomas Edwards' negro woman, aged about twenty-five, commenced breeding quite young, and had miscarried nearly every year since at about the fifth month. Commenced giving the medicine as soon as she felt the child, and gave it regularly a month, and afterward, whenever she felt pain in her back. Went the full period and did well. Has since had another.

Case 7.—A sister of the last, belonging to the minor heirs of E. Hearn, was similarly unfortunate; had had many miscarriages about the third month. Commenced giving her the medicine as soon as pregnancy was known, and continued it until quickening. Did well.

Many other similar cases could be given, but these are deemed sufficient. It will be observed that in all the above cases, child-bearing was commenced quite young; and my recollection is, that all were of a lax fibre and nervous temperament. In the treatment of the above, and similar cases,

the form of the medicine was varied to suit the inclination of the patient—some preferring that of pills, and others a syrup; and the quantity was also proportioned to their susceptibilities. Other remedies were also added, to meet such indications as presented; usually nothing more, however, than a little blue-mass when the liver was torpid, or butternut when there was slowness of the bowels. To sum up the purposes for which I give the above remedy: I give it in all cases of threatened abortion, when not caused by accidents or severe sickness. I use it for all the nameless pains, aches, and disquietudes attendant on conception and gestation. I give it to prevent, and to remove when present, premature and erratic pains in the latter stage of pregnancy. In fact, in all cases in which I am previously spoken to, I put the patient upon its use a week or more before the expected confinement, for the purpose of removing any excessive nervous excitement of the general system, especially of the os uteri; thus preparing it to yield kindly to uterine contraction; and after delivery, I give it to soothe the excited system, and prevent those spasmodic contractions called after-pains. I know that many consider these pains to be necessary and salutary, but females with first children, when the tonic contraction of the uteris is good, do not have them; and I also know that in after-labors they have, in my practice, done very well without them.

Case 1.—I will now give some cases of a different character from the above. In January of 1849,

I was called on by a Mr. B——, who had lately moved within six miles of me, and with whom I had had no previous acquaintance, who wished me to cause his wife to abort. He informed me (which I afterward found to be correct from other sources) that his wife had, the previous year, come to the full period of pregnancy, and that although he had the services of several experienced accoucheurs, they were unable to deliver her without literally taking the child to pieces, and that they informed him that his wife never could bring forth a living child. He also informed me that two of her married sisters had to have their children delivered in this manner.

I consented to visit his wife, telling him at the same time that I did not expect to procure abortion. I found her a fine-looking lady, rather short for her weight, large muscular developments, and flesh unusually firm. I made an examination per vaginam, and found, indeed, but very little space, owing principally to the fulness and firmness of the soft parts. But my conclusion was, that there was bony room sufficient to admit of the passage of a common-sized head. I accordingly positively refused to procure abortion, but expressed myself confidently that I could put her upon a regimen that would enable her to have a living child. This was about the third month, and I requested him to visit me on the seventh for further instructions; for the present I recommended nothing more than a teaspoonful of epsom salts daily, to keep her system cool. He called at the time directed, and I

sent tinc. of valerian, with vinous antimony sufficient to slightly nauseate the stomach, which, besides other benefits, would enable her more effectually to carry out my injunctions of low diet. The valerian being a good diuretic, would serve to keep up a drain from the kidneys, and I directed the salts to be taken daily, in such quantity as would secure a moderate purgative effect on the bowels. I also sent a box of the pills of hyoscyamus, to be taken freely during the last stage of gestation. At the full period I was sent for, and was pleased to find that my plan had worked well. There was still a good deal of *embonpoint*, but the textures were all soft and pliant; and, on examination, I found a fine dilatable condition of the soft parts. In short, after a common labor, she was delivered of a sprightly common-sized child. It was characteristic of the family to have large children; and my plan of treatment contemplated a retarding of the growth of the fœtus, as well as a softening of the textures of the mother.

I had often witnessed this effect follow daily purgation, when brought about for other purposes. This was the first instance in which I availed myself of this means intentionally.

Case 2.—Mrs. A——, wife of Mr. A——, of Nashville, a respectable English lady, had been married over twelve years, and had never borne a child sufficiently matured to live; had generally miscarried between the sixth and seventh months, sometimes between the third and fourth. These

misfortunes finally undermined her health, and the usual symptoms of uterine, hepatic, and gastric derangements became prominent, and finally, in the fall of 1852, she presented the symptoms of jaundice in marked form. She obtained the advice of several scientific physicians, but failed to receive more than partial and temporary relief; and for several months had run the round of irregulars and nostrum-venders. At certain intervals her stomach would sympathize so strongly with the deranged liver and uteris, as to throw her into most intense suffering; and during one of these attacks I was consulted. I found her with great tenderness of the epigastrium; frequent retching to vomit; distressing pain in the right hypochondrium, with evident enlargement; extremities cold, with skin the color of a very dark orange; discharges from the bowels like white pipe-clay; urine almost black. She informed me that her skin had been the present color, with slight variations, for seven or eight months; and that during that time there had not been the least trace of bile seen in the discharges. Treatment: a mustard bath to the feet; a large blister over the liver; a blue-pill every six hours; and a tablespoonful of the comp. syrup of valerian every two hours. On the morning of the third day I had the pleasure of witnessing the following favorable symptoms: distress and anxiety entirely subsided; pain in the right side gone; copious dark bilious dejections from the bowels; free discharge of natural urine, etc. From this time her recovery

was rapid; in a week her skin was clear, her tongue clean, and her spirits buoyant. But in order to prevent a relapse, the syrup was continued three times a day for several weeks. I now informed her that in all probability she would soon become pregnant again; but that if she would inform me when the event happened, and put herself under my treatment, I thought I could enable her to go through the period of gestation and bear a living child. Accordingly, in a short time Mr. A—— informed me that he believed his wife was *enciente*, and on visiting her I was convinced that he was correct; I therefore immediately put her upon the use of the following formula:

℞ Ext. hyoscyamus, ℥j.
" butternut, ℥vij.
Oil sassafras, ℥ss.
Sup. carb. soda, ℥ij.
Simple syrup, qt. ii.

I directed her to use just enough of this to keep up a regular condition of the bowels, and to prevent all irregular action of the uterus. Under this treatment she got along very finely until about the middle of the seventh month, when there was an evident disposition manifested by the uterus to expel the fœtus. Believing that the organ, having formed a habit of expelling its contents prematurely, would not bear much further distension, I thought best to make an effort to arrest any further development of the fœtus; and having succeeded in producing this effect in several other cases in which I thought it advisable, by a free use of this medicine, I

directed her to increase the dose, so as to produce tolerably free purgation. This effect was steadily kept up during the remainder of the term, and no more unpleasant symptoms occurred. At the full term I was called on, and, after a rather tedious but not difficult labor, delivered her of a fine, well-formed female child, weighing five pounds. The mother and child both did well. In about two years Mrs. A—— again became pregnant, and, being now in good health, thought it unnecessary to apply for medical advice; nothing unusual happened until about the sixth month, when labor-pains suddenly came on; the fœtus was expelled. Six months subsequent, she had the usual evidence that conception had taken place, and applied to me for aid. I placed her upon the same treatment as on the former occasion, giving the comp. syrup of butternut freely, after the sixth month, with an occasional blue-mass pill, with a view of retarding the development of the child, which it did as before, and with a like happy result. Both children are now living, one five, and the other two years old.

Dr. Moses, of Eldridge, Ala., writes:

"Your comp. syrup of butternut for the relief of females I have used frequently, and with not only the happiest results to my patients, but also to myself, getting me a reputation, and large practice among that class of patients."

Query: Would we not be justified in placing females under this treatment who habitually bear unusually large children, and consequently endure

severe suffering? So far as I have had experience, the treatment is entirely compatible with the best health to the mother, and safety to the child; for, although small, they have uniformly been vigorous and healthy, and grew finely.

SLIPPERY-ELM.

I have no intention of writing a history of the slippery-elm, but merely to refer to some new preparations of it which I have lately made, which I think are both convenient and useful, explaining the mode of their preparation, uses, etc.

Of the medicinal properties of the slippery-elm I need say nothing—they are known to the profession, and to the people; it is simply a cooling demulcent, and has been used from time immemorial as a soothing external application to inflammations, and as a cooling drink in internal irritations, etc. The value of the slippery-elm poultice to inflamed surfaces is well understood, but it is subject to two objections: in many positions it cannot be easily applied so as to be retained *in situ*, and is always disgusting to the nice and fastidious; then it is subject to dry at the edges, and adhere with great tenacity, often causing as much harm in its removal as it did good in its application. To avoid these objections, I conceived the idea of combining it with some non-irritating substance which would not easily dry, and making it into cloth, so as to be of easy application and not subject to dry and adhere; and also avoid the opprobrium of nastiness. Chinese

sugar-cane molasses seemed to possess the required properties, and I tried it and succeeded. By first wetting the pulverized slippery-elm bark with water, so as to make a tough mass, and then adding the molasses until it became thin enough to spread, I succeeded in obtaining the article desired. But I found this was not easily done, so as to present a smooth, even surface; after many efforts, however, I succeeded in the following manner: I spread smooth domestic on a flat non-absorbing surface, and spread the mass on it with a broad spatulæ as well as I could; I then laid another piece of cloth over it, and continued my efforts until I obtained the requisite finish; then dried it in the sun. Afterward, by wetting the surface, the cloth will readily leave the composition, and it presents the appearance of thick, wet parchment, and can be applied in a moment to any part of the body by simply wetting one side, and has the great advantage of adhering with sufficient tenacity to retain its position, and yet will never dry so as to become hard. It is better to allow the covering of cloth to remain until it is wanted for use, and then only remove that on the side intended to be applied to the inflamed surface. As many physicians would not wish to be at the trouble of making this preparation, however simple, I have invented machinery by which I can have it done with expedition, and will furnish it to the druggists as the demand may require.

Slippery-elm has been long occasionally used as

a bougie; its soothing properties, united with its great powers of expansion by the absorption of water, naturally pointing it out as a suitable material for this purpose. But the great difficulty was to obtain it of a suitable form. This I effected in the following manner; viz., I split the bark into fine slips, and then, when moistened, pressing them together, and wrapping them thoroughly with twine, and then drying. But this method, though practicable, is troublesome, and will often fail—the fibres not equally adhering. I have made a die by which great pressure can be applied, and the mass made to unite as perfectly as though it grew in that form. I make them perforate like a catheter.

It is hardly necessary to dilate upon the advantage of this instrument; it operates more expeditiously, and with less suffering, than any other appliance of which I have any knowledge. Any amount of dilation can be obtained by it. I have no doubt that the urethra can be dilated sufficiently to admit of the removal of small stones with ease. In stricture of the urethra its use is signally beneficial; it can be introduced when no other bougie or catheter can be passed. I will relate a case in point.

A negro man, belonging to a Mr. Wade, nine miles below, came to the city with his master's team; had had gonorrhœa, and a stricture remained, but had not prevented urine from being voided. But fatigue, and perhaps stimulation, had caused irritation, and his urine was suppressed. An intel-

ligent physician was called on, who exhausted all his skill in vain endeavors to pass the smallest catheter; when I saw him he was in horrible torture, and had instinctively put himself in a reversed position, his feet upwards. I also attempted the use of the catheter after a full loss of blood and an opiate, but without success. I now rode back to my office for a small slippery-elm bougie which I had a few days before prepared, and with but little effort passed it into the bladder; after letting it remain a few minutes I withdrew it, for the purpose of using a catheter; but there was no occasion, as the urine flowed freely, and soon relieved the bladder.

A great advantage of the slippery-elm bougie over every other in efforts to enlarge the urinary passages, besides its soothing properties, is, that when made perforate, it can be allowed to remain until the full effect of its expansion is obtained without having to be removed in order to allow the urine to pass. Would it not be the most suitable catheter to leave in the urethra after the operation of lithotomy?

The following interesting case was read by the author before the State Medical Society, April 7th, 1857:

CASE OF UNRUPTURED HYMEN AFTER TWELVE YEARS ENJOYMENT OF CONNUBIAL PRIVILEGES.

Mrs. ——, about thirty-five years of age, of usually robust health, had been happily married over twelve years, but having failed to obtain *pledges of love*,

consulted me as to the cause. Upon inquiry I ascertained that the catamenia had always been regular as to recurrence, but continued an unusual length of time, always becoming dark and fœtid toward the termination, and, contrary to what is observed in dyspmenorrhœa, there was no pain or constitutional disturbance at the commencement of the period, but these symptoms invariably supervened after a few days, becoming more intense toward the close, often extending through the second week, so that the period occupied near half the entire month.

As these symptoms were rather peculiar, I judged there was an abnormal condition of the reproductive organs, and suggested a vaginal examination. To this she objected; but, after several unsuccessful efforts to correct the condition of things, finally consented, when, to my surprise, I found the vagina terminated in a complete *cul de sac* about two and a half inches from the vulva. As the walls felt perfectly smooth and elastic, giving no evidence of cicatrization, and she had never suffered vaginitis, and especially as she averred that the parts had always been in this condition, I felt assured that it was a case of *Unruptured Hymen.* As the catamenia had found a place of exit, I knew there must be an aperture, but for a long time was unable to detect it. The speculum showed a smooth, unbroken surface; the *tactus euriditus* failed to detect any prominence or indentation which might indicate the point of connection between the divisions of the

vagina. But after fully failing to discover where it *probably* was, I turned my attention to where it *possibly* might be; and finally established its position about midway of the parietes of the vaginal sac, near an inch behind the *meatus urinarius*, just where I should at first have looked for it, but was misled by the hymen having been thrust upward until it apparently formed a part of the true vaginal walls. Into this opening I succeeded with some difficulty in introducing a small silver probe, which, having passed the barrier, played quite freely within, showing that there was plenty of vagina above.

This condition of things fully accounted for the symptoms attending the menstrual period; for as the discharge found an exit with difficulty, it collected in the posterior part of the vaginal cavity above the septum, and was there retained until partial decomposition took place, giving rise to the pain, fœtor, and nervous disturbance attending the latter part of the menstrual period. The first idea that suggested itself was to divide the septum; but as it had proved itself possessed of sufficient solidity to successfully resist the shocks incident to the *wars of Venus* for so long a period, I feared that its division might be attended with troublesome hemorrhage and constitutional disturbance. But feeling assured, by carefully watching the play of the probe, that the parietes were quite thin immediately below the aperture, I cautiously thrust a narrow bistoury by the side of the probe, turning the cutting edge from the bladder, making an aperture sufficient to permit the

passage of a common-sized silver catheter. But although the cutting occasioned no pain, and was followed by inconsiderable hemorrhage, yet, as I was satisfied that the walls of the septum increased greatly in thickness immediately below the incision, I desisted for the present. A few days subsequently considerable cerebral disturbance came on, whether from the incision, or from some other cause, I cannot say, but it made me fearful of again resorting to the knife, and I laid the case before my friend, Professor Bowling, who suggested that a trial be made to dilate the aperture by the use of slippery-elm bougies. As the plan appeared to be feasible, I immediately put it into practice, and with the most happy and entire success.

Commencing with one of the size of a crow quill, I daily replaced it with one slightly larger, until in a few weeks one of an inch and a quarter in diameter could be introduced with facility. Believing that this was sufficient for all present practical purposes, I concluded to trust to *natural processes* for further development, which proved to be quite adequate to the task. Although the menstrual function has since been performed with great facility, and without pain or nervous disturbance, and the uterus clear of disease, yet the main object of the operation has not yet been obtained, viz., fecundation. This may be owing to the fact that the *os tincæ* had found a resting-place in the posterior portion of the sac, above the septum, and still remains imbedded in such a way, that it does not correspond

to the plane of the vagina, which *error of position* may probably yet be remedied. But though the maternal desire has not been gratified, the shortening of the catamenial period, and the freedom from suffering which has been secured, amply compensate for the trifling inconvenience which attended the cure.

While manufacturing the bougie, it occurred to me that this would be the very material of which to make *pessaries*, provided I could succeed in giving it the proper shape, which, after much trouble, I succeeded in doing; since then, I have succeeded in making them with less trouble, and also greatly increased their virtues by suitable medication.

My usual plan and formula is as follows: To one pound of flour of slippery-elm, I add four ounces of pulverized sassafras bark; and of Dover's powder and balsam copaiva, each, two ounces; add water slowly, and work the mass until it acquires the consistence of stiff dough; then roll it into balls of from an inch to two inches in diameter, and put them in the sun or under a stove to dry; when they begin to feel hard, I moisten them, and after some hours, manipulate them into shape, and, then dry again; if they crack, or assume a bad shape, I again moisten them, and repeat the manipulations until I obtain the necessary shape and smoothness. I have lately had machinery constructed, by which they can be made with considerable rapidity, and much neater and firmer than can possibly be done by hand, and expect to keep a supply with the druggists.

It is hardly necessary for me to offer any explanation of the *modus operandi* of the above medicated pessary in the cure of many female afflictions, or to give evidence of its usefulness, as both will be perceived in a moment by every one at all acquainted with the materials used, and the maladies in which the pessary is indicated. But this pessary is of advantage in a variety of cases in which other pessaries would not only do no good, but produce positive injury. I have concluded, therefore, to give a hasty sketch of the various ills for which I have used it with advantage:

Case 1.—Soon after having made the first slippery-elm pessary, I was called to see a lady who was suffering from retention of urine, an affliction from which she had suffered much during the early months of two former pregnancies—her physician having to visit her daily for weeks, and use the catheter; but as he could not be found on the present occasion, I was called in. Now it occurred to me that my pessary was the very article indicated in this case; and, after giving present relief by the catheter, I introduced one, and informed her that it would probably relieve her from any further annoyance, which proved to be so.

Case 2.—Soon after this, I was called to see a negro woman at Boyd's sale-house, who had fallen down stairs, she being in the seventh month of pregnancy. Manifest symptoms of miscarriage soon occurred, attended with great soreness and tenderness of the abdomen. I ordered a poultice to the belly,

and introduced a medicated pessary; in a few hours the urgent symptoms all subsided, and she was up the next day. But about a week subsequently, I was hastily sent for, and she informed me that she had miscarried, but that it was no child—she passed it too, she said, without pain; upon examination, I found it to consist of the pessary I had introduced, greatly enlarged; she had not been aware that I had introduced it all. She afterward brought forth a fine child at the full time, and did well.

Case 3.—Soon after this, a stout negro woman was brought to the sale-house from Georgia. She had come all the way in the cars without stopping; her courses being on her, and the nights cold, suppression was the consequence, attended with great pain, acute tenderness, and considerable tumefaction of the whole abdomen, and high symptomatic fever. I gave her a purgative of castor-oil and turpentine, ordered fomentations to the belly, and introduced a medicated pessary. General relief followed very speedily.

Case 4.—A lady who had suffered much for a number of years with neuralgic affections and bronchitis, and who had had several premature deliveries, in consequence of the uncontrollable cough and nervous pains, always increased by reflex uterine sympathies, was now in her eighth month, and imminently threatened with miscarriage from the above causes. As she had been under the direction of several skilful physicians, who had exhausted all the usual remedies, I resolved to try the power of

the soothing and anodyne properties of the medicated pessary, supposing that, as the lungs were pathologically sympathizing with the womb, I could make them do so curatively. It acted beyond my expectations, giving complete relief.

Case 5.—A negro woman belonging to Captain Walton. Had dyspmenorrhœa; had been married for a number of years, but remained barren; her health continuing to decline, until she was incapable of attending to business; had *fits* at each menstrual period. Found the womb as large as in the third month of pregnancy, feeling as hard as a ball of wood, and very heavy; great tenderness of the cirvex, which was of a deep mahogany color, with some epithelian abrasions, profuse flocculent discharge, etc.

Introduced the medicated pessary, and put her upon sarsaparilla and iodide of potassium; introduced a fresh pessary every week, until three had been used. The uterus had now become soft and of natural size, tenderness all gone, general health much improved; has remained pretty clear of suffering, and has had no *fits* for over six months; attends to business as other negroes; had one spell of suffering since, which was soon relieved by the use of the pessary. In this case there was not much prolapsus.

Case 6.—A negro woman belonging to J. B. White. Had suffered with ulceration and prolapsus of the womb many years; had been frequently treated by different physicians, with only temporary

benefit. She came under my care about eight months since. I examined with the speculum, and found the womb very low—much enlarged, and considerable abrasions; she was generally dropsical; feet and legs much swollen; I used the pessary, and put her upon sarsaparilla and potassium, as in the fourth case. Her general health improved rapidly, and there has not been much complaint of the womb disease since, except on one occasion, when she had very imprudently exposed herself, and was suffering great pain in the back and abdomen, simulating labor-pains: the pessary gave her prompt relief.

Case 7.—A negro woman belonging to Mr. Harris, of this city. Had sent her to the State Hospital for treatment nearly three years ago, where she remained about two years; during this time, I examined her several times, at the request of the superintendent, Dr. Wharton; her womb was very low, and greatly enlarged, quite hard, and adherent to the vaginal walls; cirvex nodulated, and frequently extensively ulcerated; most of the time suffered great torture, so that full doses of opiates had to be resorted to, to give partial relief; was fully treated, according to Bennet, with leeches, scarificator, caustics, etc. Her disease would at times apparently yield, but about the time she would be thought ready to leave the hospital, would suddenly return with renewed force. When Dr. Wharton left the hospital, he said to me that he did not know what would become of Clara, as she had been

already a great expense to her kind master, and was not yet fit to be removed from the immediate eye of a physician. I suggested, that if Mr. Harris were willing, I would take her to my house and attend her there; he saw Mr. Harris, and the arrangement was made. I immediately used the medicated pessary, *and nothing else*, except an anodyne at her monthly period; she soon improved so as to become useful, and has, for the last several months, done the entire duties of our kitchen. I used perhaps half a dozen pessaries in her treatment; she still complains, at times, of erratic pains, but her general health is good; the womb is still rather low, having formed adhesions to the walls of the vagina, but has become accustomed to its position, and tolerates it.

Such cases could be detailed unto weariness that have yielded to this remedy, but it is not necessary; a mere suggestion of the instrument, also suggests to the mind of the intelligent physician its advantages. It is the very thing we wanted, a means that will hold the womb in place, and at the same time subdue irritation, remove hardness, allay fever, and promote the healing of ulcers.

A few words perhaps should be said as to my motives for selecting the particular articles which compose my medicated pessary. The slippery-elm is the *base*, and without it the thing could not be made at all; but without the sassafras, the pessary would be a *nuisance*. A few hours' confinement in an inflamed vagina, and absorbing its hot, vitiated secre-

tions, will cause such a decomposition as to convert it into an irritating fœtid mass; but the sassafras prevents this; it effectually stays decomposition, and removes fœtor if already present. I distinctly stated this property of sassafras when treating on dysentery, published in the Nashville Medical Journal in 1853, and transferred to this book.

The balsam capaiva is added to the pessary because of its known power over inflammation of the mucous membrane. Its action in gonorrhœa is believed not to be specific, but general; and it has been reported, by good authority, to cure when applied locally, by injection alone. I have long used it as one of the best means in bronchitis and chronic dysentery, and have lately apparently cured several cases of gonorrhœa in the female, with no other means but the medicated pessary and injections of tepid water. The Dover's powder is introduced for the purpose of relieving present sufferings, but it acts not merely as an opiate—it possesses other powers as a topical as well as an internal remedy.

As regards the application of the pessary, there is but little skill required; patients, with a little instruction, will do that themselves. It is only to be remembered, that it must be dipped in water *immediately* before insertion; a few minutes' delay will make it tenacious, and of difficult introduction; the cup-side must be applied to the os uteri, and the woman directed to remain in a recumbent position fifteen or twenty minutes; by that time the pessary has

adjusted itself to the uterus, so as to adhere with considerable force, and there will be no danger of its displacement, even should it be very small, and the vagina morbidly dilated. On this account I find it unnecessary to vary the size to suit the capacity, as with other pessaries. It is best, in all cases, to let the pessary remain until it begins to disintegrate. If there is abundant secretion, this will take place in a few days; and if there is great dryness of the vagina, it is best to use tepid injections occasionally, so as to make the pessary act as a poultice.

CHAPTER II.

SOME DISEASES OF CHILDREN.

I CAN only notice a few of the diseases incident to infancy and childhood on the present occasion, and these are noticed because I think they have not been well understood by systematic writers, or that I think I have something new to offer that is valuable upon the subject.

STROPHULUS—RED-GUM—HIVES.

There is a certain disease well known to mothers and nurses, and country practitioners who have been some years in the practice, but the young physician may consult his standard works on practice in vain for any satisfactory description. Hence, he will most probably deny the existence of any such disease as *hives*, except as another name for croup; and yet he will presently be called to treat a case, occurring within the first month after birth, which he sees at once is not croup, but is an equally formidable disease, and will most probably baffle all his skill, the infant dying in a few hours from strangulation. The women tell him the child died

of *hives;* and as it *died*, it was the *bold hives*—which name, fortunately for his reputation, saves him from all blame, as the general belief is that bold hives always kills.

Upon a careful examination of authors upon this subject, I find the mild form of this disease is considered as a variety of red-gum, while the graver form is confounded with croup. Dr. Wood so treats it, describing the mild form very accurately, as a variety of strophulus or red-gum, a general description of which, including this, he gives as follows:

"In all its forms, strophulus is, with few exceptions, an acute disease, generally beginning to exfoliate and disappear in one or two weeks, and seldom continuing longer than a month. It is very rarely attended with fever, and often occurs without any observable disturbance of health. When febrile symptoms do occur, it is not always certain whether they are essentially associated with the eruption, or proceed from some other cause. Strophulus is almost always an innocent complaint. Its retrocession is thought sometimes to have been followed by serious internal irritations; but it is very difficult always to determine, in such cases, whether the disappearance of the eruption is cause or effect."

Now, from this description, we would naturally conclude that the disease was of not much moment any way; not more important than itch, nettle-rash, milk-scab, etc.; and yet, if you will inquire of country practitioners of what disease the most children die under six months, they will say *hives.*

I have said *country practitioners*, for my observations have led me to believe that the disease is much more common and more fatal in healthy country situations than in cities; for this, a reason will be given hereafter. I have so frequently seen this disease present symptoms not described in the books, and assume a character so much more grave than is assigned it by writers, that I have been almost ready to doubt its being the same malady; but its milder forms being the same as described, I have concluded that some writer, centuries back, who had never seen the disease in its worst aspects, wrote a description of it, and others have merely copied without investigation. For some years past, I have embraced every opportunity of interrogating practitioners from every part of our country upon this subject, and have ascertained that they all are familiar with the disease in its gravest forms. I therefore take it for granted that it is a much more formidable disease throughout the world than agrees with the descriptions in the books.

The only cause of difficulty in this disease referred to by authors, is when the eruption is repelled from the surface, and the irritation transferred to the stomach and bowels. But I have never known any very serious consequence arise from this cause; the means they *all* prescribe, will usually meet the difficulty very promptly; viz., a warm bath, a gentle emetic, and a few testaceous powders. It is only when the irritation extends to the *air-pas-*

sages that the disease becomes really dangerous; and this form has been wholly overlooked, or confounded with croup, by every writer I have had an opportunity of consulting. The eruption is not always repelled in these cases, neither do I believe that the irritation is even transferred from the surface to the air-passages, for I have often seen the rash plentifully on the skin, at the same time that the infant was threatened with suffocation from a filling up of the air-tubes by a thickening of their coats, and a secretion of tenacious mucus. It is true that the rash always fades before the child actually dies, because the capillary action has then become too low to sustain it; but a *striking in* of the eruption is not the cause, nor is a *transfer*, but an *extension* of the irritation from the surface to the air-passages. Hence the means recommended by writers for the relief of cases in which the disease has been transferred to the stomach and bowels will, in this, prove of no avail, but may do positive harm; for the means that will increase the eruption on the surface, will at the same time increase the irritation in the mucous lining of the air-passages, and, consequently, increase the morbid secretion which is already threatening suffocation. Whoever has tried nauseants and the ordinary warm bath in this disease, when there was considerable eruption on the surface, and the child breathing hurriedly, and auscultation detected very decided mucous rhoncus, has presently ascertained that he had gained nothing by the treatment, but injured

his patient. For although nauseants, if carried to *vomiting*, may for a time appear to have produced benefit, by disgorging the phlegm which was obstructing the bronchia, yet the formation of it is actually increased by the measure, and the little sufferer will presently breathe with more difficulty than ever. A repetition will obtain a still shorter respite, at the expense of a still more abundant secretion; and if the practitioner now resorts to the warm bath, with a hope of succeeding, he will find the advantage obtained equally deceptive. While the child is in the water, the inhalation of hot vapor will, for a time, expand the lungs, and render the morbid secretion more fluid, so that the air finds a readier passage into the air-cells, and breathing is consequently easier; but the bath is rapidly exhausting nervous power, which will soon reduce the vital action of the capillaries, disqualifying those of the lungs still further for returning to a healthy condition, and lessening the ability of the respiratory apparatus for taking on such action as is necessary for freeing the lungs from the accumulating mucus; while at the same time the action of the bath upon the surface, by direct sympathy with the mucous lining of the bronchia, increases the amount of the secretion which is wanted to be thrown off. We see, therefore, good reasons why these measures which are mainly relied on, and which are so uniformly successful in relieving a transfer of the disease to the stomach and bowels, should not only fail to afford relief when the disease has extended

to the lungs, but aid it in its too certain victory over the vital powers; and the attendant will very soon have the mortification to see his little patient turning livid from a failure in the aeration of the blood, and still less able to make the extraordinary respiratory efforts required for forcing the air through the tenacious mucus, or to discharge it. But fortunately, while the black blood sent to the brain is destroying nervous power, it also destroys sensibility, and all conscious suffering is soon at an end; and *coma*, being presently followed by *asphyxia*, soon terminates the struggle. Now, as children who die of this form of disease always turn livid, presenting purple spots before they die, or immediately after, if convulsions have hastened death before asphyxia had time to do its work, the matrons of the land call it the *bold-hives;* and hence, as this appearance is never presented until death is inevitable, or has actually taken place, the opinion is well founded that *bold-hives* is never cured. As children who die of *croup*, generally present this same appearance, and from the same cause, it is generally confounded with this form of hives by nurses, as well as by doctors. But they are very different diseases—*this* is peculiar to early infancy; I have known of no case occurring after the first year, and rarely after six months, and by far the greater number happen within the first month after birth; but *croup* is not a disease of *infancy;* few cases happen under two or three years. Then, again, they affect different locations: croup

is a *cynanchia trachialis*, and this a *cynanchia bronchialis*—that is, croup is located in the wind-pipe, and mostly at its upper extremity, while this is located in the minute branches, and perhaps in the air-cells; croup is characterized by a shrill sound, as if breathing through a metallic tube, or like the crowing of a cock, as the name implies; but this is known by a flat plashing sound, as of air passing through water, or more like gas escaping from paste while boiling; it resembles *mucous rale* in pneumonia more than any thing else, and is the result of about the same condition.

But though croup and this form of hives are very different in their commencement, they run very near together in their termination; both usually kill by producing asphyxia; and croup, though always attacking the upper part of the trachea at the commencement, often extends its influence downward, until, if protracted, it is seen to produce the identical effects upon the minute branches, and the air-cells, characteristic of hives, occasioning, too, the same rapid accumulation of morbid secretion.

I have already hinted that this form of hives is probably much less prevalent in cities than in the country, and I now say it is more prevalent in the most healthy regions of country; the same I think will be found to be true also of croup; and the reason that both are the most prevalent where there is the highest health, is because both are diseases of high action, laying hold of the most pre-

cocious developments of the sanguino-nervous temperament. This being the kind of subject most commonly attacked by this form of hives, it may readily be perceived why it should be seldom met with in city practice, such constitutions being rarely found there among infants; and it also explains the omission of writers to notice the disease, as all, as far as I know, have learned disease as presented in cities.

TREATMENT. — The disease, then, at its commencement, being one of high action, the indications are for the use of such measures as will subdue over-action; but while selecting them, we must remember also that our patient, though robust and sanguine, is also highly nervous and excitable, rendering the system extremely subject to collapse of nervous power; we must not, therefore, make any heavy drafts upon the source of vital energy; hence bold depletory measures are entirely out of the question; such means only should be resorted to as will subdue excessive arterial action, with the least expense to the general strength, combined with appropriate means for allaying nervous excitability. I am not at all certain that I have fallen upon the best possible measures for filling these indications, for all my efforts have occasionally failed of success, but have proved much more certain than the plan I was taught and practiced in my early professional career. They are as follows: When called to see an infant, and I perceive its face flushed, eyes somewhat suffused and

slightly injected, considerable restlessness, not crying as from pain, but making rather a grunting moan, often desiring a change of position, seizing hold of the breast with energy, but letting go after a few draws as though for want of breath, breathing rather short, and more than usual heaving of the chest, I suspect at once that I have this disease to contend with, and inquire of the mother or nurse if the child has had any *breaking out.* If I am informed that it has not, I examine still closer the condition of the surface, and I will perhaps detect the appearance of an eruption *within* the skin; but if this should fail to be detected, I proceed further, and, having the infant's face turned toward a strong light, I excite it to cry, so that the *fauces* and *pharynx* may be fully exposed; and if the surface appears to be suffused, with more or less distinct evidence of a fine papillary eruption, I become satisfied it is this form of hives, but that it has *commenced* in the air-passages, and has not yet extended to the general surface. In this stage there is not much secretion from the mucous lining, the difficulty in breathing being occasioned by overfulness of the vessels, and a thickening of the lining of the cells and air-passages. In this stage I direct a mustard bath, as warm as the child can bear without occasioning positive suffering, the object of which is to equalize the circulation, and invite the eruption to the skin, so as to produce an equilibrium of action between the external and internal surfaces. This bathing should only be continued a

few minutes at a time, and the child enveloped in a light flannel in the interim; at the same time I administer, internally, grain doses of *calomel* every hour, and a tablespoonful of *catnip* or *hop tea* every fifteen minutes. The mercurial is given to produce revulsion to the liver, and also to make that organ perform some of the labor usually assigned to the lungs. As soon as bilious discharges are obtained I discontinue the calomel, but continue the tea, for the purpose of quieting nervous excitement and maintaining the action on the liver and bowels. These measures will rarely fail to procure entire and permanent relief. But if I have been called in later, and find the child breathing with great difficulty and exhibiting the peculiar sounds indicating profuse secretion in the air-vesicles, the countenance anxious and distressed, the pulse hurried, etc., I adopt a very different set of means. In this case the excessive secretion *must* be diminished, or the child will inevitably die of asphyxia; it cannot be thrown off fast enough, by any means which can be devised, to prevent a fatal accumulation. My only hope now is in the *cold wet sheet.* I order a panful of cold water, and another of very warm water, and dipping a large towel, or some other conveniently obtained cloth, in the warm water, envelop the child's lower extremities in it; and then saturating another in *cold water*, apply it *around* its naked body, and over it a dry flannel. As soon as reäction is fairly established, I remove the cloths, not desiring a perspiration. I

give nothing in this case but the teas above named, administered cold. A fresh application of the cloths should be made every hour or so, until entire relief is obtained; then calomel may be administered as before, for the purpose of revulsion, and to free the blood from the carbon which has accumulated in consequence of imperfect aeration. If given sooner, the sickness occasioned by it might have completed the prostration. The *rationale* of the cold application is easily given—it arrests the morbid secretions, just as it does internal hemorrhage, by acting as an astringent; it also excites the surface. I deprecate a sweat, because it would invite an increase of the secretion in the lungs, by concert of action with the surface.

CYNANCHIA TRACHIALIS—CROUP.

This is a disease of high action, most prevalent in healthy country situations, and in northern latitudes. It is seen much more frequently to the north of Mason & Dixon's line in our country than to the south. I met with ten cases of this disease in Ohio, in the same amount of practice, to one in Tennessee. Here, the great majority of the cases which I have seen, that were supposed to be croup, were nothing more than common cold in a phthisical child; the stifled cough and wheezing respirations of which are easily distinguished from the ringing cough and crowing respiration of *cynanchia trachialis*. This misnaming of the disease has led many to look upon croup as rather a slight com-

plaint, which a warm bath, or a snuff plaster to relax spasm, and a little melted butter and honey to allay the tickling of the glottis, are fully able to subdue. But croup lays hold with a giant grasp, and demands the prompt interference of our most efficient measures to meet it successfully.

In the management of the first stage of this disease, I have never been able to improve upon the plan taught me by Doctors Hamilton and Judkins, then of Mt. Pleasant, Ohio, when a student of medicine; which was the warm bath, and calomel and ipecac. in such doses as would produce, first, active emesis, and then a brisk cathartic effect; and, at the same time, a fly-blister to the throat and upper part of the chest. As there is usually great insensibility to the influence of medicine in this disease, the dose must be very large, say from ten to twenty grains of each, and then made to act as an emetic, by free draughts of weak mustard water. This plan will fail in very few cases to give entire relief, if adopted early; but if the disease has progressed until it begins to assimilate the severe form of *hives*, described in the last chapter, these measures become very hazardous; and for reasons therein given. I treat this stage of croup just as I do the advanced stage of *hives*, to which I will refer the reader. I would even now, however, apply the blister as soon as considerable relief had been obtained by the cold cloths, in order to prevent a relapse.

OPHTHALMIA.

When called to a case laboring under this disease, we should consider whether it is catarrhal, malarious, or scrofulous, as our diagnosis must, to some extent, determine the plan of treatment. This distinction can generally be made with ease, but occasionally it is quite difficult. In fact, a case may be the combined result of several causes, and present a mixed character, in which it may be difficult to determine which element preponderates. A person, for example, may have been impressed by malaria, and then exposed to atmospherical vicissitudes; and both of these causes may operate upon a scrofulous constitution, and then the case might be both catarrhal, malarious, and scrofulous. But generally one of these causes so far impresses itself upon the disease, as to give it character, and determine the peculiarity of treatment. Common catarrhal or inflammatory opthalmia is usually recognized by the mode of its accession, it being ushered in along with other symptoms of *cold*. But little skill and less medicine are necessary to manage this form of sore eyes successfully, if taken at an early stage. A dose or two of epsom salts, a hot mustard foot-bath, and bathing the eyes in warm water, will effect very certain and speedy relief. But if the attack comes on after exposure to a malarious atmosphere, and is ushered in with many of the symptoms commonly associated with an attack of malarious fever, such as headache, loss of appetite,

dinginess of the surface, a sense of weakness, etc., we may safely consider it malarious ophthalmia, and treat it as we would an attack of bilious fever, with calomel, quinine, etc., paying very little attention to the local disease of the eyes. But if the patient has had an attack of sore eyes, without any of the common symptoms of cold, or those of the first stage of bilious fever, the first indisposition being an unusual sensibility of the eye to light, subsequently becoming a dark-red, and always being much relieved by shutting out the light; and especially if we detect other evidences of a scrofulous diathesis, we may, without hesitation, set it down a case of scrofulous ophthalmia, and treat it accordingly. The plan which has succeeded best in my hands in managing this form of sore eyes is, to put the patient upon an alterative course of medicine internally, and apply anodyne collyria to the eye. The best alterative course is sarsaparilla and iodide of potassa; and, as a local application, I have found nothing to compare with the following: Spts. nit., 1 oz.; chloroform, 1 oz.; morphine, 5 gr.; rose water, 4 oz.; saturate raw cotton and bind it to the eyes on going to bed.

I will illustrate the effect of this treatment by reporting a case. In the spring of 1854, I was called to see a little daughter of Mrs. Lane, South Vine street. I found the child's eyes so exceedingly sensitive, that it was with great difficulty I could examine them at all. They were swelled and deeply injected with blood, and had to be covered with

several folds of some dark material to keep down intense suffering; yet when it was quite dark, she could uncover her eyes, and would play about the yard with as much glee as children usually do in the day. Her mother informed me that she had suffered in the same way the most of the summer of '53, and that medical aid had been resorted to without benefit. I looked upon the disease as inflammatory, complicated with chylopoetic derangement, and put her upon strict diet, purgatives, and cooling applications to the eyes, but no benefit followed. And after trying my prescription for several weeks, it was dropped, and another physician, eminent for his skill, called to take charge of the case; who, taking the same view of the case which I had, pursued a similar mode of treatment, with a like want of success. She was then taken to the Springs, but still obtained no relief; but as the weather became cool, she gradually improved, and remained pretty clear of suffering all winter. Being at the house on some other occasion, I examined her eyes, and found that one had suffered but little, but there was considerable opacity of the cornea of the other. I now began to suspect that I, as well as all the other physicians who had attended on the child, had mistaken the nature of the case; that the disease was not primarily inflammatory, but that the child was of a scrofulous habit, and that the disease first manifested itself by morbid sensibility of the retina; and that the intense irritation of light, acting upon this morbidly sensitive nerve, created so much irri-

tation as to set up inflammation in the organ. I informed Mrs. Lane that I thought I had a better understanding of the disease than I had had the previous summer, and that if the child's eyes commenced getting worse, as usual, in the spring, I would like to watch them. I was accordingly sent for, and found that I was correct, so far as the origin was concerned: that the morbid sensibility was the cause of the inflammation, and not it, as we all had supposed, the cause of the sensibility. I did nothing for a time but watch the progress of the disease, until it had assumed a virulence equal to any previous attack. I then put her upon the plan stated above, and in a very few days had the satisfaction of seeing a decided amendment; and in a few weeks the disease was so effectually removed, that she could run in the sun without a bonnet, and has remained well until now, notwithstanding she went through the ordeal of an attack of measles during the summer. The plan pursued was, to apply pledgets of cotton, saturated with the collyria, to the eye every night on going to bed; remove them in the morning and let them remain off an hour or so, and then reäpply, and keep them on until evening; then again leave them off an hour or two. After considerable amendment, the application was only made at night; and finally, only occasionally. The internal medicine was composed as follows: two ounces of comp. syrup valerian, (my common fever syrup,) two ounces of comp. ext. sarsaparilla, and one drachm iodide of potassium.

Of this I ordered a teaspoonful *after* each meal. I combined the fever syrup in this prescription, for the purpose of giving tone to the bowels and restraining diarrhœa.

I have treated several other cases of a similar kind, with the same flattering success—cases which were originally of the true catarrhal kind, and also those having a malarial origin, in scrofulous constitutions.

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NOTICES OF FIRST EDITION.

BY PROFESSOR BOWLING.

The work before us is the production of a Tennessee physician—a fellow-practitioner of our own city, an intimate acquaintance, and a warm personal friend. Upon many medical topics he entertains views and opinions essentially foreign, and often in diametrical opposition to those into which we were early indoctrinated, which have grown with our growth and strengthened with our strength, and we have passed that period of life beyond which, it is truthfully said, medical men seldom or never change. It is written, that no physician over forty years of age could be convinced by Harvey concerning the circulation of the blood; and none of us have forgotten that when Jenner forwarded his immortal paper upon cow-pox to the Royal Society of London, that his great discovery might go to the world under its high auspices, its learned members, after giving the matter a careful and deliberate consideration, came unanimously to the conclusion that it ought to be suppressed, upon the

ground, that by its publication the author would hazard the loss of that reputation which he had gained in the scientific world by a paper previously published upon the natural history of the *cuckoo!* Such ludicrous mistakes as these, in the highest places of science, should teach all a profitable lesson of humility, and to exercise a spirit of forbearance and inquiry concerning every proposition in medicine that makes its way up to the judgment-seat through the accredited channels, however boldly it may contradict our most cherished opinions.

The opinions of Dr. Thompson, defended in his book, have not been hastily formed and promulgated. Jenner employed thirty years in talking upon and investigating an inquiry which seemed to be the chief end of his existence to elaborate and enforce, before he published the result to the world. Dr. Thompson has employed an equal length of time in his investigations; and after so many years of anxiety and toil, from a firm conviction of the truth and value of his discoveries, like Jenner, has become a publisher from necessity. So ardently was he engaged in testing at the bedside the claims of his conclusions upon his professional brethren, that he found no time to write any thing concerning them, and, until three years ago, had never written a line for the public eye. Every capable, honest, and unprejudiced mind that faithfully examines this work, will award to its author the possession of talents of the highest order, an intellect conspicuous for the power to grasp and

analyze the mysteries of man's organism in disease, and often to light up, in a peculiar manner, its darkest and most inaccessible recesses. This no ordinary mind can achieve. Dr. Thompson's mind seems to be so constituted as to delight in the very effort to solve the most knotty etiological and pathological problems. Not content to remain within the febrile boundaries, widened, as we had supposed, to their utmost limits by the wonderful genius of Fordyce, he delights to take up the ramifications of the subject precisely where Fordyce had pursued them to exhaustion, where the light of his intellect could follow them no farther, and constrained him to acknowledge that their termini were lost in impenetrable darkness. Our author delights, we repeat, in groping for the lost ends of febrile inquiry in the long and tenebrious night during which they have reposed in their dark abodes; and he has, as he believes, succeeded in grasping and emerging with them in the sunlight of truth beyond. We have said that Dr. Thompson makes no pretensions to fine writing, yet there is a force and compactness in his style, which many older writers might study to advantage.

While Dr. Thompson's love of theory, apparent upon almost every page of his book, tends to the raising of prejudices against it, yet his love of truth, on the other hand, should go far to subdue any feeling of that kind, and secure for the work a candid study.

Dr. Thompson has displayed extraordinary in-

genuity in accounting for many mysteries connected with fever, selecting, for the most part, such as Fordyce believed totally inexplicable, and which, perhaps, for that very reason, subsequent writers upon fever have avoided. The most difficult of these Dr. Thompson brings the analyzing power of his mind to bear upon with a force that no one can fail to admire, however far he may be from being convinced.

We have made quotations enough to show that Dr. Thompson knows what he is talking about, and that, if he were fond of controversy, he possesses the ability to constitute him a dangerous competitor in the highest walks of science. We have said that he makes no great pretensions to science as ordinarily construed. That is, he has not filled his *memory* with the dogmas of accredited authorities, and thus become scientific to the extent of the powers of his recollection. He has not abandoned the right to think for himself, and whenever the result of his cogitations comes in conflict with the dogmas, even of the high-priests in the temple consecrated to scientific truth, he modestly states his objections, and the reasons upon which they are founded.

Whether the practice suggested by Dr. Thompson's theory will prove as successful in the hands of others as he believes it has in his own, remains to be ascertained, and concerning which we hazard no opinion whatever.

BY DR. REESE, OF THE MEDICAL GAZETTE, NEW YORK.

This is an unpretending little volume, by an unpracticed author, who, nevertheless, shows that he belongs to a class of thinking physicians for whom we entertain great respect. No one can read this work without perceiving that its author is eminently a practical man, who refuses to think by proxy. What he writes has the rare merit of being suggestive, by broaching new ideas, which are calculated to set other men thinking. We honor such a man, and recommend his book.

FROM DR. S. A. CARTWRIGHT, OF NEW ORLEANS.

Your book is a small one, and nothing seemed easier than to give it a thorough perusal; but I found the task a difficult one, and I cannot say that I yet have accomplished it. Your book is hard to read; not on account of the *bones*, in the shape of theories and speculations into the essential causes of things, but on account of the fat, marrow, and savory viands you have stewed up with them. Hippocrates is hard to read, and so is Sydenham, and so is every author abounding in useful and practical information—so you need not wonder if it has taken me more than three months to read your book. Although it abounds with many objectionable things, (typographical errors, etc.,) yet I regard it as an arrow shot in the right direction. It is a safer book to practice by than Watson or Wood. It is so far ahead of the age in which we

live, that it is not likely that its author will live to see the day when it will be duly appreciated by the profession. The day is coming when the folly of treating the diseases of negroes by books written by authors who have never seen a negro, and know nothing of his anatomical and physiological peculiarities, and who have never been in a cane or cotton field, or in a negro-quarter, will be apparent. That will prepare the way for seeing the folly of treating white people, in a Southern climate, by the remedies found most appropriate for the same people in a Northern climate, laboring under diseases peculiar to that climate—a climate where arterial reäction, and not venous congestion, constitutes the main outlet to human life.

Your syrup is so strong a capillary stimulant, that here it will often cut short pneumonia, by sending the blood into the almost bloodless capillaries in cases which would probably die under the lancet, gum-water, and antiphlogistics. Your book is a look in the right direction, because it leads back to the old regular practice of medicine, and goes against the *reformed system* imported into this country originally from Edinburgh—a system of practice almost certain death in the apoplexies of the South, hurtful in most of the fevers met with in Southern climates, and very apt to put out children's eyes in ophthalmia. I lately treated a child, attacked with purulent ophthalmia a few days after birth, with the lotion you advise, applied over the eyes, and cured it promptly. This child would

have lost its eyes under leeching and the methods observed in the popular medical books of the day. The disease, in addition to the means you advise, requires a change in the nurse; good milk nourishment to cause the bones of the head to fill out—an essential indication which calomel, leeching, and lunar-caustic can never fulfil. But in apoplexy, pneumonia, and the most of our fevers in this climate, the pathological condition of the system is like the child with purulent ophthalmia, in which bleeding and antiphlogistics are as dangerous to life as they are to the eyes in the child's case. But your kind of practice is not a new thing to me. I have tested the merits of a similar practice for a third of a century. It was not very long after I began the practice before I found that the treatment recommended in Northern and European books was not adapted to Southern latitudes.

Your book is most valuable, if for nothing else, in setting sassafras, the peppers, and the spikenard of the ancients on their legs again as valuable therapeutic agents in the treatment of Southern diseases. It is also valuable in being the first thing in the shape of a book which has set a face like flint against that modern empiricism that doctors all the world, not by the remedies adapted to the diseases and climate peculiar to each part, but by only those adapted to the diseases and climate of a very small spot on the earth's surface, as different from the rest of the world as any two things can be from each other.

EXPERIENCE OF OTHERS.

R. D. HALL, M. D., Leiper's Fork, Tenn., says: "I have used your remedies for fever and flux, and have found them fully equal to what you represent. I have treated twelve cases of typhoid fever, and have succeeded promptly in all; I therefore heartily subscribe to your plan of treatment."

K. G. BOBO, of Mt. Oliphant, Tenn., says: "I have adopted your plan of treating fevers, much to my own advantage, as well as that of my patients."

F. W. COOPER, M. D., Clover-Port, says: "I have used the prescriptions given by you during the lectures of 1853–54, the past summer and fall with uniform success. I am satisfied that your manner of treatment will do to depend upon."

DANIEL ABBEY, M. D., Rock Spring, Ga., says: "I have used your remedies for fever with the best effect. I hope I will soon receive your book," etc.

JAMES L. MILES, M. D., Butler, Ala., says: "So far as I have tried your plan of treating fever, it has succeeded most admirably. I think it worthy the attention of the profession," etc.

DR. L. S. TOLSON, of Wood Lawn, Miss.: "Your theory of fever is evidently in advance of the teach-

ings of the age. I have used your fever syrup and chloroform liniment quite extensively, and am much pleased with them; have in many cases aborted or broken up typhoid fever in its incipient stage; but think the syrup contra-indicated when the glands of Peyer are ulcerated, but believe that if your plan of treatment is pursued in the early stage, it will prevent this from taking place. Have broken up erysipelatous inflammation many times by three applications of the oil of sassafras. I have recommended your work to the physicians of my acquaintance, because I believe it eminently worthy."

Dr. A. D. Cutter, Dresden, Tenn.: "I have used your plan of treatment quite extensively this year, with complete success; have used over six gallons of the *fever syrup*. I believe your practice the best that can be adopted in all fevers having a bilious origin. I have not tried it in any other."

Then follows a report of a number of very interesting cases.

Dr. L. H. M. Cooke, Silver Spring, Tenn.: "I had used your remedies for fever and dysentery before the purchase of your book, from representations of young men from my office, who had heard your lectures before the medical class, and I think your plan equal to your representations."

Dr. Y. S. Hereford, Whiteville, Tenn.: "I

have treated a number of fever cases according to the plan laid down by you, and in every instance with complete success. I have a beautiful little daughter, now in the enjoyment of the most perfect health, who was brought down to the verge of the grave by that great *baby-destroyer*, cholera infantum, upon whom every means had been employed without avail, until treated according to the plan you recommend. It acted 'like a charm.' I feel that I am indebted to you for her life. I believe your work is destined at some day to occupy an enviable position in the eyes of the medical world. If you publish a second edition, *I want it*, and you may consider me a subscriber."

Dr. N. P. Bond, Lake Providence, La.: "I have used your plan of treatment in many cases, and all grades of fever incident to the region of country in which I practice. In intermittent fever, I might say it has proved a *specific*, if such a thing can be. I have used it in rubeola scarlatina, and the sequelæ of these diseases; in pleuritis, pneumonia, etc., with the happiest results. But have I aborted typhoid fever? It is a difficult matter to say yea or nay; I have been called to see many patients who, if symptoms are to be relied on as indices, I would say had typhoid fever, and I have restored them to health in from three to seven days. I have several times called in other physicians to see such cases, who agreed with me that I had typhoid fever to treat, and yet in every case the patient was discharged cured in less than seven days."

Dr. M. J. Miller, Gas Factory, Tenn.: "I have treated upon your plan all the forms of fever common in this latitude, with as much success as could be desired. If I have not aborted typhoid fever, I have never cut short a case of remittent fever. In all the cases I have seen, the treatment has been uniformly effectual—some of them not lasting over four days, and the longest not over seven days."

Dr. Daniel Abbey, Benton, Mo.: "I thank God that your life was spared to give to the medical world the benefit of your observation and experience. I believe that the doctrines contained in your little book are founded upon the true science of medicine. Your plan of treatment never deceives me; and it leaves the system in such fine condition that I have no relapses, nor any dyspepsia follow, as formerly was frequently the case."

Dr. W. Kirk, Buckhorn, Miss.: "I received your work on Fever over two years ago, and regard it as one of the best works I have ever seen. It has been of infinite value to me in my practice. I have done a large practice, and have adopted your treatment of fevers in full, and have been entirely successful."

Dr. J. G. Boyd, Shady Grove, Tenn.: "Your plan of treatment in fever, dysentery, etc., has proved entirely successful in my hands. I consider it a great achievement in the healing art."

Dr. S. L. Walker, Franklin county, Va.: "I have tried your plan of treating fever with complete success. I will say, in all candor, that I consider your work on Fever, etc., a complete practical treatise, sufficiently scientific for the most profound minds. In fact, your system is beautifully original: were your work in the hands of every practitioner of medicine, what an incalculable amount of human suffering might be avoided! I hereby endorse your work, and cheerfully recommend it."

Dr. J. E. Fulton, Big Oak, Miss.: "I can say that I am well pleased with your work on Fever, Dysentery, etc. I have tried your plan of treatment in typhoid fever, dysentery, etc., and can safely say it has not fallen short of any thing mentioned in your work. I loaned your book to a distinguished physician, who says that the principles inculcated therein are founded on facts, and will speak for themselves yet. I hope you will publish an enlarged edition."

Dr. J. N. Croom, Mifflin, Tenn.: "I have been in possession of your work on Fever, Dysentery, etc., for over two years; and having a large practice, I have tried your plan of treatment in full, with great advantage to myself and my patients. I am well pleased with your little book, and if you are preparing another edition, I hope I will soon receive it."

Many notices are omitted, for want of space.