

Observations on the prevention and treatment of epidemic fever, at present prevailing in this Metropolis and most parts of the United Kingdom. To which are added remarks on some of the opinions of Dr. Bateman. And others, on the same subject ... / [Henry Clutterbuck].

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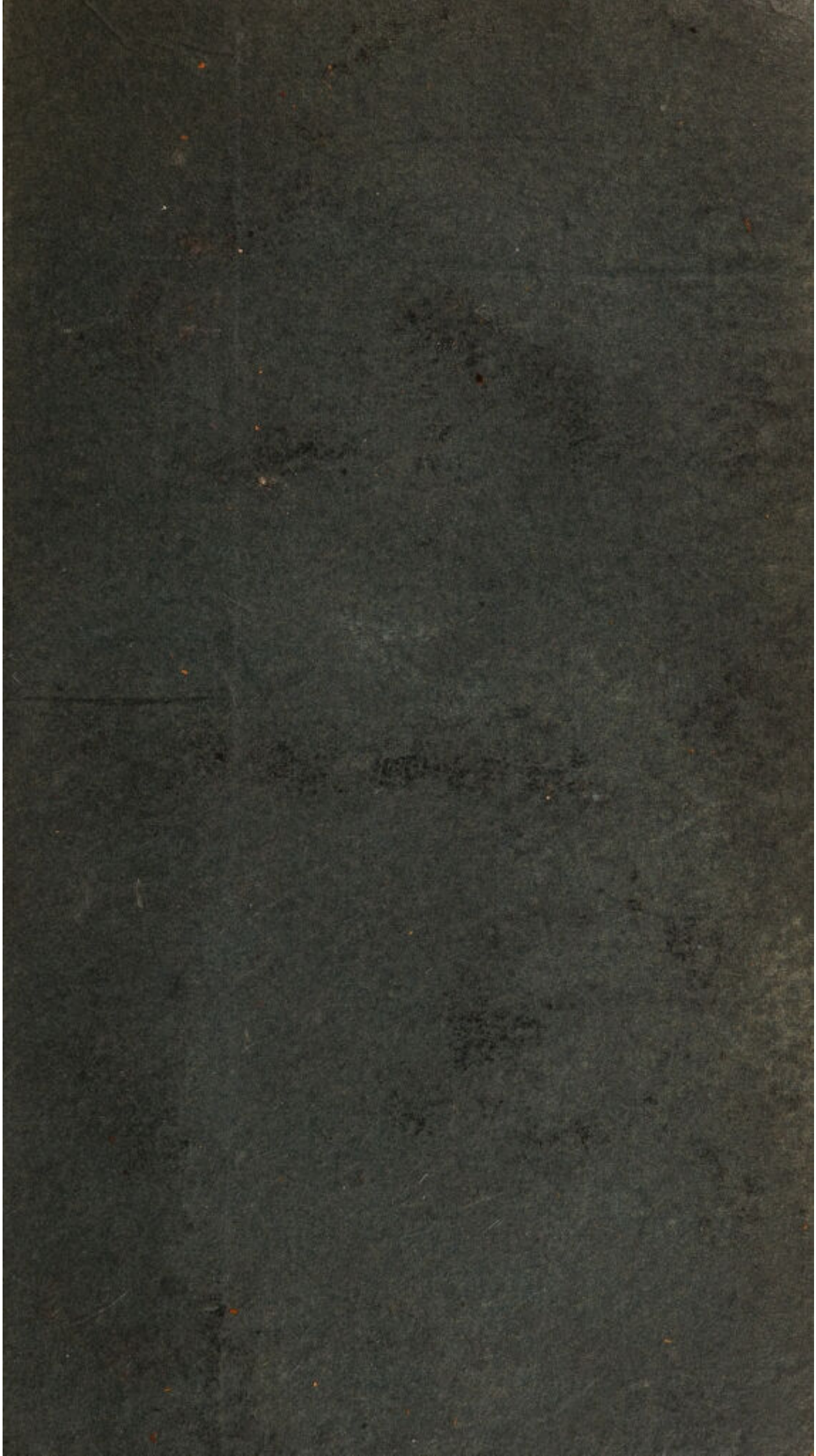
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James Nash

OBSERVATIONS

ON THE

PREVENTION AND TREATMENT
OF
EPIDEMIC FEVER.

BY
JAMES NASH, M.D.
OF THE UNIVERSITY OF CAMBRIDGE.

LONDON:
PRINTED BY J. JOHNSON, ST. PAUL'S CHURCH-YARD.

1825.

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BY

WILLIAM H. WATSON, M.D.

OF NEW YORK

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1852

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IN THIS METROPOLIS AND MOST PARTS OF THE
UNITED KINGDOM.
TO WHICH ARE ADDED,
REMARKS
ON
SOME OF THE OPINIONS OF DR. BATEMAN
AND OTHERS, ON THE SAME SUBJECT.

By HENRY CLUTTERBUCK, M.D.

LICENTIATE OF THE ROYAL COLLEGE OF PHYSICIANS,
AND ONE OF THE PHYSICIANS TO THE
GENERAL DISPENSARY.

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UNITED KINGDOM.

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AND OTHERS ON THE SAME SUBJECT.

BY HENRY CLATTERBUCK, M.D.

LECTURER IN THE LONDON HOSPITALS IN MEDICINE,
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OBSERVATIONS, &c.

THE unusual prevalence of *contagious* fever, for the last two or three years, both in this metropolis, and in most parts of the United Kingdom, is now too well established to admit of a question. The records of every public medical institution in this town, as well as elsewhere, attest the fact. The mortality that has taken place in consequence is by no means inconsiderable. In the hospitals, and other institutions devoted to the reception of such cases, the proportion of deaths has varied from one in twelve, to as much as one in five or six in this country. In Ireland, as appears from various statements published by the physicians of Cork and Dublin, the proportion of deaths has been nearly the

same ; though, from the wider spread of the disease, the mortality, upon the whole, has been much greater. In Glasgow, and in Edinburgh, it appears that *contagious* fever has been hardly less prevalent, or less fatal, than in the sister-kingdom.

This degree of mortality exceeds, undoubtedly, that which is observed in private practice. To account for this, however, it is only necessary to bear in mind, that the worst cases are commonly selected for the hospitals ; and that the disease is, in general, too far advanced, before admission into those institutions, to allow of much being done for its relief.

The subject has, as yet, lost nothing of its importance ; for, the disease, at the moment I am writing, is, according to my observation, hardly less prevalent, or less fatal, than at any period during the reign of the present epidemic. And, should a rigorous winter succeed to the unusually dry and hot summer we have just experienced, and which has occasioned a scanty production of some of the most essential articles of human sustenance, there

will be reason to apprehend a still more serious extension of the malady.

It becomes therefore a duty, incumbent on those particularly who have been placed in situations favourable for observing the disease, to give the result of their experience to the public, should it tend, in any degree, either to *prevention* or *cure*. The inquiry is by no means exhausted, considered either in a *theoretical* or *practical* point of view. There is still a want of uniformity of opinion among physicians regarding the nature of the present epidemic, as well as of fever in general: while, I am sorry to add, in practice, we are not much better agreed.

My attention, for nearly twenty years past, has been particularly directed to the subject of fever; from a desire, and perhaps a hope, of being enabled to dispel some part of the obscurity which confessedly involves both its nature and treatment. I have eagerly embraced every opportunity that has offered for investigating the subject; and from my connection, during the greater part of this time, with one of the

most extensive medical establishments in the metropolis, the *General Dispensary*, have had frequent occasion to witness the course of the present epidemic in all its forms and stages.

Dispensaries, constituted as they now are, furnish ample opportunities for investigating a subject of this kind. It is among the class of patients who attend these, that fever, when epidemic, commits its principal ravages. They afford opportunities of observing the disease from the first moment of attack to its final termination. This can seldom be done in hospitals, whether of a general nature, or devoted to fever exclusively. In these, the fever has commonly run a considerable part of its course before admission. We are informed by Dr. Bateman, that but few of the cases were admitted into the *Fever Institution*, earlier than the fifth day, and many of them not till the third week, or even later; and the same, I conclude, is the case in other hospitals, where fever-patients are rarely sent, till the character of the disease is fully developed, and danger become apparent.

So partial a view of the disease as can thus be obtained, I consider as quite inadequate to the satisfactory elucidation of the subject. Fever, in regard to its effects on the system, is the most general of all diseases, and gives rise, during its progress, to the greatest variety of symptoms. These, contemplated in the mass, present nothing but confusion. Like all complicated phenomena, they require to be subjected to strict analysis; that their order may be traced, and their relation to each other, and to the exciting cause, shown.

To the neglect of this, may be ascribed the error, as I conceive it to be, which has been so generally fallen into, of considering fever as an *universal* disease, or one that affects, from the first, the whole system; no one part being supposed to suffer necessarily before the rest. Whereas, when the disease is minutely scrutinized, and its first appearance accurately noticed, (which, indeed, from the slightness, and consequent neglect, of the first symptoms, is rarely done,) it will be found to be strictly a *topical* affection, the general disorder of system

being merely *secondary*, or symptomatic of this. It will appear further, upon the same examination, that the *brain is the part primarily affected*; and that the affection consists essentially in inflammation of this organ. The grounds of these opinions are laid down in a work published some years back*, a continuation of which I hope shortly to lay before the medical public; when, I flatter myself, the theory which I have advanced will be rendered still more clear, by both anatomical and physiological deductions, and its important relation to practice more fully shown.

In the course of the following pages, I shall have occasion to allude frequently to the recent work of Dr. Bateman on the same subject.† This, besides being the latest, is,

* Inquiry into the Seat and Nature of Fever, as deducible from the History of the Disease, the Effects of Remedies, and the Appearances on Dissection. 8vo. 1807.

† A succinct Account of the contagious Fever of this Country, exemplified in the Epidemic now prevailing in London, &c. By Thomas Bateman. M. D. F. L. S. Physician to the Public Dispensary, and Consulting Physician to the Fever Institution in London. 8vo. 1818. Longman and Co.

on other accounts, entitled to attention, from the influence it will deservedly have upon public opinion. Few have had greater opportunities than this author of investigating the subject of fever, and no one is better qualified to profit by them. It is no small satisfaction to me to find so much of accordance in our general opinions, especially in regard to the most momentous point, that of *practice*. It seems that our *theoretical* views do not yet perfectly correspond. There is, however, such an approximation, that I am willing to flatter myself, a further and candid consideration of the subject will have the effect of obliterating the slight shades of difference that still subsist between us. Whenever this is accomplished, I shall consider I have achieved a triumph. In the mean time, I shall canvass his opinions, as contrasted with those I have adopted, with all the freedom which the interests of science demand, and which I would willingly experience with regard to my own. Were I not convinced, that the doctrines I have promulgated with regard to the nature of fever

are likely to have a material and beneficial influence upon the treatment of the disease, I should not presume to solicit his or the public attention to them. But in truth, they will be found to have the strongest bearing upon practice, which will ever be wavering and unsteady till founded on a just pathology.

A great revolution is taking place in the treatment of this disease. *Blood-letting*, which, but a few years ago, was looked upon with abhorrence in the cure of contagious fever, and the utility of which is still far from being generally appreciated, is proved, by ample testimony, to be not only the most powerful, but the safest of remedies ; that is, when conducted upon proper principles, and regulated by a due regard to circumstances. The practice, however, I am willing to admit, requires much caution, and many limitations ; which nothing but a knowledge of the principles upon which it is founded can sufficiently make us acquainted with. Without these, it is liable to abuse, and, not improbably, will in consequence be again abandoned, as has formerly

been the case ; for the practice of bleeding in fever is not a new one.

To direct the administration of so powerful a remedy as blood-letting, in all cases of fever, with sufficient discrimination, is a difficult task, and one which no individual probably is yet fully qualified to execute. But I feel perfectly convinced, that the rules, with regard to it, must be sought for in the general principles I have advanced. Fever, to be treated successfully, must be treated upon the general principles of inflammation ; but at the same time, with modifications arising out of the peculiar nature of the organ affected, and, in some degree, also, the nature of the exciting cause. To ascertain these modifications is the great *desideratum*, which nothing but the most cautious observation, aided by much time, and the joint efforts of numerous individuals, can fully supply. I have presumed only to trace the outline of the path which, in my judgment, must be followed, in order to arrive at the object we are all in pursuit of ; namely, the most effectual method of combating a very frequent and formidable disease.

OF THE GENERAL CHARACTER OF
THE DISEASE.

IT is by no means my intention to give a history, in detail, of all the various appearances the present epidemic has been observed to assume in different individuals. Such an account would extend to an almost immeasurable length; with no adequate advantage, that I can perceive, in return for the labour bestowed on it. Or, if necessary, the task has been already accomplished, in numerous recent works on the subject, with a degree of minuteness that might more than satisfy our wishes in this respect.

Fever in general is subject to be so much varied by circumstances, many of them of a very trivial kind, without any essential alteration in its nature or treatment, as almost to defy description, if we allow ourselves to descend to all the *minutiæ* which present

themselves in different individuals. *Situation, mode of life, occupation, individual constitution, sex, and age*, are, all of them, circumstances capable of considerably modifying the character of the disease, so as to give minute shades of difference to it in appearance; — to say nothing of the effect of medical treatment, and the general management of the patient while under cure.

That this is really the case, any one may satisfy himself, who will be at the pains of comparing together the histories given by different writers, of the present epidemic, as it has appeared in London, Dublin, Cork, Glasgow, and other places. In each, a variety of feature may be discovered, though a *family*-likeness prevails throughout the whole. I have now lying before me a history of more than fifty cases of the prevailing fever in my own practice, as it has appeared in this city, and of which I noted down the symptoms with the greatest minuteness at the bed-side of the sick, generally once, and often twice, in the twenty-four hours, throughout the disease. Upon a revision of these cases, I find that no two

of them correspond in the minuter points ; though they all agree in the essential one, that is, in a manifest affection of the brain and its functions ; various in degree, and probably in extent ; with numerous, but accidental, complications from the affection of other organs.

Independent, however, of the circumstances above mentioned, epidemic diseases are subject to considerable diversity, from causes that are as unknown as those which originally produce them. This we learn from the works of Sydenham, one of the most acute observers, and faithful narrators of what he saw. In tracing the history of epidemic fever for five successive years, he describes each as marked by considerable peculiarities ; and so, no doubt, it would be found at any other period ; new varieties springing up every day with every change of circumstances.

This serves to shew, that an affectation of extreme minuteness, in the description of diseases that are liable to be varied by a number of trivial circumstances, can answer no pathological purpose. The nature of

fever must be deduced from more general views of the disease, and not from the laboured descriptions that have been often given; which, though they prove the industry of their authors, serve only to confuse the subject, by confounding the *essential* with the *adventitious* circumstances of the disease. Were it otherwise, indeed, we should not, at the present day, be so continually disputing about the essence of fever, and as to its seat in the body; for accurate observers and minute describers of diseases have abounded in different ages, quite down to the period of the present epidemic; without, however, having either settled the pathology of the disease, or agreed upon the proper method of cure.

Instead, therefore, of attempting to furnish a minute detail of all the varieties that have occurred in the present epidemic, (which would, indeed, be as impracticable by any individual, as it would certainly be useless,) I shall content myself with pointing out the leading and *essential* characters of the disease, by which its presence may without difficulty be known. It is of the

greatest moment to notice the earliest symptoms ; both because the disease is at this period frequently overlooked, and because it is at this time also that we have it the most in our power to interrupt its further progress.

I may premise that, after the most attentive observation, I have not been able to discover any difference in symptoms, whether the disease obviously originated in contagion, or arose independently, to all appearance, of such a cause. In many instances, I have observed the fever to follow exposure to some common cause of disease, such as cold, fatigue, or intemperance, and where not the least reason could be discovered for even suspecting it to proceed from *contagion* ; yet the disease has afterwards proved infectious to others ; and, in both cases, the symptoms have been quite undistinguishable. A similar observation has been made by Dr. Percival of Dublin, in his account of the fever as it appeared in the *Hardwicke Fever Hospital* in the years 1813, 14, and 15. “ No peculiarity of symptoms or sequel,” he observes, “ distinguished the fevers

which could apparently be traced to contagion, from those which could not be so referred.”*

If this be really the case, it predisposes one to believe that the circumstance of *contagion* will have no material influence on the effects of remedies. It is reasonable to suppose, that a disease which exhibits none but *ordinary* symptoms, is subject to ordinary means of cure. Similar symptoms can result only from a similar condition of body; which of course will be influenced by similar means. How far this is confirmed by actual experience, in the present case, will be shewn hereafter.

The first symptom almost invariably complained of, is more or less of uneasiness in the head: sometimes trifling in degree, but often amounting to severe head-ach, the seat of which is generally the forehead; when a pain is often felt, at the same time, in the eye-balls, and which is aggravated whenever the eyes are turned to one side or the

* Transactions of the College of Physicians of Ireland, vol. i.

other. Sometimes, complaint is made of the back of the head, when the pain generally extends to the neck, and down the spine. At other times, the pain is referred to the top of the head, or is spread over the whole of it.

When the head-ach is severe, it is generally attended with throbbing at the temples; and the same sensation is often felt deep in the centre, as it were, of the brain. The pain and throbbing are always much aggravated by stooping, or by any bodily exertion, or emotion of mind. On some occasions, the pain felt is of a dull kind, attended with a sense of weight, as if the head were too heavy to be supported.

This uneasiness in the head continues in general in greater or less degree throughout the disease, or till the senses and intellect become obscured, when the patient often says he has no pain in the head, or elsewhere, though the disease exists in an alarming degree, and the functions of the brain are manifestly disordered. Such a want of consciousness of his real situation,

belongs peculiarly to fever, and serves to mark the organ affected.

The pain in the head, in short, though invariably complained of at the outset of the disease, bears afterwards no necessary proportion to its violence or danger; for the pain is often the least, where the fever is the most severe. It frequently ceases after the first few days, or is only felt upon motion, or any agitation of mind.

Oftentimes the patient suffers much from a sensation of noise in the ears, which he compares to the ringing of bells, or the roaring of the waves of the sea, or the beating of a sledge-hammer, and which prevent his falling asleep.

There is a general listlessness or indisposition to exertion, both mental and bodily. The external senses are all more or less disturbed, being generally, at this period, too acute, and, at the same time, depraved; so that *light*, and *sound*, and *tastes*, and *odours*, that were before agreeable, become the contrary.

Mr. Blegborough, whose case is related hereafter, compared the noise of a hackney

coach moving gently behind him in the street, to the sudden discharge of a cart-load of paving-stones.

The appetite is likewise impaired; and food is often loathed. And there is frequent chilliness felt, without any change in the temperature of the surrounding atmosphere; showing that the sense of *touch* is as much disordered as the rest: which is further evinced by the universal pains, resembling those of rheumatism, that are often felt. The eyes, by their dull look, express the languid feelings of the patient.

These symptoms often continue for many days, before the patient considers himself as really ill, or requiring medical assistance. And in this way the disease is often greatly aggravated, and the danger enhanced, while he is fallaciously hoping for a spontaneous cessation of the symptoms. Gradually, however, they become worse, and at length compel him to submit.

There is as yet no remarkable general disorder of system, nothing that in strictness entitles it to the appellation of *fever*. Yet such symptoms constitute, indubitably, the *incipient* state of the disease, and which

it is of great practical importance to be aware of; for the entire and almost immediate suppression of it, is now greatly in our power. It is this stage, however, that is always the most neglected.

The disease hitherto is merely *local*, and confined to the brain and its *immediate* or proper functions. In many instances, it proceeds no further, the symptoms gradually declining again. This is the case where the disposition to fever is naturally but little; or where an indisposition has been acquired by habitual exposure to contagion; and also where prompt and active means of cure have been applied.

But if the disease is about to pursue its course, the general vascular system becomes affected precisely as in other inflammations. Rigors, in greater or less degree, take place, and are succeeded, as usual, by increase of heat, frequency of pulse, thirst, and foulness of the tongue; in a word, by the concurrence of symptoms, technically called *pyrexia*; and now it is that the disease literally merits the name of *fever*. This is the *confirmed* state of the disease. It may terminate

in two or three days, sometimes spontaneously, but especially by the active treatment of inflammation being applied to it; or it may be protracted to one, two, or three weeks, and even longer.

If it terminate early, and especially when the solution has been effected by blood-letting, the termination is generally rapid, and by sweating, which seems to be *critical*. But if, on the contrary, the disease is prolonged to the extent of three weeks or more, it then declines gradually; both the *general* symptoms, and the *local* affection of the brain, subsiding by slow degrees.

In a few instances, the attack of the fever has been more sudden and violent; severe rigors, with perhaps vomiting, taking place, and these immediately followed by the *febrile state* in a high degree; the *incipient*, or premonitory symptoms before described being wanting. It is in such cases, more particularly, that a sudden and critical termination by sweating may be expected.

This *confirmed* state of the disease is subject to much diversity. It may be mild in its character, and, at the same time, either

of long or short duration, as already observed. Or it may be violent from the first; when its duration is commonly, and indeed necessarily, short. Or, lastly, it may set out mildly, with no appearance of danger attached to it; yet, at a more advanced period, it may assume the most malignant characters.

The disease, when thus confirmed, may be considered as compounded of the *local* symptoms, which consist in pain of the head and disorder of the sensorial functions; and of *pyrexia*, or general febrile symptoms; the common attendant of inflammation. These may exist in very different degrees in regard to each other, with no necessary correspondence, or determined relation between them.

Sometimes the febrile symptoms, as indicated by the pulse and heat of skin chiefly, are mild, yet the functions of the brain are greatly disordered; as seen in the early delirium, the disorder of the senses, and of the muscular power. This is a dangerous state of fever, in which the tongue commonly assumes a brownish hue, in different de-

grees, with more or less thickness and dryness of the crust that covers it, according to the greater or less severity of the disease. This variety of fever has not, as far as I know, been designated by any particular term.

Sometimes the reverse of this occurs. The *pyrexia* or general febrile symptoms are violent; the pulse being strong and full, and the heat of skin excessive. The tongue is covered with a white fur. The face is flushed. Severe throbbing pain is complained of, both in the head and over the whole body. Yet the functions of the brain are but little disordered. This variety, which, to the by-standers, has a terrifying aspect, is, in reality, attended with comparatively little danger; and easily yields to a prompt application of remedies. It is what has been called the *inflammatory* form of fever, (the *synocha* of Dr. Cullen,) of which I have met with instances during the present epidemic, as well as at other periods.

If the early stage of this variety of fever be neglected, and active evacuations abstained from, under an apprehension of a

typhoid state succeeding, it sometimes happens that the general vascular action of the system subsides, while the affection of the brain increases ; and the disease assumes at length the character of the *low nervous* fever, to be presently mentioned. This is an instance of a “prophecy fulfilling itself ;” for the dread of *typhoid* symptoms, and the consequent neglect of evacuations, with perhaps the use of stimulants, are the very cause of the symptoms dreaded. This is the variety that has been arbitrarily called *Synochus* by Dr. Cullen. It is perhaps the most frequent form in which the present epidemic has hitherto appeared.

Now and then, the *febrile symptoms* run high, and at the same time the *sensorial functions* are greatly disordered. This form of fever is never of very long duration. After eight or ten days, or less, the general vascular action declines, while the disorder of the *sensorium* continues or increases. Symptoms of *putrescency* now make their appearance ; as blackness of tongue, teeth, and lips ; foetor of the breath, and of the various excretions ; *petechiæ*, or livid spots.

appear upon the skin, and often dark-coloured hæmorrhages take place from different parts. This is what has been called *putrid or malignant* fever, a form of the disease which has seldom appeared during the present epidemic, but of which, nevertheless, I have met with several instances. It appears to be owing to an unusual degree of susceptibility to fever in certain individuals, just as is observed with respect to small-pox ; but it is likewise often the creature of neglect, or of art. Where, for example, the patient is confined in a close and heated atmosphere, or where stimulants are too early and too actively employed, such a form of disease is not uncommon. The most robust men appear to be more frequently affected in this way than weak ones, or than females : doubtless because, in the former, when inflammation once arises, as in this case in the brain, the disease proceeds with greater, and often fatal violence.

A fourth variety of the disease may be marked, namely, where both the disturbance in the sensorial functions, and also the

pyrexia or general disordered vascular action, are at first very slight, and continue so for a week or more, before any alarm for the safety of the patient is entertained. Then the disorder of the *sensorium* increases from day to day, till it arrives at the highest pitch. This usually takes place towards the end of the third week, but sometimes later. The pulse, which was at first but little altered from the natural state, either in point of fulness, force, or frequency, becomes gradually smaller, weaker, and more frequent. The skin becomes hotter, especially in the trunk, though the extremities are often cold, unless their heat be supported by artificial means. There are seldom any defœcations of the skin, or hæmorrhages of black blood. The patient sinks gradually, till the sensorial functions become wholly obliterated, and the vascular action fails. This is the state answering to the *low nervous fever* of Huxham, and is one of the most frequent forms, in which the present epidemic makes its appearance, at least among the poor in this town.

In the above account of the different varieties in which the prevailing fever shews itself, I have not described any one under the name of *Typhus*, for I have really met with none that corresponded with the definition of this, as given by Dr. Cullen; at least his definition applies only to an advanced stage of the disease. *

I have passed over, likewise, a great number of trivial occurrences, because, in my opinion, they contribute nothing towards illustrating the nature of the disease, and because they are to be found in almost all the different histories that have been recently given.

Children and young people have been more frequently the subjects of this fever than persons of a more advanced age. Very young infants, as of a few months, seem hardly susceptible of the contagion :

* A similar opinion is expressed in an interesting account of the existing Epidemic, just published by Dr. Duncan, junior, as it appeared in Edinburgh. See *Reports of the Practice in the Clinical Wards of the Royal Infirmary*. By Andrew Duncan, junior, M.D. F.R.S. E. page 26.

for I have seen several instances of their continuing in health though suckled by the mother throughout the disease.

Where children have been the subjects of this fever, it has often assumed in them, after a week or more, the form of *hydrocephalus*, in a marked manner; and in general, in proportion to the age of the child, more or less of this character has been left behind; as shown by dilated pupils, occasionally squinting, and an unusually slow state of the pulse. Indeed in adults I think it evident, from the inability to stand when they first attempt to get into the erect posture, the tottering that attends their first efforts to walk, the dilated pupil, and the general vacancy of countenance, so unlike what follows other diseases, that even in them, more or less of effusion takes place in the brain, and which afterwards slowly disappears.

The extreme weakness of the lower limbs is principally felt when the patient attempts to stand; for, when lying, he is scarcely conscious of his inability to walk. This is explained, by the gravitation of the effused

fluid down the channel of the spine. In every fatal case which I have had an opportunity of examining, after death, there was more or less of fluid found effused between the membranes of the brain, below the tentorium. And thus another proof is afforded of the nature of the disease.

In some cases, the fever apparently ceases, yet the disordered vascular action in the brain is excited afresh by the most trivial circumstances. The slightest exertion of either body or mind, and every error in living, are sufficient to renew the head-ach, and to disorder, for a time, the sensorial functions. The longer the fever has continued, the more likely is this to be the case. So that after severe and protracted cases, weeks and even months often elapse, before either the general feelings of the body, or the muscular power or mental energy, are perfectly restored.

Such a state is never observed, where the fever has been cut short by blood-letting or other adequate means. However great the weakness may be that is induced by such treatment, it is of a simple kind, totally

different in its characters from what has been just described, and, in general, quickly recovered from.

The occasional complication of the fever with other inflammations, will be spoken of hereafter.

OF THE MEANS OF PREVENTION.

EPIDEMIC diseases take their origin, often from causes that are beyond our comprehension ; or, at all events that are beyond our knowledge. No known state of the atmosphere, in regard to heat or cold, moisture or dryness, or any other physical or chemical quality, can, in general, be admitted to have given rise to them ; for they frequently arise altogether independent of such circumstances. This appears to be the case with the present epidemic ; which, for a period of three years or more, has been continued through a succession of seasons, amidst a variety of atmospheric changes, with little other difference than what we observe to be impressed upon diseases of all kinds, by the influence of such causes.

As epidemics frequently thus originate in causes which are in no respect cognizable

by our senses, so they are observed to terminate after a longer or shorter interval, in a manner equally spontaneous, or independent of all human regulations. As long as they continue to reign, our object should be to study the manner of their propagation from one individual to another ; in the hope of being able to derive, from observation the means of checking, in some degree, their destructive progress. For that they may at once originate in some general cause, (whence their *epidemic* character is derived,) and be afterwards propagated by *intercourse*, cannot, I think, be questioned. In regard to the present epidemic, this is sufficiently apparent ; for in a great number of instances, the disease has taken place where no *contagion* could be traced, or even suspected, to which it might be referred ; while its propagation afterwards has unequivocally proceeded from this source.

Without stopping to examine the supposition of Sydenham, that epidemic diseases are occasioned by the escape of some subtle effluvia from the bowels of the earth, (an opinion destitute of all found-

ation in fact or observation,) I shall proceed at once to enquire into the means we possess for interrupting the further propagation of such as are decidedly *contagious*; as is the case with the fever that at present prevails in various parts of this kingdom.

That "prevention is better than cure," is a maxim, the truth of which is universally admitted; and it is doubly important, in regard to *infectious* diseases, that are apt to be disseminated widely among the public; as is the case with many of those called *epidemic*, a term that may be applied with perfect justice to the fever which for some time past has raged, and still continues to rage with unabated violence among us. Prevention, however, requires for its accomplishment a knowledge of *causes*; or, at least, of the manner and circumstances in which diseases are propagated. These are therefore the points which fall naturally to be first considered.

The origin most generally assigned to the present epidemic is a deficient supply of food to the poorer classes, owing partly to unfavourable seasons, partly to political circum-

stances of an extraordinary kind, which are too well known to require being here particularised. History, indeed, shows, that famine and pestilence are generally found in combination, or at least in immediate succession; and many of our epidemic fevers, as well as the present, have arisen in times of scarcity. This was observed to be the case at the commencement of the present century; since which, up to the period we are now arrived at, the metropolis has been remarkably exempted from *contagious* fever.

That seasons of scarcity are favourable to the production or propagation of *infectious* or *contagious** fever, is not to be doubted: but the relation that subsists between them is obscure. There are many circumstances which make it doubtful, whether deficient nutriment can be justly considered as the direct or principal source of the fever now prevailing; or whether they stand to each other at all in the relation of cause and effect. It is true, that the

* I use these terms synonymously.

disease has been far more prevalent among the poorer classes of society ; and, in some measure, in a ratio corresponding to the degree of privation endured : admitting this to be the case, the fact may nevertheless be otherwise explained.

Supposing the disease to be capable of being propagated by *contagion*, as I think cannot be reasonably doubted, it is obvious that the poor are peculiarly obnoxious to every circumstance favourable to the operation of this cause. The close and crowded state of their dwellings ; their general disregard of cleanliness, increased, as it is, by that indifference to the common decencies of life, which extreme poverty is so apt to generate ; and the depression of mind with which this state is necessarily accompanied ; are all circumstances favourable to the operation and spreading of contagion, and from which the higher orders are exempt.

The effect, it may be further observed, is not in general proportioned to such a supposed origin. We do not find, among individuals, that fever is brought on by mere deficiency of food ; at least I have never

met with an instance of the sort, although the opportunities of observing it, if it were so, must frequently occur. Nor do years of scarcity invariably bring with them the scourge of epidemic disease. The *Sweating sickness*, as it was called, that occasioned so much mortality in this country, in the fifteenth and sixteenth centuries, occurred in seasons not at all remarkable for scarcity. The same is remarked by Hodges, in his account of the plague which devastated London in the year 1665. "There was then," he says, "a greater plenty of all provisions; and the reverse of a famine." *

With regard to the present epidemic, according to my observation, the disease from the first has been as prevalent among the higher and middling classes of society as among the poor, allowance being made for the greater number of the latter, and for the circumstances mentioned above. Of the lower orders also, the disease as frequently makes its appearance in those who suffer

* *Loimologia*, or an Historical Account of the Plague in London, in 1665, by Nathaniel Hodges, M. D. 8vo. p. 21.

no remarkable privations, as in the really indigent. In short, there appears to be an unusual but general disposition to fever in all ranks; the disease arising in numerous individuals who experience no privations, and who are in no way, as far as can be discovered, exposed to the influence of contagion.

I cannot, therefore, agree with Dr. Bateman, when he alleges, that, " unquestionably, epidemic fever is generated in the first instance by defective nutriment." I rather think with him, that such a circumstance is more a *predisposing* than an *exciting* cause of fever; and merely acts by rendering the body prone to fall into the disease, when exposed to anxiety, fatigue, cold, intemperance, or the like.

In respect to the connection between scarcity and epidemic disease, may it not rather be, that the same causes, whether atmospherical, or of any other kind, which so affect the vegetable tribes as to diminish greatly the ordinary produce of human food, exert at the same time an unfavourable in-

fluence on animal life, so as to excite disease, or at least a disposition to it?

It is admitted by Dr. Bateman, that in the autumn of 1816, before the influence of the scanty harvest of that year could be felt, there was more than the usual number of applications for admission into the *Fever Institution*; and in September and October of the same year, a contagious fever appeared in different parts of the town. This seems to confirm the idea thrown out above, that the cause of the bad harvest was also the cause of the unusual disposition to fever, which manifested itself about the same period. They were merely simultaneous occurrences.

Another cause frequently supposed to give rise to infectious fever, is the crowded and filthy state of the habitations of the poor, and the consequent accumulation of the exhalations and excretions from the body, which are thus supposed to acquire virulent properties, so as to become capable of exciting fever.

It is undoubtedly true, that fever of a most malignant kind, and communicable by

contact or effluvia, has often appeared where numerous individuals have been crowded together in a small space, and where cleanliness and ventilation have at the same time been neglected. But these circumstances have so frequently occurred, without any such effect following, that it appears at least doubtful, whether *infectious* fever can be thus simply generated. Want of cleanliness and of ventilation exists constantly in the highest degree, in various parts of this metropolis; while *contagious* fever is of rare occurrence. It is probable that these, like want of food, are only *predisposing* causes, serving to give effect to the real *exciting* cause, whatever this may be, and to promote the further propagation of the disease, when once established. When contagion already exists, it is plain that such circumstances must favour the accumulation of the virus, and thereby increase its activity; while at the same time, by enfeebling the general system, they may possibly increase the susceptibility to the disease.

In short, it is easier to say, what is *not* the source of the prevailing epidemic, than

to assign its real origin. That the disease is *infectious*, that is, is communicable from one individual to another, either by actual contact, or by the effluvia escaping from the bodies of the sick, can scarcely admit of a question. I know of no one, indeed, in these times, of any considerable experience on the subject, who seems disposed to deny it. The establishment of *Fever Hospitals*, and all other means of prevention, are founded upon the admission of this fact. But whether the *virus* producing the disease be generated, like that of small-pox, in the bodies of the sick, and thrown off from them by exhalation or excretion; or whether, as some have imagined, the mere crowding together of numerous individuals, with neglect of cleanliness and ventilation, is sufficient for its production, without the actual existence of the disease previously, is a question that is more difficult to be determined. The decision is of some importance, undoubtedly, in regard to the means of prevention to be employed: for if the cause of the fever be *extrinsic* to the body, and merely the result of the accumu-

lated exhalations and excretions taking place from it, prevention becomes an easier task, and will depend almost solely upon cleanliness and ventilation. In the other case, such means will of course be less effectual. I must however pass by this point for the present, having no means of deciding it satisfactorily.

We know absolutely nothing of the essence, or intrinsic nature, of febrile contagion. There appears no reason to believe it to be at all cognizable by our senses ; it cannot therefore be submitted to examination. We know enough, however, of the laws and circumstances which regulate its action, to enable us to found upon them practical rules for prevention ; which, happily, are simple, and of easy application.

Both actual contact, and a near approach to the sick, are capable of exciting the disease ; hence it appears, that the virus is thrown off in the form of vapour ; but whether it enters into chemical combination with the surrounding atmosphere, as Dr. Haygarth has supposed ; or is simply and mechanically diffused through it, has not

been clearly ascertained. I think the latter the most probable supposition, as serving better to explain its adherence to cloathing and the like, which are again capable of communicating the infection.

Not only is the virus capable of attaching itself to cloathing, but there seems sufficient proof, that it may adhere to the furniture, walls, or floors of rooms, so as to infect persons coming into such a situation, even long after the sick have been removed from the place. In this way, contagious fever has been often kept up in a particular house or room, through a succession of inhabitants; till some powerful means of destroying the *contagion* have been had recourse to.

The range of action of *contagion*, it is impossible perhaps to determine accurately. That it is however much less than the public are impressed with the belief of, and does not in general exceed a few feet, as Dr. Haygarth has endeavoured to prove, is very probable. We should however be cautious in narrowing our views too much upon this point, as it would lead to expo-

sure that might be hazardous. I have more than once seen reason to believe that the disease was caught, merely by passing the door of an apartment in which the sick was lodged, where ventilation had been neglected. And, in a recent instance, a gentleman whom I attended, believed himself to have been infected by passing repeatedly through a narrow and dirty alley, where the fever had been raging for some time.

The activity of the *virus* appears to depend chiefly upon the degree of its concentration; even a moderate degree of dilution in atmospheric air being sufficient to deprive it of its power of acting altogether. Hence the utility of ventilation and cleanliness, which are the chief means of prevention, and which are generally found adequate to the purpose. Accordingly it is observed, that where the sick are placed in tolerably spacious apartments, through which fresh air is freely and constantly transmitted, by means of open fire-places, doors, and windows; and where the body is kept clean, by frequent ablutions, and by often changing the linen and bed-clothes, it

is very rare that even the immediate attendants of the sick are infected; while, under such regulations, there is no danger of the disease being propagated to the nearest adjoining apartments.

Hence it is, that when fever arises in an individual of the wealthier classes, however violent the disease, it seldom proceeds further. But in the dwellings of the poor, every thing conspires to propagate the fever widely. The individuals of the family, perhaps a numerous one, are necessarily congregated together in the same apartment, and often in the same bed, with the sick. Fresh air is excluded, in order to guard against cold; and cleanliness, in such situations, is too generally neglected.

We easily understand from this, why contagious fever sometimes prevails with greater frequency and fatality in winter than in summer; not because, as some have imagined, the cold of winter is in itself more favourable to the action of contagion, but because the circumstances upon which prevention particularly depend, namely, ven-

tilation and cleanliness, are, at that season, more studiously avoided.

The concentration of contagion, and the consequent danger from it, are still further enhanced, by circumstances, for which the poor themselves are not wholly answerable. The houses they occupy are often large, and every room has its family, from the cellar to the garret: thirty or forty individuals are thus often collected together, under the same roof: the different apartments must be approached by a common stair-case, which is rarely washed or cleansed; there are often no windows or openings of any kind backwards; and the *privies* are not unfrequently within the walls, and emit a loathsome stench, that is diffused over the whole house. The houses are generally situated in long and narrow alleys, with lofty buildings on each side; or in a small and confined court, which has but a single opening, and that perhaps a low gateway: such a court is, in fact, little other than a well. These places are at the same time the receptacles of all kinds of filth, which is only removed by the scavenger at distant and

uncertain intervals ; and always so imperfectly, as to leave the place highly offensive and disgusting.

Under such circumstances, an accumulation and concentration of infectious virus, once generated, become inevitable. In this manner, a perpetual source of contagion is kept up, from which every new comer is liable to receive infection ; and thus, there is reason to believe, contagion is preserved from year to year, waiting only a favourable concurrence of circumstances, to bring it into action.

While such evils are allowed to exist, in the center of a crowded city, it is in vain that *Fever Hospitals* are established, however extensive or well conducted. They may contribute to the more speedy recovery of the individuals received into them ; but they can effect little towards the extinction of contagion, a perpetual hot-bed of which is thus kept up.

To prevent the spreading of contagious fever in a large town, requires an attention to various circumstances, but principally to the three following: 1. the immediate separ-

ation of the sick from the healthy ; 2. the destruction of the *contagious virus* in the places from which the former have been removed ; and 3. the correction of the evils above enumerated, upon which the prevention of the disease for the future, principally depends.

1. The first of these, namely, the speedy removal of the sick from his family and friends, is the most pressing object on these occasions, as that in which the safety of the neighbourhood, and ultimately of the whole population, is immediately involved. To accomplish this however it is required, that proper and sufficient accommodation be prepared for the reception of the sick. But here the most lamentable deficiency will be found to exist ; and which, if not remedied, will, in the event of the continuance of the present epidemic, of which there seems every probability, be productive of the most fatal consequences. It is not a point, in which the poor alone are interested. Limited as the operation of contagion may be, under ordinary circumstances ; it is more or less active, and probably to a

greater or less extent, according to the degree of its concentration. And although it be incapable of being conveyed, by the air merely, to any considerable distance, it may undoubtedly be carried by other means. It is easy to conceive a thousand ways in which this may be effected. A more or less direct intercourse is unavoidably kept up between the different classes of society, so that in times of general pestilence*, the highest are not exempt from danger.

The other large towns in the kingdom are provided with establishments for the prevention of the spreading of contagious fever, in some measure adequate to their wants. But in London, there is nothing,

* I use this term in a general sense without meaning to apply it to the reigning epidemic, which is, in a majority of cases, of a mild description, and in few comparatively attended with danger. Yet a very general alarm begins to prevail upon the subject, in the districts where the fever is chiefly prevalent; so that many shopkeepers have expressed to me their dread of its being known to exist in their houses, from an apprehension that they should be deserted by their customers in consequence.

that deserves the name. In Dublin there is accommodation afforded for the reception of several hundred fever patients exclusively ; in London, with a population perhaps ten times greater, we have a Fever Hospital that will barely accommodate fifty patients. This may be sufficient in ordinary times ; but when fever prevails epidemically, as at present ; and threatens to extend its ravages, such means are totally inadequate to the purpose.

The other hospitals are very unwilling to open their doors at all to *fever patients* ; and justly so, while they are unprovided with wards devoted exclusively to this disease, and continue to mix fever cases indiscriminately with others, as has been hitherto done. The different parish workhouses are equally averse to take in persons ill with fever ; and indeed send away such as they already have to the *Fever Institution* ; a great proportion of the cases of which, are drawn from this source. *

* Many of the parishes *contract*, with the *Fever Institution*, to receive their fever-patients, on contributing a few guineas annually to the funds of the hospital.

If hospitals for fever exclusively, are to be relied upon for checking the progress of the epidemic, then it is obviously necessary that such establishments should be either greatly extended or multiplied. The different parts of this vast metropolis, where fever may and does prevail, are so wide asunder as to occasion great delay and inconvenience in removing patients to any one single spot. Institutions of a similar kind ought, therefore, to be opened in different quarters of the town, according to the probable exigencies of the case.

It may fairly be questioned, however, both in point of general policy and of utility, whether any considerable extension of *Fever Hospitals* exclusively be advisable. Besides the inconvenience and delay, arising from the having to remove patients to a distance, the expense attending numerous establishments of this kind is not to be overlooked; and especially when the occasion for them is likely to be but temporary. We have reason to believe, from former experience, that epidemics, though supported by contagion to a certain degree,

have yet their periods of duration, though uncertain ; after which, they spontaneously decline and disappear, to recur again, in all probability, at some future epoch. Permanent establishments, therefore, can hardly be deemed necessary for an evil that is merely temporary in duration and of uncertain occurrence.

It may be made a question, also, whether the great concentration of infectious virus, that must necessarily take place, where great numbers of patients, ill with fever, are constantly accumulated together and continually renewed, may not, in spite of all precautions, exert an unfavourable influence upon the disease, so as to increase its violence and retard recovery ; and perhaps also give occasion to frequent relapses. This was suspected lately to be the case at the *Fever Institution*, and additional means of ventilation were resorted to in consequence. But if the fact be as I have supposed, no means of ventilation that could be employed consistent with safety and proper attention to the comfort of the sick, would altogether suffice for the purpose.

Nor is it quite certain, that the conveyance of patients, in aggravated states of fever, along the crowded streets of this metropolis, is not capable of communicating infection to such passengers as are more than usually susceptible of contagion. In the memorable instance that occurred at the Old Bailey sessions, in the year 1750, the infectious effluvia appear to have been carried from the prisoners' box, by the current of air from an open window, to the most distant parts of the court, so as to infect a great number of those in its immediate direction. This proves that *contagious virus* may be blown to a distance of many yards at least, in an active state.

When hackney-coaches are used for the purpose of conveyance, the danger of infection to the public will of course be enhanced. It is indeed very properly made a rule at the *Fever Institution*, not to receive a patient but from their own vehicle, which is employed expressly for the purpose. In spite of the rule, however, either from ignorance of it, or from wilful neglect, on the part of the public, I know that hackney

coaches are sometimes used for the occasion. It is true that the patient has been refused admission in consequence, and obliged to return home again ; but, in so doing, the evil, for that time at least, is increased. In regard to the other hospitals, fevers, as well as other diseases, are conveyed, I believe, indiscriminately by the hackney coaches. This is a practice in which the safety of the public is involved, and which ought to be prohibited under severe penalties.

Instead, therefore, of either enlarging the present establishment for fever, or incurring the permanent expense of forming new institutions of a similar kind, more numerous places of reception for patients ill of fever might, on extraordinary occasions, be provided, without the inconveniences mentioned.

Every hospital might fairly be called upon to devote one, at least, of its wards, the most detached from the rest, to the purpose of receiving fever patients. If the funds, required on such an occasion, should be

wanting, they ought undoubtedly to be supplied by the public; for it is strictly a public concern, and one in which the community is deeply interested. The different parish workhouses are likewise fit receptacles for diseases of this description. One or more rooms, according to the size of the building, or the extent of the parish, should be applied to the purpose; and, under proper regulations, this might be done without risk to the other inmates.

Should these means be thought objectionable, or be found at times inadequate to the purpose, the parishes in which the fever particularly rages might hire, temporarily, one or more houses in the immediate vicinity; a sufficient number of such are always to be found, untenanted. The furniture that would be necessary on such an occasion is of the simplest kind, and might be procured at a trifling expense. The attendants and nurses might always be supplied, in sufficient numbers, from the parish workhouses, with little or no danger of infection to themselves.

In such situations, the care of the sick might be undertaken by the medical attendant with security : whereas, at present, in his attendance on the poor, in their close and filthy habitations, his life is continually in danger. A much larger number of medical practitioners fall victims to contagious fever than is generally known or suspected; they have an undoubted right to stipulate for their own safety, by exacting, as the condition of their attendance, the adoption, on the part of the public, of some such measures of precaution as are here suggested.

By such means, the immediate separation of the sick from those in health might be effected ; and thus the object of prevention insured, as far as depends upon this point. The expense would be amply compensated by the magnitude of the object.

The time in which patients recovering from fever cease to be infectious to others not being ascertained with certainty, several days at least ought to elapse, after the cessation of the disease, before they are permitted to return to their homes ; during which interval, they should be removed

from the sick rooms to others devoted to the purpose of convalescence; with all those precautions which security for the future requires, and which are now sufficiently understood. I know that, in different instances, owing to the crowded state of the *Fever Hospital*, and a desire to comply with the numerous petitions for admission that are presented, patients have been prematurely discharged from the Hospital, who have themselves relapsed into the disease on their return home, and communicated infection to others.

The day-schools in the different parishes, in which a great number of poor children are assembled together, are another fertile source, from which I have known contagious fever to be widely disseminated around the neighbourhood. Children are often sent to these schools merely to be out of the way, while the fever is raging at their homes; and sometimes they are themselves actually labouring under the disease at the time, though in so mild a manner as to be able to go abroad, yet still capable of communicating infection.

There seems no adequate means of checking this evil, but by a careful inspection of the children, from time to time, by some one competent to detect the disease. The parish apothecary might very properly be empowered to do this, and also to visit the houses in the most crowded courts and alleys, where fever is suspected to exist ; but especially the lower sort of lodging-houses, which ought to be submitted to regulation in respect to crowding, ventilation, and cleanliness : all of which are proper and sufficient objects of attention to the police.

2. The next object in regard to *prevention* is the destruction of the *contagion* in the places from which the sick have been removed. When a single individual of a family only has suffered, and has been removed at an early period of the disease, nothing further will be required than purification by airing and washing of the clothes and bedding which the sick has used ; with proper ventilation of the place by a free and constant admission of air from without. If the infection has prevailed longer in a room

or house, and especially where many individuals have fallen ill in succession, it becomes necessary then to resort to stronger means of purification. The inhabitants should, if possible, be removed for a time from the house altogether, or at least from the infected room, which should be white-washed with hot quicklime ; not the walls and ceilings merely, but the floor also. And fumigation with some of the mineral acids should be practised more than once. Such of the furniture and bedding as does not admit of being washed, should be subjected to the same process of fumigation. By these means, and by proper ventilation afterwards, the destruction of the contagion might be confidently relied upon.

Fumigation has another advantage, when employed where the dwelling is still inhabited ; namely, that of *compelling* the lodgers to admit fresh air, in order to overcome the disagreeable and suffocating odour, which the diffusion of the acid vapour occasions. And it would be well if this were enforced, from time to time, at short intervals, by the parochial authorities, under the

inspection of persons of competent judgment, in the districts where fever is known to prevail.

3. The third point requiring attention, towards the prevention of contagion in future, is the correction of the evils before hinted at, in regard to the close and filthy state of the courts and alleys, where the dwellings of the poor are chiefly situated. This is not only of importance in respect to fever, but to the general health of the inhabitants of those districts, and indeed of the whole town; the air of which must necessarily be more or less deteriorated, according to the degree in which those circumstances are allowed to exist.

The spirit of improvement, which is so active among us at present, is, unhappily, seldom directed in the most proper channel. We are busily employed in widening and beautifying the streets in the best parts of the town, where alteration is hardly required, or at least is comparatively of little moment, while we neglect objects upon which the health of the community greatly depends. More real benefit would be conferred upon

the inhabitants of this metropolis at large by pulling down some of the miserable courts and alleys in *St. Giles's*, *Grub-street*, *Cripplegate*, and others similarly situated, than in constructing palaces, widening streets that are already sufficiently commodious, or in other works of ostentation.

These tend to draw together an excessive population ; but it is too often to their destruction. The silent mortality that is daily taking place among the poor, but more particularly the infant poor, in the situations alluded to, in this large town, is known but to few ; and where known, is but too much disregarded. It is not so much abject poverty, as lingering disease, that thins the population, so as to demand an incessant and enormous supply of fresh victims from the country ; without which, great part of the immense business of this town would soon cease to be carried on.

It is some consolation to the philanthropist to reflect, that the evil, great as it is, is not a necessary or indispensable one ; though to correct it may require some sacrifices. The continually breathing a foul and pol-

luted atmosphere, is the great cause which gradually and imperceptibly undermines the health of a large portion of the inhabitants of this metropolis ; and, as far as the evil admits of remedy, this ought to be applied. It is, happily, as much within the power, as it is unquestionably the duty, of the legislature, aided by the minor authorities, to correct, in a considerable degree at least, the evils here complained of.

Many manufactories prejudicial to the health of the community are still permitted to be carried on in the centre of the town. The immense volumes of dense smoke which many of these are continually emitting, and which we necessarily inhale into the lungs in respiration, is itself an evil of no small magnitude, and for which no excuse can be offered, since, by a little regulation, this could be readily prevented.

The most important improvement that could be devised at present, would be to lay open the confined courts and narrow alleys in the most crowded parts of the town ;—or rather, to remove altogether the most objectionable of them ; and to

erect in their stead straight and sufficiently wide streets, the elevation of which should be always proportioned to their breadth. A plentiful supply of water should be every where introduced, and the kennels be daily flooded. The business of the scavenger, also, should be better performed than is now done; and not confined, as at present is too much the case, to the larger streets, where it is the least wanted. *Privies* should be allowed to be established only on the public sewers, which are now extended into almost every part of the town.*

* I am informed that a law is in existence, which prohibits, under severe penalties, the opening a communication between any *privy* and the public drains and sewers. If so absurd a law still exists, it is very wisely infringed as often as possible; without which, indeed, the salubrity as well as comfort of our dwellings, would be greatly diminished. Some excuse might be offered for such a law, while London depended solely upon the water-works at London-Bridge, for its supply of water. But at present, that the New River is brought home to our doors, and while the purer water of the Thames is conducted to us from a considerable distance above the City, it is no longer necessary that we should use the disgusting water at London-Bridge, after it has received the drainage and filth of the greater part of the town.

The houses occupied by the poor, that are situated in courts and alleys, and especially when large, as is frequently the case, should have permanent openings made in all directions, and on every floor; so as to insure a thorough draught of air. These openings should be so contrived, as not to be easily closed by the inhabitants, a thing, in winter especially, which they are always studious of doing. A permanent opening in the roof, corresponding with the staircase, should likewise be made. When this is effected, every time that a door is shut or opened, the escape of a portion of the air from within will necessarily take place, and be immediately replaced by fresh air from without; and thus ventilation, in a considerable degree, be insured.

However obvious the propriety of attending to these points is, they are, in fact, greatly neglected, and the houses of the description mentioned are in general disgustingly offensive, both to sight and smell, to those that enter them. Indeed, all the laws relating to the petty police of the town are very imperfectly executed in those

parts where the neglect is productive of the greatest inconvenience and danger. No means of eradicating contagion can possibly avail, while this is suffered to be the case; and to this it is doubtless owing, that contagion is never altogether absent from this and other large towns, though it only occasionally spreads as an epidemic, from causes which are inscrutable to us.

When the measures now suggested are properly carried into effect, and which may be easily accomplished by proper legislative enactments, aided by the vigilance of the police and parish-officers, there will be nothing wanting to render this metropolis one of the healthiest cities in the world; it is supplied with every natural requisite to health; the air, soil, and situation, are the most favourable; it is, in general, sufficiently drained, and is now plentifully supplied with good water. If, under such favourable circumstances, contagious fever should ever prevail to a great extent, it can only be from gross neglect on the part of those who are entrusted with the conservation of the public weal.

As to other means, than those of *ventilation* and *cleanliness*, for preventing the spreading of contagion, it is always to be borne in mind, that their efficacy is less satisfactorily ascertained; and, therefore, they should always be deemed of but secondary importance. White-washing, with quick-lime, the walls, ceilings, and floors, of rooms in which the contagion has recently existed, is highly proper; in addition to the other modes of cleansing and airing. The practice of diffusing the vapours of the *mineral acids* throughout the apartment where the sick are placed, is, I think, more questionable. In regard to the patients themselves, it is not free from objection, as interfering with their breathing of a pure atmosphere, which cannot but be of some importance; and as to the power of these agents in destroying contagion, the fact is difficult of proof; because the other means of ventilation and washing are always resorted to at the same time. On many occasions that I have witnessed, where both the *nitric* and the *muriatic* acids have been assiduously employed, throughout the

whole course of the disease, and in as concentrated a form as the lungs could well bear, the patient himself has not been apparently benefited by it, nor has it secured the attendants from infection.

The extrication of the vapour of *vinegar*, which is so generally in use on those occasions, vitiates the air nearly as much as the *mineral* acids; while, as an agent for destroying contagion, it is worthy of no confidence.

Vinegar is also liable to another objection; which is, that it makes it difficult to judge of the purity of the air of the room, by covering any ill smell that may exist.

It would be unnecessary to notice the trifling custom of wearing *camphor* about the person, as a supposed *preventive* of contagion; were it not that a considerable degree of reliance seems still to be placed upon it by the public. It is proper to observe, that neither this, nor any other substance, so worn, possesses such a power.

There is reason to believe, that a very *short exposure* to the contagion, as for a few

minutes at a time, is not in general sufficient for infection. Persons too, by frequent or continued exposure, probably acquire a degree of insusceptibility with regard to it, which becomes, in some measure, a security. It is probably upon these grounds, that the frequent immunity of medical practitioners and of nurses who attend the sick, rests. Those who have recently gone through the disease, appear also to acquire a temporary insusceptibility of the action of contagion : were it otherwise, indeed, relapse must be inevitable, and recovery impossible.

It seems still undecided, whether, or in what degree, the body can be rendered less susceptible of the influence of *contagion* by medicines of any kind, or by any particular mode of living. The *Peruvian bark* has been supposed to have such a power, but I know of no facts that unequivocally warrant the supposition. The analogy is perhaps too much strained, in imagining that because the bark prevents the recurrence of a paroxysm of *intermitting* fever, it will, upon a similar principle, prevent the accession of fever of a *continued* form.

It is, I believe, sufficiently confirmed by observation, that what is called a generous mode of living, with a moderate use of wine, tends to guard the body in some degree against *contagion*, as it does against cold, and some other causes of disease. But, to counteract this advantage, it is certain, that when persons in the most vigorous state of health, and especially free-livers, become the subjects of fever, the disease in general proceeds with greater violence, and is attended with more danger, than in feeble habits. It is, at the same time, equally clear, that every kind of excess, renders the body highly susceptible of the impression of contagion.

OF THE TREATMENT, IN GENERAL.

I FEEL myself called upon now to state what I have learned from experience, with regard to the treatment of the prevailing epidemic; and as my observation goes to confirm the utility of a practice that is as yet in its infancy, and which will probably encounter some opposition; and as, further, this practice is the natural result of the doctrine I have endeavoured to support, with regard to the nature of fever; it is the more incumbent on me to be explicit on the occasion. It is hardly necessary to add, that I am alluding to the treatment of the disease by *blood-letting*, and the other remedies of active inflammation.

I must observe, however, *in limine*, that it is an error to suppose, as some have done, that the doctrine I have advocated, leads to the indiscriminate employment of blood-

letting in fever. My principal object has been, to establish a more just pathology of the disease from which we might derive some general but certain principles of cure. How much these have been hitherto wanting, it would be superfluous to state. Believing, as I did, the essence of fever to be *inflammation of the brain*, all that necessarily followed was, the recommendation, in the cure, of the remedies for inflammation, of which *blood-letting*, indeed, ~~is~~ one, and the most important; but, at the same time, one that requires many limitations in its use; as is the case in all other inflammations.

The restrictions and limitations, with regard to this practice, must be determined, in part, by analogy with other inflammations; but chiefly, by observation of its effects in fever itself; and therefore only to be learned from actual and extensive experience. This was not to be obtained at the time I wrote, both from the rarity of the disease at that period, and my own want of opportunities. I did not, at that time, venture to recommend the practice of *bleed-*

ing in fever, from my own observation ; but contented myself with showing, that it had been extensively practised by others, and with the most decided advantage.

I can have no intention, therefore, of arrogating to myself, the merit of having introduced the practice of blood-letting in fever. The bold and decisive use made of this remedy on various occasions, during the late war, by the physicians and surgeons of both navy and army, had placed its utility in the most striking point of view, before any observations of mine on the subject were given to the public. Indeed, the practice of blood-letting in fever is of great antiquity. It has undergone various revolutions, having been repeatedly adopted, and as often again abandoned, in spite of the most positive and unequivocal testimony in its favour. Such is the influence of prejudice and hypothesis, which often render us blind to the most obvious truths ! The only merit, which I presume to claim, is the suggestion of the *principle* upon which the practice is or ought to be founded ; and without the adoption of which, as it

appears to me, the practice is neither intelligible in itself, nor can it be applied with the requisite discrimination or effect.

Many late writers on the subject of fever have spoken of *blood-letting* as an *occasional* remedy, proper to be employed under certain circumstances of the disease; but they have not in general relied upon it, as the first and principal means of cure.* It has been sometimes recommended, under circumstances where its use, according to to my experience, is very equivocal; and it has been neglected, where it would have been of the most decided advantage. In short, it has been employed to *relieve*

* Thus, Dr. Cheyne, of Dublin, who has furnished us with an admirable account of the fever as it appeared in the *Hardwicke Fever Hospital*, says, “blood-letting, in six cases out of seven, was practised to relieve determination to the head, the lungs, the liver, or some part of the alimentary canal;”—which is a very different thing from using it as a direct means of cure. And yet it is added soon after, “blood-letting often strangled the disease in its birth; and when practised, on a relapse being threatened, it several times restored the patient to health in a few hours.” See *Dublin Hospital Reports*, i. 15.

symptoms, which it is better calculated to *prevent*; and which I am confident it will do, when administered with such a view. It is scarcely necessary to remark, that a difference in the *time* and *mode of application* of the same remedy, may make the widest difference in the result; and this will be found to be the case, in the present instance. I venture to predict, that the practice of blood-letting in fever will be neither conducted in a consistent manner, nor prove eminently successful, till the principle upon which it is founded shall be generally understood and admitted.

But although, under certain circumstances, blood-letting should be found inadmissible, or not required, in the treatment of fever; *still*, various other means, directed to the same object, the removal of inflammation, present themselves; and which, taken together, are hardly of less importance than blood-letting itself, towards insuring a favourable result. It is obvious, that not only the administration of remedies, properly so called, but the whole conduct of the case, will be materially influenced by the view of

the disease which I have given. In short, this it is which I am anxious to inculcate—not so much that we should *bleed* in fever; (for that will require great discrimination;) but that we should always bear in mind, that it is *inflammation* we are treating, of which the *brain* is the seat; and that all our measures should be directed upon this principle.

Without at present inquiring into the practice or opinion of others upon this point, which I shall have occasion to do hereafter, I shall proceed at once to point out briefly the mode I have myself generally pursued, in the treatment of the present epidemic, and which I believe to be the most successful.

I may premise, that I consider the fever which prevails so widely at present, to be, in a great proportion of cases, the result of *contagion*, generated in, or immediately about, the bodies of those labouring under the disease. Nevertheless, I have observed it, in so many instances, clearly marked in its symptoms, and proving contagious afterwards to others, springing up *individually*, quite independent, as it would

seem, of *contagion*, and where no grounds of suspicion of this existed, that I am compelled to suppose, either that the disease may originate in other sources than *contagion*, or that the opinions generally entertained with respect to the very limited sphere of action of this principle are not well founded.

It appears to be the opinion of most physicians at present that there is something *specific* in the nature and character of fever, when produced by *contagion*, which serves to distinguish it from fever occasioned by *cold*, or other ordinary causes, and which, in an equal degree, governs its treatment so as to withdraw it from the influence of ordinary means of cure. They think that the great depression of strength, and other *malignant* characters of contagious fever, are the result of the direct, and *sedative* operation of the *virus* upon the *vital principle*, or, as it is sometimes expressed, upon the *nervous power*, and that in such cases *stimulation* is wanted, in order to counteract the sedative effect of the poison; consequently, that all debilitating

means are improper if not highly dangerous. Upon this principle, wine and other *excitants* are recommended, more or less early in the disease, for the purpose, as is said, of *supporting* the system against the depressing power of the *contagion*.

It seems not unreasonable to believe *à priori*, that a cause so peculiar in its nature and origin as *contagion* undoubtedly is, should give rise to a disease, marked by somewhat peculiar symptoms. Yet, in practice, I have not been able, on a great number of occasions, to distinguish between fever produced by contagion, and such as appeared to originate in a different source.

We often remark the greatest diversity in the character of fever, as it takes place in different individuals, though proceeding from the same contagion. In many, the disease is so mild, as to allow of the patient pursuing his ordinary business, complaining only of slight head-ach, languor, listlessness, and want of appetite; with scarcely any discernible *pyrexia* or febrile symptoms. In others, we find it marked with the most *malignant* and fatal characters.

In many of the cases which assume, in their course, the most unfavourable appearances, the symptoms, during the first week, or longer, are of the mildest description, so as to give no indication of the future danger. In some, the fever is highly *inflammatory* at first, the pulse being full, strong and frequent, the heat of body great, and the head-ach and other pains extremely severe. In many others, neither the pulse nor heat of skin deviate sensibly or materially from the natural state; and little or no pain is complained of. Sometimes, even, I have found the pulse preternaturally slow.*

On the other hand, where fever is clearly traceable to some ordinary cause, such as

* I would observe here, that although, in many cases, the pulse, in respect to force and frequency, does not vary materially from the natural state, yet it impresses the finger in a peculiar manner, and which appears to be characteristic of fever; a *vibratory* kind of feel, arising, as it seems to me, from an irregularity of action in the individual fibres of the muscular coat of the artery. It is accompanied with *softness*, unless the fever should be complicated with membranous inflammation in another organ, when the pulse generally becomes *hard*.

fatigue, *insolation*, intemperance, or exposure to cold, singly or combined, all the varieties described above, from the *mildest* to the most *malignant*, have been observed to take place. This opinion of the identity of the fever, as far as regards its symptoms, whether arising from contagion or a different cause, is maintained by others, and in particular by Dr. Percival of Dublin, in the passage before quoted.

If the contagion operated by a directly *sedative* power, it seems hardly credible that it should produce such various and opposite effects, sometimes *exciting* the general vascular action of the system in the highest degree; at other times, equally *depressing* it, deranging, in turn, every function, both of body and mind, and that in the most opposite ways. It appears to me, that a more intelligible, and more satisfactory explanation of this diversity is afforded by the assumption that the contagion first excites active disease in the brain, (of which there are so many proofs,) and that the variety observed in the general symptoms,

is owing to the different degree and extent to which this organ is affected.

To understand this, it is necessary to advert to the peculiar constitution of the brain, in regard to its circulation, in which it differs from all other organs.

The brain, it is to be considered, is included in an unyielding case of bone, which defends it from all immediate pressure of the surrounding atmosphere. The skull, like the other *cavities* (as we call them), is always completely filled by its *contents*, namely the brain, with its membranes and vessels, and the blood contained within them ; there is no *vacuity*, all the surfaces being in perfect apposition, when no fluid happens to be interposed between them. These contents are all in their nature *incompressible*, at least by any force that can possibly be supposed to be applied to them during life.

It follows from this construction, that neither can the brain itself suffer any immediate alteration in its bulk, from pressure of any kind being made upon it, nor, upon the simplest hydraulic principles, can the

whole quantity of blood in its vessels vary, from one time to another, in any sensible degree. * A difference in the *force* and *velocity* with which the blood moves in them, or in the *relative distribution* of this fluid, is all that can possibly take place. To one or other of these must be referred the changes that are observed in the state

* I exclude, of course, from this consideration the slow changes induced in the brain by protracted disease; such as effusion, and suppuration, or ulceration with loss of substance: all of which are consequences, more or less remote, of inflammation. If a greater quantity than natural of effused fluid exists within the skull, either the blood vessels must contain less blood, or the substance of the brain be removed by absorption, to make room for it; and on the other hand, if the substance of the brain be wasted by ulceration, the *vacuum* must be supplied, either by a more distended state of vessels, or by the deposition of an additional quantity of fluid.

This view of the subject was taken many years ago, by the late Dr. Monro; but has been strangely lost sight of since, in all our pathological reasonings with regard to this organ. It has however been lately applied with great effect to the case of apoplexy, by Dr. Abercrombie, in an interesting dissertation on this subject in the Edinburgh Medical and Surgical Journal for Nov. 1818. Mr. John Bell also adopts it, though to a less extent, in his *Principles of Surgery*, when treating of inflammation of the brain.

of its functions when suffering under active disease.

From the peculiarities mentioned, an increased action of arteries in the brain will often produce effects widely different from those which take place in other organs, not similarly circumstanced.

The arteries of the brain, as well as others, may have their actions increased in a certain degree, and at the same time preserve their diameters unchanged, or nearly so. When this happens, the force and velocity of the circulation in the brain will be increased, and the functions of the organ be carried on with augmented energy. A state of *excitement* in the general system will succeed, with or without disorder, according to the degree of increased vascular action in the brain, and according as it is accompanied with inflammation, or otherwise.

After a time, however, and sooner or later in different instances, from the continuance of increased action, the diameters of the arteries will be increased, and these, by occupying a disproportionate space within the skull, will compress the veins, which, in

this case, are the only parts capable of yielding to pressure. The circulation through the brain will, in consequence, be interrupted in greater or less degree, and the functions of the organ be proportionally impeded, with a diminution of energy throughout the whole system. Thus the same cause, *increased vascular action*, may produce the most opposite effects.

This serves to explain what takes place in fever. At the *outset* of the disease, while the inflammatory action is moderate, the functions of the brain are carried on in an *excited*, but *unequal* and *disordered* manner; accompanied with that general disorder of system (*pyrexia** or the febrile state) which inflammation, wherever seated, so commonly induces.

* It is proper to observe, that I always use this term *pyrexia* to express what is usually called *symptomatic fever*, and which, with some modification, is characteristic of inflammation wherever this be seated. The term *fever*, I employ in the ordinary strict sense, to denote what, by way of distinction, is called *idiopathic fever*, and which includes *pyrexia* or general febrile symptoms, common to fever with other inflammations.

In an *advanced stage* of the disease (and, in unfavourable cases, even in the beginning) the *arterial* system of the brain becomes distended, the *veins* are compressed, and the circulation through the organ more or less interrupted; and its functions, consequently, imperfectly performed. This may take place, up to the degree of complete apoplexy, or total insensibility; but still with more or less of *disorder* in the functions, owing to the nature of the disease (inflammation), and the unequal affection of the organ.

When we consider the variety of structure in the brain; that, in all probability, each part has its destined office, and is connected with, and influences, a different part of the system; and further, that the disease may affect one or more of these parts, and that unequally and in different degrees, we need be at no loss to account for the great diversity observed in the character of fever at different times, independent of that which proceeds from climate, constitution, and other causes.

From these physiological considerations in regard to the brain, I conclude that the oppressed state of functions, observed in violent or *malignant* states of fever, is not the result of the *sedative* operation of contagion, upon either the *nervous power*, or *vital principle*; but proceeds from interrupted circulation in the brain, the consequence of increased arterial action taking place there, and producing its effect in the manner stated. The use of such vague terms as *debility*, *exhaustion*, *collapse*, *venous congestion*, and the like, in order to explain the phenomena, appear to me to be quite unnecessary.

The determination of the question as to the immediate cause of the great depression of strength and other signs of *malignity* which sometimes appear in fever, is of the utmost importance, in regard to practice. If we consider them as the effect of the direct and sedative operation of the virus upon the *vital* or *nervous* power, it is natural that we should endeavour to counteract them by *stimulant* means; as has been most frequently done. If, on the contrary, such

symptoms are only consequences of previous *inflammatory action* in the brain, then the treatment will be directed by quite other views. Our object will be, to anticipate and prevent such a state of the organ, by active antiphlogistic measures at first.

In proportion as we succeed in lessening the inflammatory action that is going on in the brain, we shall preserve it from those changes in the state of its circulation, and the consequences of this, effusion, and alteration of structure, which the continuance of such an action will inevitably sooner or later induce. This is the general principle, according to my judgment, which is to be kept in view, and upon which we are to act, in the treatment of *contagious*, as well as all other idiopathic fever.

But although it be admitted that fever proceeding from contagion has something *specific* in its nature, as is not improbable, and that this may modify, in some measure, the effects of remedies; still, it remains to be proved, in what degree this is the case, and how far the disease is thereby withdrawn from the influence of ordinary means of

cure. The question, as far as regards practice, can only be certainly decided by experience. It may be observed, however, that diseases may be more or less *specific* in their nature, that is, may deviate in greater or less degree from ordinary diseases, and be proportionably more or less under the influence of common remedies.

The disease produced by the *variola* poison, for example, is *specific* in the highest degree. It can be produced, as far as we know, but by a single cause, which is, its own proper contagion; its characters and periods are determined with much uniformity; and it resists all ordinary means of cure: the effect of our most powerful remedies being merely to mitigate its symptoms, or possibly to suspend for a time its course. The *measles* appear to be not less strongly marked, in all these respects, than the *small-pox*.

The *scarlet-fever*, on the other hand, while it is nearly as determinate in its characters, and probably as to the cause inducing it, is nevertheless, in a much greater degree than the former, under the influence of common

remedies ; for it is unquestionable, that this disease has often been superseded entirely, at its commencement, by the *cold affusion*; by *vomiting* and *purgings*, purposely excited; and, as is said, by *blood-letting* also; and thus prevented from running through its course.

But in ordinary idiopathic fever the causes inducing it are various ; while, as compared with the true *specific* fevers, the characters of the disease are less determined, and subject to much greater diversity. The difference in character between the mildest form of fever and the most *malignant*, is extreme, although proceeding from the same source. We frequently also meet with fever, arising, as far as we are able to discover, from the ordinary causes of disease, such as *heat*, *cold*, *violent exertions* and the like, and wholly independent of contagion, which, in its course, assumes all the characters of contagious fever of the worst description, so as to be undistinguishable from it. If this be true, it at once settles the question, as to the immediate cause of the *malignant* symptoms: they cannot be the effect of the direct oper-

ation of the contagious virus upon either the *vital principle*, or the *nervous power*.

Contagious fever, therefore, admitting it to be of a *specific* nature, is yet incomparably less so, than the other *specific* fevers; and accordingly might be expected to be more under the influence of ordinary remedies: and this, I contend, experience proves to be the case. The common remedies of inflammation will be found nearly, if not entirely, as efficacious in the cure of *contagious* fever, under certain limitations, as in fever originating in other sources, or even as in inflammation in general. Any difference that exists in this respect, is to be referred more to the peculiarity of the organ affected, than to the nature of the exciting cause, or of the disease induced by it.

For it is always to be borne in mind, that the effects of remedies are influenced, in some degree, by the nature of the organ affected: so that the same remedy is not always applicable to the same disease, when seated in a different part; or may require limitations and modifications in each particular case. This is evinced in the case of

acute rheumatism, as compared with *pleurisy*; both are acute inflammations; both often attended with the symptoms commonly considered as indicating the necessity of *blood-letting* in the highest degree. Yet from experience we learn, that while this evacuation is almost a certain remedy in the cure of *pleurisy*, it often fails in that of *rheumatism*, and sometimes seems even to retard the cure.

The brain, like other organs, appears to have *its* peculiarities in this respect; and they chiefly, I think, regard the *stage* of the disease, when this consists in inflammation. Hence, although blood-letting is a most efficient remedy, at the commencement of fever, it may be no less injurious when employed at too late a period. The time in which blood-letting can be usefully employed in fever, and, I believe, in all inflammatory affections of the brain, appears to be more limited than in other inflammations. The cause of this peculiarity must probably be sought for in the peculiar circumstances of this organ, with regard to its circulation, as hinted at above.

After these general remarks I would observe, that the remedies which I have found to be *unequivocally* useful, in the treatment of the present epidemic, are very few in number, and in all respects the same as those employed in the ordinary treatment of inflammation. They are *blood-letting*, *vomiting*, *purgings*, and the *digitalis*; and, occasionally, *blistering*. These, though all tending to the same object, are of different relative importance, and applicable under somewhat different circumstances. I shall endeavour briefly to point out the particular cases to which each is adapted, according to the experience I have hitherto had of them.

I have placed *blood-letting* at the head of the list, both as the most powerful in its effects, and as requiring the greatest judgment and discrimination in its employment. Under certain circumstances of the disease, which may be defined with sufficient precision, I can speak of it with confidence; not only as the most efficacious, but, in a certain sense, as the safest of our means of cure; for it obviates danger by anticipation.

In other circumstances, I have felt, and still feel, with regard to it, a considerable degree of doubt and even apprehension ; well knowing, that improperly administered, it may be productive of the greatest mischief. No one can be more strongly impressed than I am with the necessity of caution in the employment of this powerful remedy. I consider that we have yet much to learn, as to its proper administration, under different circumstances ; especially in an advanced stage of the disease.

It appears to me, that the commonly received division of fever, founded on its greater or less severity, or the prevalence of particular symptoms, as into *typhus mitior* and *gravior*, *synocha*, *synochus*, and the like, is nearly useless in regard to practice ; since these being but different degrees of the same thing, require no essential difference of treatment. In one respect, such a division of the subject is injurious ; as it leads to the neglect of the milder forms of the disease, at a time when it is almost entirely in our power to subdue it : in consequence of which, it often insensibly creeps on to a

stage, in which we are precluded from the use of our most effective means of cure. It is well known, that a fever which sets out with the mildest symptoms, often assumes, during its course, the greatest malignity of character, so as to defy all the efforts of art.

Nor is the division of fever into *simple* and *complicated*, practically considered, of any real moment; for it is inflammation that we are to treat, whether the brain alone suffers, as in the simplest form of the disease, or other organs are affected at the same time along with the brain,—it is still inflammation, and our remedies are essentially the same. The proper adaptation of them to the different circumstances of the case, is the only point of difficulty.

It is the *stage* or *period* of the disease that chiefly requires attention in practice; for upon this, more than any other circumstance, the utility and efficacy, and even the safety, of some of our means of cure depend. If I were called upon to arrange the prevailing fever with a view to practice merely, I should divide it into different *stages*; and

three of these, I think, might be usefully marked.

1. The *first*, or *incipient* stage, as before described, is that in which the disease is not yet completely formed. The topical affection of the brain at this period is sufficiently denoted by dull pains in the head, restlessness, disordered sensations, and enfeebled muscular and mental powers ; but there is little or no *pyrexia*, or febrile state of system.

2. The *second*, I would call the *active stage* of the disease, where, by the accession of *pyrexia*, or general febrile symptoms, the fever may be considered as fully formed. In this stage, which makes up the principal part of the disease, there is evidence of much increased and disordered action going on in the vessels of the brain. The pain of the head at this time becomes more acute, and is of a throbbing and distensile kind. The sensorial functions are more disordered, and are generally in an *excited* state ; while the *incipient* stage was characterised rather by general torpor and insensibility. The febrile symptoms, as heat of skin, quickness

of pulse, thirst, and foulness of tongue, are now all present in a high degree.

3. The *third stage* is the stage of *oppression*, which, when strongly marked, approaches in its characters to the apoplectic state. In this, the sensorial functions are carried on in the most imperfect and disordered way; the different senses are obscured; the muscular power extremely enfeebled, and exerted involuntarily; and the mind almost wholly obliterated.

This division, it is proper to observe, is in some measure arbitrary, and has no exact relation to time. For as the whole duration of the disease is extremely various, extending from a few days to as many weeks; so the continuance of the different stages, both individually and relatively to each other, is not less various; and it becomes impossible to assign a determinate extent to any one of them; added to which, they often run imperceptibly into one another. A sufficient discrimination, however, may be made between them for the purposes of practice; which is the point of chief importance.

1. *Of the Treatment of the first or* INCIPIENT *Stage.*

This stage is sometimes so short in duration as to escape notice. The patient, from being in apparent health, may be all at once seized with severe *rigors*, which are quickly followed by increase of heat, and other febrile symptoms; and, in the space of a few hours, he is in a high and complete state of fever. There is, in such cases, no time for the employment of remedies, before the disease is fully formed.

In most cases, the approach of the disease is more slow and gradual; the first or *local* symptoms are however generally disregarded by the patient, as of trivial importance; and, on that account, are seldom brought under the observation of the practitioner. They are usually ascribed to the *having taken cold*, or some other trifling cause; and, in many cases, after a few days, they subside again of themselves, without ever acquiring or meriting the name of fever. Such a spontaneous, and by no means unfrequent, termination of the disease, is the occasion of al-

most all cases of fever, that arise in this gradual manner, being neglected at a time, when it is commonly easy to remove them; and thus they are allowed to proceed, till they reach the second or *active* stage, when their removal is a matter of greater difficulty and uncertainty.

Were it possible, at the outset, to distinguish the cases that would thus subside spontaneously, from such as are disposed to proceed through their course, and perhaps prove severe or dangerous; or were it afterwards equally in our power, as at first, by active treatment to ward off the fatal tendency, — such neglect would be of little moment. But as the contrary is the case, common prudence seems to require that attention should be paid even to the earliest symptoms; in order to avert, or to lessen the chance of, a more formidable train of evils.

Under certain circumstances, this is a matter of real importance; as when children are affected, in whom fever often makes rapid strides to danger; and in robust and inflammatory habits, which, when at-

tacked, are apt to suffer a violent disease. When fever is epidemic, likewise, and when, consequently, the disposition to it is strong, and a more severe disease naturally to be apprehended; in all such cases, the precaution of attending to the earliest symptoms should never be neglected.

Blood-letting to a moderate extent, when employed in this *incipient* stage of fever, will generally, as far as my observation goes, bring the disease to an almost immediate termination. I have frequently had recourse to it at this period, under the circumstances mentioned; and never without decided advantage.

The *necessity*, however, of employing so Herculean a remedy, at such a period of the disease, and before any alarming symptom has appeared, will, perhaps, be questioned. Without doubt, it may often be safely dispensed with. But still, *in the particular circumstances alluded to above*, and considering how little we are able to foresee the future course of the disease, the practice appears to me to be both justifiable and proper; and we make ourselves responsible,

I think, for all the bad consequences that may ensue from the neglect of it.

In other circumstances, blood-letting may be safely omitted, and the cure trusted to what are *commonly*, though not always *justly*, considered as milder means; such as *vomiting* and *purgings*, which, at this period, will rarely fail to answer the intended purpose. *Sweating* has often been employed in this stage of fever with success; but as the means of inducing this evacuation are in general of a highly stimulant nature, the remedy is on that account equivocal. The attempt, which is frequently made, to throw off the first symptoms of fever by wine, or other stimulant means, though it may occasionally succeed, is well calculated, in case of failure, to confirm the disease; and it has often had this effect. The *cold affusion* has been said, and not without probability, to cut short the disease in this stage; but of this practice I have no experience.

Of the Treatment of the second or ACTIVE Stage.

The violence, extent, and termination of this stage, which (as before observed) makes up the principal part of the disease, are all extremely various.

The more violent the common febrile symptoms are at the outset, the shorter, in general, will be their duration. The disease will, in this case, either terminate in health within the space of a few days, and then generally by a critical sweat; or symptoms of *oppressed brain* above alluded to will take place, constituting the *third stage*.

Where the symptoms of the second or *active stage* are mild at first, and especially when the disease has crept on for several days by almost imperceptible degrees, the fever altogether will be commonly more protracted; and it then will either terminate gradually but slowly in health; or, at length, prove fatal. When the latter is about to be the case, the brain, towards the end, exhibits the usual signs of *oppression*; for the closing scene, in all the varieties of

simple fever, as well as other forms of inflammation of the brain, is very nearly the same.

At the commencement of this *active stage* of fever, I consider the disease as still greatly within our power. It will often require, however, a very active use of remedies, in order to accomplish our purpose ; and the earlier they are applied, the greater always will be the chance of success from them.

The treatment must, of course, be proportioned to the circumstances of the case. And in forming our judgment we should distinguish between the *essential* symptoms of the disease and the *pyrexia* merely or common febrile symptoms ; the former consisting in the disorder of the *sensorial functions*, the latter indicating only the state of general vascular action in the system.

When the *essential* symptoms are violent, where, for example, the external senses are much disordered, the muscular power greatly weakened, and the mind early and much disturbed, the most active treatment is required in order to insure the patient's safety. When such circumstances occur

very early in the disease, as within two or three days of the complete formation of the fever, *blood-letting* should be considered as indispensable; and it ought to be the first measure resorted to. It should be used, too, with all the freedom that the strength of the patient will allow of; and in estimating this, we are not to be guided by the feelings merely, which may be languid in the extreme; nor by the state of the muscular strength, for this may be greatly prostrated. Nor will the pulse alone serve us as a guide, for this is often weak, where blood-letting is highly necessary; we should consider, rather, what the patient is likely to be able to bear, in reference to his *previous* state of health; without regard to the *present* depression of strength, which is temporary, and will disappear, in proportion as the disease is relieved.

The *quantity* of blood to be drawn, appears, as far as I can judge, to be regulated by nearly, if not precisely, the same circumstances as in other inflammations. If blood be largely taken at first, as from 20 to 30 ounces, and that within 24 hours

or little more of the attack, a single bleeding will often suffice for the cure. If only 10 or 12 ounces be drawn, it is often found necessary to repeat the operation several times, till, in some instances, more than 80 or 100 ounces have been taken away. By this mode, more blood is lost upon the whole, and the disease is more protracted. The former, therefore, is generally to be preferred. It may be laid down, as a general rule, in the treatment of both fever and other inflammations, that the quantity of blood, proper to be drawn at one time, is in the *inverse ratio* of the continuance of the disease. Numerous instances have occurred during the present epidemic, where a patient has been just able to crawl to the *Dispensary*, with fever strongly depicted in his countenance; tottering and tremulous in all his movements; complaining of severe head-ach, and pains in his back and limbs, with a tongue thickly coated; the pulse, at the same time, extremely feeble; and who, after a large bleeding, with other evacuations, has returned on the following

visiting day free from complaint, and only moderately weakened.

In cases of a somewhat worse description, where the patient has been too ill to leave his home, and where the disease has run on for three or four days unattended to, it has generally required three or more moderate bleedings, on successive days, to accomplish the cure. In such cases, every repetition of the operation gives the most decided and immediate relief, both to the head-ach and other symptoms, but they have commonly returned again, after a few hours, with their former violence; till, by a sufficient use of the remedy, the disease has been entirely subdued.

It often happens, that shortly after even the first bleeding, the patient falls into a profuse sweat, which continues for some hours and terminates the disease. If, however, the symptoms, though diminished in violence, remain afterwards, in any considerable degree; such as head-ach, quickness of pulse, and foulness of the tongue; a repetition of blood-letting becomes ne-

cessary. It is to be always recollected, that we are not drawing blood to *relieve symptoms* merely, but to *cure the disease*. If we desist, as soon as the symptoms are a little diminished, without being effectually checked, they will often, after a few hours, begin again to advance upon us, and little real advantage will have been gained. The reason which I have often heard given for restraining the lancet, namely, that *the patient is better*, is with me, in such cases, an argument for the further use of it.

It is remarked by Sydenham that blood should not only be taken early, but in quantity sufficient to effect the entire removal of the disease; and that the taking less than this is even injurious. I have not observed this to be the case in the present epidemic. Where blood has been too sparingly drawn to accomplish the absolute cure of the fever, it has yet always had a good effect in mitigating the symptoms, and apparently diminishing the future violence and danger of the disease.

I think it is generally desirable that the patient should be bled in the recumbent

posture, in order to insure a sufficient loss of blood, and, at the same time, to avoid fainting; which, while it is rather disadvantageous, by the great disorder it occasions in the circulation of the brain, is apt to excite alarm, and aversion from further bleeding afterwards.

It may be here remarked, that the appearance of the blood in this stage of fever, while it essentially corresponds with that drawn in other active inflammations, has yet something peculiar, by which it may in general be distinguished; and which, I think, is characteristic of fever, or inflammation of the *medullary substance* of the brain. The inflammatory crust, (which, except at the very outset of the disease, is rarely wanting, and which serves to prove the nature of the affection,) is rather like a semi-transparent jelly, or melted glue, than *buff*; while, in other inflammations, the *buffy coat* is opaque and yellowish, or really resembles what the name imports.* And

* The distinction between *sizy* and *buffy* blood is here quite appropriate.

the *crassamentum* altogether, instead of being contracted or globular in shape, is flat and broad on its surface, or merely the edges of it curled up; and, at the same time, red particles are observed to be interspersed unequally through the *size*, giving it more or less of a pink-coloured tinge.

It would be of importance to be able to state with precision, the length of time within which blood-letting might be freely had recourse to, not only with safety, but with a reasonable presumption of its cutting short the disease; or, at all events, of lessening its future violence and danger.

In hot climates, this term appears to be of very limited duration, hardly exceeding, perhaps, 24 hours; after which, blood-letting can in general no longer be thought of. In the fevers of this country, and particularly in the present epidemic, bleeding will, I believe, be always safe, and generally effectual for the purposes stated, if resorted to at any time within three or four days of the attack; and I do not mean to include here

the first feelings of languor and uneasiness, which the patient may experience for many days, before the disease is fully formed.

When used with tolerable freedom within the period here mentioned, and *before* bad symptoms have made their appearance, *I have never once observed symptoms of malignity*, as they are called, to come on afterwards; neither the muttering delirium, the blackness of the mouth, the *subsultus tendinum*, nor the extreme prostration of strength, so often seen in cases that have been either left to themselves, or treated upon a different plan. Early blood-letting, therefore, may be considered as almost certainly *preventive*, with regard to such symptoms.

On many occasions, I have employed this evacuation, not only without injury, but with manifest advantage, at the end of the first week, and have known it used still later: its effect, however, generally, then is, to *mitigate*, but not to *cure*, as at an earlier period. It is still often capable of *preventing* that horrible train of symptoms which are apt to present themselves, where

a different mode of treatment has been pursued ; while, I am quite convinced, it has not the least tendency to *induce* such symptoms, nor in any degree to accelerate their approach.

I have been extremely solicitous to discover any signs, upon which we might, in all cases, confidently rely, in order to determine, with exactness, the period when blood-letting becomes unsafe, in the treatment of fever ; but I cannot say that I have succeeded to my perfect satisfaction.

It does not appear to depend *solely* upon the *stage* of the disease ; though this is undoubtedly the most important point to be attended to. I have occasionally employed it with advantage, at almost every period, up to the end of three weeks ; but it is very seldom that I have ventured to do it, even after the first week. I believe, however, it may be safely done, (with proper caution as to quantity,) as long as the *sensorial functions* are carried on in a tolerably perfect manner ; that is, while the external senses are preserved, the intellect sound, and

the voluntary power merely impaired, without being disordered.

Contrary to what is generally supposed, I think that *the more the sensorial functions are disturbed, — particularly, the greater the delirium, — the less confidence can be placed in this evacuation*: it then generally comes too late. It is easy to *prevent* such symptoms, but difficult to remove them, when once established. *The practice of waiting for symptoms of this kind, in order to justify loss of blood, cannot be too much deprecated.*

In judging of the propriety of blood-letting, the pulse alone is not a sufficient guide; for, on numerous occasions, this has been weak and small, where blood-letting has proved of the most decided advantage. The full and bounding pulse appears to me to be much more equivocal, as a reason for bleeding, than a small and contracted state of it. But where the pulse is extremely soft, and compressible with the slightest force, I hold blood-letting to be altogether inadmissible. Whenever it is employed in

the advanced stage of fever, it should be done with much caution and reserve in regard to quantity. The loss of even two or three ounces of blood, will then produce a great effect.

There are many practitioners who object to *general bleeding*, in cases of fever, yet will employ, without hesitation, a number of *leeches*; believing them to be more safe, and equally effectual. The justice of this opinion may, however, be questioned.

I suppose it will be admitted, that the benefit derived from the application of leeches results from the change induced by the loss of blood upon the general system; and not from *derivation* or *revulsion*, as was formerly imagined. This change will be in proportion, not to the *quantity* merely of blood drawn, but, in some degree, according to the *rapidity* with which it is taken away. Hence, the loss of two or three ounces of blood from a large vein will affect the system more than an equal quantity taken by leeches; but the greater effect in the former case will be temporary only;

while the ultimate or permanent effect, in inducing weakness, will be the same in both cases. As, therefore, the system can be more readily affected by a small quantity of blood taken from a large vein, than when drawn by leeches, the former mode, in an exhausted state of system, appears the preferable one. As a mere question of experience, I have never seen any reason to believe the application of leeches to be attended with superior, or even equal, advantage, with general bleeding.

There are other disadvantages attending *leeches*, which are not unworthy of notice. Their application is often attended with much fatigue to the patient; and we are seldom able to estimate, with sufficient accuracy, the quantity of blood lost. We lose the opportunity likewise of examining the change induced on the blood by the disease.

It has been urged as an objection to blood-letting in the fevers of this climate, that it is not in general *necessary*; as the disease, for the most part, terminates favourably, where bleeding is not employed. The

objection is plausible, but of little real weight. The fatal cases, in the fevers of this country in general, as well as in the present epidemic, under the judicious management that is now generally adopted, are certainly few in number, compared with the fevers of hot climates, or of former periods; but when we find the deaths rated so high as 1 in 15 or 20, and often much higher, it cannot be considered as unimportant whether we adopt or reject a mode of treatment that promises to diminish, in a material degree, the fatal tendency of the disease.

One great reason for the early use of blood-letting, even in the milder forms of the disease, has been already stated, namely, the uncertainty with regard to the future state of it, which can by no means be always judged of from the early symptoms. These are often extremely mild for several days, and yet the disease afterwards assumes the most malignant and fatal character.

By adopting the practice here recommended, blood-letting will sometimes undoubtedly be employed unnecessarily, that

is, in cases that would have done well without it : but this is a trifling evil, in comparison with the neglect of it in such as are likely to turn out unfavourably, and which cannot be foreseen. If any rule could be laid down upon the subject, it would be, as before observed, that where fever attacks vigorous subjects, and especially when it is prevailing epidemically, blood-letting should be considered as indispensable.

Where the indigent classes, whose constitutions are enfeebled by scanty or unwholesome food, become the subjects of fever, the disease in them being likely to be of a milder description, blood-letting is less necessary. But I know from experience, that, even in such, the practice is not attended with danger, if early administered, and in a degree proportioned to the state and circumstances of the patient ; while it appears, under these limitations, according to my observation, to be hardly less successful than in others. It is a great mistake to suppose, that blood-letting is inadmissible, either in this or in other diseases, merely because the patient is weak.

A dread of bleeding has been entertained from an apprehension that the general health would suffer afterwards, from the debility induced by it. This is, however, a most unfounded apprehension. There is no comparison to be made between the weakness following a long protracted case of fever, and that which succeeds to the loss of even a considerable quantity of blood. In the former case, many months often elapse before the strength is restored; and, in many instances, one or more of the sensorial functions are left in an imperfect state. In the latter, the weakness is of a simple kind, and in general very quickly recovered from.

Another, and not the least advantage, attending the practice here proposed, is that, by shortening the disease, it lessens the danger of infection to others. It is probable, that these fevers do not acquire an infectious property for several days after their commencement.* The speedy cure of them, therefore, becomes nearly equivalent to se-

* Dr. Haygarth limits the period to four days.

paration as a means of prevention, and thus not only are the danger and suffering of the individual greatly lessened, but the safety of the public is consulted at the same time. The subject thus acquires a double degree of importance.

The other means above alluded to of cutting short the course of fever, though far inferior, as I believe, to blood-letting, in point of efficacy, are yet of no trifling power in this respect. The effect of *vomiting* has been long known; and I have had very many proofs, during the present epidemic, of its sufficiency for the purpose. The practice of employing *emetics*, at the outset of fever of all descriptions, is indeed very general; but the intention with which they are administered being different, the object of cutting short the disease has seldom been attained.

It is not enough merely to discharge the contents of the stomach, for which the mildest medicine might suffice: the object is, to produce *counter-irritation*, as the means of checking, and ultimately superseding, the diseased actions that are going on

in the system in general, but particularly in the brain. The medicine used for the purpose should therefore be of a sufficiently active kind, and it should be repeated, at intervals of six, eight, or twelve hours, for one, two, or three days, if the disease should not sooner yield. In this way, I have often succeeded in shortening the course of *contagious*, as well as other fever, before I had acquired sufficient confidence in the use of blood-letting; and also where the circumstances of the patient seemed not to call for this.

As with regard to *bleeding*, the success of this practice depends greatly upon the earliness of its administration. It is not in general injurious, even where it fails to cure; and may therefore be employed with less apprehension, and at a later period of the disease, than blood-letting.

There are, however, occasional objections to the use of *emetics*, which must not be overlooked. In vigorous and plethoric habits, with marks of great determination to the head, full vomiting may prove dangerous, without the precaution of previous

blood-letting. Also, where great irritability of stomach already exists, as shown by frequent and severe vomiting, the use of emetics is objectionable. And where the slightest apprehension of inflammation in this organ is entertained, and which is indicated by continued spontaneous vomiting, with pain at the stomach, and tenderness to the touch in that region; or even where inflammation is suspected to exist in any part of the abdomen, the employment of emetics is highly dangerous. It is for this reason that, in the fevers of hot climates, of which inflammation in the stomach is a very frequent attendant, emetics are seldom admissible.

Purging, at the commencement of fever, is a remedy of the greatest utility, and which alone will often cut short the disease, as I have proved by much experience. Like *vomiting*, it should be excited by active means, and repeated at short intervals, for two or three successive days. If the object is not then attained, the practice should be discontinued; for the excessive and long-continued employment of drastic purgatives, may induce ulceration of the mucous mem-

brane of the intestines, and thus add to the danger of the disease.

Throughout the whole course of the disease, it is of advantage to keep up a daily discharge from the bowels, in a mild way, as a means of counteracting the affection of the brain, which it always tends to relieve, though it may fail to cure. It is generally a favourable circumstance, when diarrhoea occurs early and spontaneously in fever; and I have often had occasion to regret the officious interference of art, in endeavouring to check this symptom, especially when opium has been used for the purpose. I have seen every bad symptom almost immediately follow this practice. It is only when the evacuation proceeds to an excessive length, or where the irritability of the intestinal canal is unusually great, that either *astringents* or *opiates* are required.

If the diarrhoea is attended with much pain, or tension of the abdomen, it will require moderate blood-letting, which, by removing the cause, namely, inflammation of the mucous membrane, tends at once to relieve the pain, and to moderate the dis-

charge ; while it is beneficial to the primary disease, the fever.

The combination of *emetics* and *purgatives*, where no objection to either of them exists, seems more effectual in checking the course of fever than either of them singly. A few grains of *ipecacuanha* and *jalap*, or *rhubarb*, answer the purpose sufficiently well. I have found no single medicine, however, equally efficacious with the *elaterium*, which I have very frequently employed with this view, in the dose of an eighth or a quarter of a grain, two or three times in the 24 hours ; and that for several days. This medicine, in the quantity stated, generally answers the double purpose, of exciting *vomiting* and *purging* at the same time. It may be worth remarking, that these evacuations are rendered more effectual in cutting short the fever, by previous blood-letting.

Among the remedies of the confirmed or *active stage* of fever, the *digitalis* well deserves to be mentioned, a medicine from which I have frequently experienced the most decided advantage. Its effect appears

to be the same as in other inflammations ; and its utility to be in proportion to the accelerated state of the general circulation. It reduces both the *frequency* and *quickness* of the pulse *, and, at the same time, often renders it fuller ; it diminishes also the excessive heat of the body. Thus it relieves some of the most important symptoms, upon which the event, probably, in a great measure depends. I have often observed it, to all appearance, keep the fever effectually in check, so as to allow it to go quietly and mildly through its course, but without seeming to shorten its duration.

The apprehension entertained by some, of the depressing power of this medicine in a disease that exhibits so many signs of general debility in the system, I know to be without foundation. The quantity of from six to ten or twelve drops of the *tincture*,

* *Frequency* and *quickness* of pulse are very distinct qualities, and ought not to be confounded together. *Frequency* alludes merely to the number of pulsations in a given time ; *quickness* refers to the suddenness of the stroke, and is indicative of inflammation. This quality, quickness, is as observable in fever as in other inflammations, and disappears as the fever ceases.

given every six or eight hours, is commonly sufficient for ensuring its good effects ; and that, in general, without risk or inconvenience. The operation, however, of such a medicine ought always to be attentively watched. What are its effects in a very advanced stage of fever, or when symptoms of *oppressed* brain make their appearance, I am not able to state.

It is in this stage of fever, when the temperature of the body, is usually much increased, that the practice of frequently sponging the skin with cold or tepid water, and the admission of cool air to the patient, are so highly advantageous. By lessening the violence of vascular action in the skin, we seem to diminish, at the same time, that of the brain ; as may be inferred from the almost immediate relief to the head-ach or delirium, often experienced from such means, where the patient had been previously confined in a close situation, and a heated atmosphere. This practice, however, though highly useful and conducive to a favourable termination, can only be considered in the light of a palliative; and there

are circumstances in which it is not admissible.

The heat of the body is not always sensibly or materially increased in fever; nor, at all, in a degree corresponding with its violence or danger; the mildest cases (as to the essential part of the disease, the *sensorial* disorder) being often attended with the greatest degree of general vascular action, and increase of heat. In weak subjects, and in an advanced stage of the disease, the extremities are often cold; and the body is readily chilled by exposure to too cool an atmosphere. I have, more than once, seen life nearly suspended by an imprudent application of cold in this way; and yet, upon the heat being restored, no diminution of the essential symptoms of the disease had taken place. On one occasion, a gentleman, on a sudden attack of delirium at night, and which was not apprehended, got out of a garret window on the roof of the house; where he was discovered, after some hours, lying in a torpid state, from which he never recovered.

Of the *cold-affusion*, or sudden and intense application of cold to the skin, as a means of interrupting the course of fever, I can say little or nothing from my own experience. Acting, as it probably does, by *counter-irritation*, I should judge it to be more likely to supersede the disease in the earliest stage, than when the fever is fully confirmed; as we find to be the case with other remedies. I doubt the propriety of Dr. Currie's rule, with regard to the administration of this remedy; namely, that it should be confined to those circumstances of fever, in which the heat of the skin is considerably above the natural standard. This is most remarkably the case, when the disease is completely formed; and then we have but little power over it. Increase of heat, it is to be recollected, is only one of the many symptoms or attending circumstances of fever, and by no means one that is invariably present. The removal of it, therefore, is not necessarily *curative* of the disease.

Blistering, as a means of *counter-irritation*, though it may possibly contribute some-

thing, in conjunction with the others above described, towards the removal of the brain-affection, possesses, I believe, but little power in this respect; and, therefore, may be safely neglected, at least in the beginning, and throughout the *active* stage of the disease. Indeed, there has always appeared to me a great inconsistency, in violently and painfully irritating the skin, at the very time that we are so studiously avoiding all powerful impressions upon the other organs of sense, as well as irritation of all other kinds.

The practice of giving *opium* in this stage of fever, in order to procure sleep, I have generally found to be injurious; by aggravating the febrile symptoms, and inducing delirium earlier than would otherwise be the case.

The *minor* points of practice, I shall not dwell upon; because I believe they contribute but little to the general result. *Antimonials* may do some good, by equalising the general circulation, where this appears to be wanting; but a great deal too much importance has been attached to them, in

the cure of fever. After many trials, I have not experienced from them the advantages, which Dr. Balfour states, in his recent publication on the subject.

Cold applications to the head appear often to have given much relief; and I think they are most serviceable, where the pain and throbbing are the most violent, and the heat of body generally much increased. Where, on the contrary, the face is pale, and the extremities cold, as is often the case in feeble subjects, and in an advanced stage of the disease, I have thought the use of *warm fomentations* of the head, from time to time, more beneficial.

Although, in the foregoing pages, I have advocated, in strong terms, the employment of blood-letting and other debilitating means, both in the *incipient* and *active* stage of fever; not only in *strong* habits, but even in *weak* ones, under proper limitations; it cannot be supposed, that this or any other remedy is adapted to all the infinitely-varying circumstances of the disease. This would be to suppose, that fever had no analogy with other diseases; all of which,

it is well known, require a diversity of treatment under different circumstances; and I can see nothing in the nature of fever, which should withdraw it from the operation of so general a law.

There are circumstances in other inflammations, to which not only is blood-letting not adapted, but in which it is positively injurious; and a very opposite mode of treatment required. When, for example, inflammation has continued for a considerable time, and when the general strength is materially reduced, either by the treatment had recourse to, or by the continuance of the disease; a moderately cordial and soothing plan of cure is often the best. Mild *opiates*, gentle *diaphoretics*, (in other words, *stimulants*,) together with the use of light and nourishing food, (*if the appetite calls for it*,) afford the best means of relief.

So it is in fever. In an advanced stage of the disease, and under the circumstances mentioned, minute quantities of *opium* and *wine*, with gentle *stimulants*, (whether in the form of *snake-root*, *camphor*, or *ammonia*, is

probably of little moment,) are, I am convinced, really useful.

It is, however, when *symptoms of irritation*, rather than *coma*, or an *oppressed* state of brain, prevail, that this is the case. When, for instance the *senses* are greatly disturbed, as when the patient fancies he sees objects floating before his eyes which he tries to remove with his hand, (*muscæ volitantes*;) or hears imaginary sounds, (*tinnitus aurium*;) when his muscles are agitated by constant tremors, (*subsultus tendinum*,) and his mind active, though disordered, (*muttering delirium*;) and, when, along with these, there is a general feebleness of action in the vascular system, indicated by a weak and tremulous pulse, and cold extremities; in such a case, I have no doubt, from experience, that the remedies mentioned are highly advantageous.

Sometimes, along with the *symptoms of irritation* above described, the pulse is found of tolerable strength and fulness, and the heat of the body pretty uniformly preserved. When this is the case, there is no inconsistency, as it appears to me, in taking

away blood to a small amount, (as to two or three ounces,) at the time that we are employing the cordial and soothing plan here recommended. I am sure that I have seen it advantageous in practice.

The inconsistency of different means, is sometimes more *apparent* than *real*; and proceeds rather from our preconceived, and perhaps hypothetical, notions on the subject, than from accurate observation of the effects. This I believe to be the case in another instance I may mention; which is, the allowing patients to take food according to their inclination, at the same time that we are largely abstracting blood, in order to check active inflammation. I have so often seen the advantage of this in practice, as to be quite convinced of its utility, however inconsistent, at first view, it may appear; and I think it admits of explanation.

The powers of the system are naturally employed and expended in various directions, and in the performance of different functions at the same time; one of which thus becomes, in some degree, a check to

the others. If the natural functions are not continued, these powers will be directed too exclusively to any diseased action that may happen to be going on ; which will thus be likely to be increased. Whereas, by putting the *organs of supply* into an active state by the moderate use of food, the attention of the system (to speak figuratively) is divided, and the disease proceeds with less activity. This I believe to be the *principle* ; but of the utility of the *practice*, I have no doubt whatever.

I am therefore, in general, very indifferent as to a patient's taking food in diseases, however active or inflammatory ; provided the food is of a simple kind, and taken with real appetite ; and provided also, the proper remedies for the disease are administered at the same time, and to the requisite extent.

Treatment of the Third Stage, or Stage of
 OPPRESSION.

This stage of fever is marked at once by *great disorder* in, and *imperfect performance* of, all the *sensorial* functions. The patient is nearly *insensible* to impressions; there is extreme *prostration of strength*, indicated by the supine posture, and the continual sliding of the body towards the bottom of the bed, together with involuntary tremors of the hands and tongue; and, lastly, early delirium followed by an almost total *annihilation* of the powers of mind. The pulse is commonly soft and compressible, though often with considerable fulness; and the heat of the body is generally considerable.

It is in this stage, that black *sordes* collect about the mouth and teeth; that the tongue turns black on its surface; and that *petechiæ*, *purple spots*, and *vibius*, with dark-coloured hæmorrhages, are apt to occur; succeeded by involuntary and foetid discharges by stool and urine. It is to fever

marked by such symptoms, that the terms *putrid* and *malignant* have been especially applied.

In former times, when persons labouring under fever were confined in a close and heated atmosphere ; and when *cordials* and *stimulants*, under the name of *alexipharmacs* were largely employed ; such a train of symptoms was by no means uncommon. At present, owing to a more judicious management of the patient, they are rarely seen. I have, however, witnessed them in more than one instance, during the present epidemic. When fever of any kind is about to prove fatal in a *simple* form, and not by *complication* with some accessory affection, it very generally presents more or fewer of these characters.

With regard to the best mode of treatment in this stage of fever, my own observation has been too limited to warrant my speaking with any degree of confidence. My attention has been more directed to the *early* stages of the disease ; when such symptoms as have been now described,

may almost always, by an active *antiphlogistic* practice, be prevented.

It is evident, that much contrariety of opinion exists among practitioners on the subject. Twenty or thirty years ago, when *debility* and *putrescency* were considered as the essential part of such fevers, the object of the physician was, to counteract these by *antiseptics*, and *tonics* and *stimulants* of the most active kind; and *bark* and *wine*, in large doses, were more especially administered for the purpose. So many well-authenticated instances, are upon record, of the favourable result of such a mode of treatment, that it would betray an unreasonable degree of scepticism, to doubt the general fact. Yet there may be a fallacy in this mode of judging, which it is necessary to be aware of; for *experience* itself, or what is usually so called, is not always to be implicitly relied on.

A certain proportion of the more *severe* or *malignant* cases of fever will terminate favourably under different, or even opposite, modes of treatment; and this will likewise be the case where the disease is, in a great

measure, left to itself; as we may infer from the cures alleged to have been accomplished by the employment of *yeast*, the *mineral acids*, and other trifling means, so that *apparent* success becomes an equivocal mode of judging; unless the enquiry is conducted with much caution, upon an extensive scale, and with due allowance for a great variety of circumstances. But it is evident, that few possess opportunities of this kind. The observations of individuals, therefore, though made in good faith, and correct as far as they go, require to be received with caution, before we venture to deduce from them a general rule.

The state of the system in the *oppressed stage* of fever, is such, as to render the effects of *stimulant* and *tonic* remedies, exceedingly equivocal. The body is, at this time, very insensible to a variety of impressions; and the effect of applications are thereby greatly lessened. Hence, when wine and other stimulants have been actively employed, and the termination has been favourable, it may still be made a question, whether such means really contributed, in any material

degree, to the result. And the doubt is strengthened, when we advert to the well-ascertained insusceptibility of the system with regard to mercury, in cases of violent fever, especially those of hot climates; as well as in brain affections of other kinds.*

I have, in various instances, attentively watched the employment of wine and other stimulants, under the circumstances mentioned; and although the case has sometimes ended favourably after their use, the contrary has so often happened, as to leave a doubt in my mind, of their real efficiency. And I am the more inclined to doubt of their utility, from finding the experience of Dr. Bateman, who has had so many more opportunities of observing the disease in this stage, decidedly against them.

After all, it is experience that must decide the point, as to the real utility of such means, under such circumstances; but it must be experience uninfluenced by pre-

* An explanation may perhaps be thus given, of the alleged fact (which many have thought incredible) that large doses of *calomel* and *opium*, combined, are useful in this state of fever.

judice, and upon a sufficiently extended scale. It would undoubtedly throw much light upon the treatment, could we ascertain with certainty the actual condition of the body, but more especially of the brain, in the peculiar state of fever here alluded to.

It appears very evident, that in the worst form of fever, that which has (in this case, not unmeaningly) been called the *typhoid* state, the *sensorial functions* are all most *imperfectly* carried on; the *sensibility* of the body is impaired, the *voluntary power* greatly reduced, and the *mind* nearly incapable of exertion. All these functions are, at the same time, in a state of more or less *disorder*; hence, the *false perception* of objects floating before the eyes, and the *noises* which the patient fancies himself to hear; the *involuntary* or *convulsive* action of muscles; and the continual *muttering delirium*. As the disease approaches to a fatal termination, the *disorder* in the sensorial functions lessens, while the *inability* to perform them increases; till it at length becomes complete; and death takes place.

Whatever may be the state of other functions, under these circumstances of fever, it cannot be questioned that the symptoms above described, refer themselves exclusively to the brain, and argue a highly morbid condition of this organ. Nor can it be denied that these symptoms are preceded, as well as accompanied, in most cases, by obvious marks of *increased arterial action*, in and about the brain; evinced by the heat, and pain, and throbbing, almost constantly felt in the head; as also by the flushing of the face, and redness of the eyes. Hence it seems reasonable to conclude, that the symptoms mentioned are merely the effect of such increased arterial action: the one is the natural consequence of the other.

Of the sufficiency of such a state of violent action in the vessels of the brain, to produce the train of symptoms mentioned, no one, I think, can doubt, who considers the peculiarity of this organ, before alluded to; and how readily any general increase of action in its arteries may have the effect of interrupting the circulation in this part, so as to impede its functions in different de-

grees, or even to suspend them altogether, according to the particular seat and extent of disease in the brain.

When, from any cause, (whether it be mental emotion, or intoxicating liquors, or exposure to the mid-day sun, or *contagion* of any kind acting in a way that we do not understand, or any of the other numerous causes of irritation to the brain,) the arteries of this part are excited to greater action, the circulation through the organ is carried on *at first* with more rapidity and violence, and its functions performed *for a time*, with more energy than natural; and the influence of this will be felt and communicated throughout the system.

But increased action of vessels is soon followed by distension and enlargement; and had the brain, like other organs, room to expand in, there can be no doubt, that its bulk altogether would speedily be augmented, and its circulation be continued without interruption. But enclosed, as it is, in an unyielding case of bone, expansion becomes impossible. Distension of vessels in such circumstances is necessarily partial, and limited in extent. If some vessels are

dilated, others must be compressed in an equal degree; and thus the circulation will suffer interruption. This however will take place unequally in different parts, and the functions of the organ be variously affected in consequence.

Whether the arteries or veins are the most affected in this case, is of little moment; the balance between them, so indispensable to a healthy circulation and a perfect performance of functions, will be destroyed. We know with respect to other parts, that when the arteries are excited in an unusual degree, their calibre becomes increased; and the same may be expected to take place in the brain. If this should be the case with the whole arterial system of the brain, the veins, as the only yielding parts, must suffer compression, and the circulation be thereby effectually interrupted, as appears to take place in some varieties of apoplexy.* But whether the

* Several cases of apoplexy, apparently from this cause, are related by Dr. Abercrombie, in the paper before quoted, in which the general circulation was feeble, and the body cold. Upon taking away blood, the pulse rose in proportion as the brain was relieved.

arterial or venous trunks are the most distended, it is plain that the *capillaries* will chiefly suffer; and it is upon the due action of these, that the functions principally depend.

The large venous trunks upon the surface of the brain are, indeed, in some measure guarded against compression, by running in the furrows formed by the different convolutions*; and accordingly, in this part, they are, in such cases, always found greatly distended after death. This unusual distension renders them at the same time tortuous in their course, by which they enter the great longitudinal sinus at an unfavourable angle for discharging their contents; and thus the course of the blood becomes further impeded. Such a state of the superficial veins is observed after death in the *sanguineous* apoplexy, in *phrenitis*, in the *acute hydrocephalus*, and in fatal cases of fever; and, doubtless, in all of them,

* This appears a more obvious and probable use of the convolutions upon the surface of the brain, than their constituting so many different organs of the intellectual faculties.

proceeds from the same first cause, an excited state of arterial action in the brain.

Of the actual state of the vessels of the brain during life, in this stage of fever, we can have no demonstrative proof. But there are the strongest reasons, from analogy, for believing that the organ is in a state of *oppression*, owing to its circulation being interrupted in greater or less degree. There is much of that kind of *stupor*, which simple pressure upon the brain is observed to occasion, combined with those marks of *irritation* and disorder, which inflammation so readily excites. It is, in fact, a mixed state of *coma* and *irritation*, very similar to that which mechanical injuries of the brain when succeeded by inflammation, are generally observed to induce.

The appearances above described, and likewise the disturbed state of the sensorial functions in fever, have been attempted to be explained by referring them to an increased *determination of blood* to the brain simply; not as the effect of inflammation, nor as the result of any peculiar state of action in the vessels of the brain itself.

What the power effecting this *determination* is, however, we are not told; nor is it easy to conceive. No increase of action in the heart or great arterial trunks, can produce a determination of blood to any one organ exclusively. The brain is, indeed, far more independent of the state of general circulation, than appears to be commonly understood. It often carries on its functions, altogether undisturbed, during the most excited state of general vascular action; as may be observed in *acute rheumatism*, and other violent inflammations; and likewise when the powers of the general system are extremely exhausted, either by loss of blood, or by long-protracted disease of other parts. But the slightest disorder in the action of its own vessels, such as hardly excites an uneasy sensation, is found sufficient to derange its functions, and, if long continued, ultimately to injure and destroy its organization.

What is called *determination of blood to the brain*, as commonly understood, and as independent of the action of its own vessels, I believe to have no existence. Every un-

usual accumulation of blood in the vessels of a part, is the result of the increased action of those vessels themselves, and not of the heart or other part of the vascular system.

The disturbance observed in the sensorial functions in fever, has by some been referred to *sympathy* with other parts; as if the brain itself were not the actual seat of the disorder. Such language, however, carries no distinct or intelligible meaning with it. Irritations of other organs may indeed act as *remote* or exciting causes of disorder in the brain; but no material change or disorder in the state of the sensorial functions can take place, without a previous change in the *physical* condition of the brain itself, and which is always to be considered as essentially the cause of such disorder. Our proper business is, to investigate the nature of the change that has taken place. It is the state of vascular action alone that we can look to, as the cause of every physical change in this part, and of every derangement in the state of its functions. This is the palpable source

of at least the chief part of such disorders ; and we have no distinct knowledge of any other.

The *oppressed* state of fever, which, when it takes place, constitutes what I have called the *third stage* of the disease, has of late been ascribed to a supposed state of *venous congestion* ; and this variety of fever has accordingly been denominated the *congestive*, as if the phenomena could be explained from such a source. It may be easily shown, I think, that such a supposition is unfounded in theory, and inconsistent with physiology ; while it is injurious, or at best useless, in its application to practice.

The idea attached to *venous congestion* is, “ that it consists in an almost stagnant accumulation of blood in some part of the venous system, *not at all depending upon increased arterial action* ;” and that “ the *brain* and *liver* are the parts most frequently and seriously affected in this way ;”—and, as applied to fever, “ that the force of the arterial system is not only diminished generally but the whole venous circulation oppressed, and particularly obstructed where the con-

gestion exists :” —and further, “that the venous system is more immediately and chiefly concerned in the phenomena of the *congestive typhus*, and the arterial system in the *simple* and *inflammatory* species.” — As to the treatment, very early blood-letting, purging, and the warm-bath, are the remedies advised, “with the view of relieving the local congestions, and of restoring the natural balance of the circulation.” *Calomel* and *opium* in large doses, are also recommended for the purpose.

It would be difficult, I believe, to find elsewhere so many unfounded notions within the compass of so few words ; and it would be not less so, to find an example of their obtaining equal currency in so short a space of time. The supposition of the existence of such a state of *venous congestion* rests only, as far as I can perceive, upon the simple fact of a turgid state of veins observed after death ; for certainly the *symptoms*, during life, give no countenance to such a supposition. These, at first, are all of an *active* kind, and denote an *excited* state of arterial action in the brain ; the very opposite of the *passive* state which the term

congestion, as here employed, implies, and which is declared to be independent of increased arterial action. The torpor or *oppression* which succeeds to this excited state of vascular action, and which is unmeaningly termed *collapse*, is easily referable to the same source. As to the proof derived from the turgid state of veins after death, this is entitled to no attention. We have no right to infer the state of vessels during life, from their appearance after life has ceased. The arteries *then* are generally found empty; the veins alone containing the principal part of the blood: but no one supposes this to be their relative condition during life.

How a state of *venous congestion* should take place “combined with a deficiency of arterial action,” it is not easy to conceive, nor is any explanation attempted. There are but two modes in which an unusual quantity of blood can be accumulated in the veins—one is, an additional quantity being propelled into them by an increase of action in the arteries, in consequence of which the corresponding veins are generally

observed to become more distended—the other is, some obstruction of a mechanical kind to the return of blood to the heart. The former of these is here expressly denied to exist; and of the existence of the latter no idea appears to be entertained. There is in the brain, indeed, in such states of fever, a mechanical impediment to the return of blood from this part, produced by arterial excitement and consequent distension, in the manner already explained; but this is the reverse of *passive venous congestion*, here imagined to take place. Allowing such a state of veins to exist, either in the liver, or brain, or any other part, how would it serve to explain the phenomena? We are told, “that the attack of the *congestive typhus* is generally sudden; with over-powering lassitude; deep pain, giddiness, and sense of weight in the encephalon, with anxious breathing.” It rests with the author of such opinions to show, how these symptoms are reconcileable with the idea of simple or *passive* accumulation of blood in veins, even though it should amount “almost to stagnation.”

How far they are consistent, or otherwise, with the principle of *excited arterial action*, for which I have contended, I leave to others to determine.

If the formidable train of symptoms alluded to, under the name of *congestive typhus*, were really occasioned by a load of blood accumulated in the veins of the brain, (though how it should get there, or how be retained, we are not informed,) the proposal made for opening the jugular, in order to relieve the gorged veins of the brain, seems feasible enough, though somewhat mechanical. But one does not so readily see how opening the temporal artery in such cases (which is advised where enough of blood cannot be obtained from the veins) should tend to unload the veins; nor how this evacuation should be at all useful, "where the force of the arterial system is diminished generally;" nor how, upon such a supposition, "the pulse should generally rise under or immediately after blood-letting."

In short, I consider what has been called *venous congestion* as an imaginary state,

resting upon no proof, and leading to injurious practice, by considering the brain as *passively* instead of *actively* affected; it is at all events taking the effect for the cause. The phenomena ascribed to *venous congestion* indicate an *oppressed* state of brain, induced by excess of arterial action, in consequence of which the circulation through the organ becomes interrupted, in greater or less degree, in the manner stated. This appears to me fully adequate to explain the symptoms, while the supposition of a *venous congestion* is both gratuitous and unnecessary.

There is another cause of *oppression* to the brain, and consequent interruption of its functions, that has not been sufficiently noticed, and which is likewise the natural consequence of *excited arterial action*, especially when long continued. I mean *serous exudation* or *effusion*, which appears to take place in greater or less degree in every fatal case of fever, as proved by dissection. This, we easily see, may be a cause of permanent oppression to the brain, so as to disorder and impede its functions, after the dis-

ordered vascular action, in which the disease essentially consists, has ceased, and when the balance between the arterial and venous systems is restored. The phenomena attending the convalescent state from fever, show a condition of this kind to exist in the brain, and probably in the spinal canal also. The dull eye, the relaxed feature, the tottering and half-paralytic gait, and the fatuity of mind, which succeed to every case of severe and protracted fever, and which very slowly disappear afterwards, admit, in my opinion, of no other explanation.

In a word, an excited and distended state of arteries,—compression of veins,—interrupted circulation,—serous effusion or exudation,—these appear to be the successive and necessary links in the chain of morbid changes which take place in the brain in the most *malignant* or *apoplectic** state of fever, (as it may be justly called,) and out of which the *mixed* symptoms of *oppression* and *disorder of functions*, arise.

* “Always the state of the brain proves, upon dissection, that those who die of fever die apoplectic.” *Principles of Surgery*, by J. Bell, 4to. page 605.

If the brain be in the condition supposed, it is no wonder that the other organs of the body should be in a scarcely animated state ; that the muscles, both voluntary and involuntary, should be in a manner paralyzed ; that the heart should lose its energy, and the vessels their power of contraction, so as to allow the blood to escape from their extremities ; and that the blood itself should appear to be deficient in the vital principle. This appears to my mind to offer a more satisfactory explanation of the phenomena of the disease than the unfounded idea of *venous congestion*, or the more mystical terms *debility*, *collapse*, *depression of nervous energy*, or *exhausted excitability*. The real changes wrought by the disease are *physical* and not *ideal* ; and, such as I have described them, appear to be consistent with the best established physiological principles.

If this be a just theory of the *oppressed*, *apoplectic*, or *comatose* state of fever, (for either of these terms is applicable,) there remains to enquire what kind of treatment it suggests, or what are the proper indications of cure. Some, doubtless, will say,

the treatment ought to be deduced from *experience*; and not from *theory*, which is uncertain and fallacious. We are told by one Professor*, who, in his recent observations on the subject, sets out with declaiming against *theory* and *proximate causes*, “as doctrines fit rather to amuse the pathologist, and to engage the student, than to appear in the capacity of prompters or of guides in our contest with diseases;” that he “has been able to draw no general rule with regard to fever, except that *generalizing is nearly impossible in this disease.*” The practitioner is to be guided, it seems, “by reflection on the circumstances of the case;” and to have recourse to “a repeated and more careful investigation of phenomena, by contrasting symptoms and circumstances together.”—But to what purpose are this *reflection* and *contrasting of symptoms and circumstances*, unless it be to deduce from them some

* Practical Observations on continued Fever, &c. by Dr. Robt. Graham, Regius Professor of Botany, &c. &c. Glasgow. 1818.

general principles? and what is this but generalization, or *theory*?

If *theory* is liable to the charge of uncertainty, *experience* is no less so; as is evident from the contradictory nature of what are called *facts* in medical history. *Experience*, so called, is often nothing more than individual assertion, founded on partial and limited observation, warped by prejudice, and not unfrequently distorted by wilful misrepresentation, as the records of medicine too clearly prove. Hence *experience* is often a not less fallacious guide in practice than *theory*; while its errors are more difficult of detection. When such is the case, we can only escape from the labyrinth of conflicting testimony, by the clue of general principles, or *theory*, which is, or ought to be, a simple deduction from well-ascertained facts. Where, as with regard to our present subject, such manifest contradiction exists, there seems nothing left to direct our steps in practice, but a cautious employment of those means which appear consonant to the obvious nature of the disease; in other words, taking physiology

as our guide, we should bring all the suggestions of theory to the test of careful and repeated observation. Thus will theory and practice serve mutually to illustrate and confirm each other.

Upon the view now given of the condition of the brain in the third or *oppressed stage* of fever, namely, that it consists in a partially-interrupted state of circulation in this organ, the result of increased or inflammatory action of vessels, which still subsists, in greater or less degree, the object to be aimed at in the cure is sufficiently obvious, namely, the restoration of the circulation to its natural state. The brain, in this stage of fever, appears to be suffering more from the immediate consequences of inflammation, than from the inflammation itself, though in some degree from both. When the circulation through the organ is interrupted, and the functions consequently impaired; and when effusion has perhaps taken place, if not alteration of structure; it would be unreasonable to expect the same advantages from blood-letting, or other antiphlogistic means, as at an earlier

period of the disease. Such practice might even be injurious, especially if carried to any considerable extent. By reducing too much the powers of the system, although it should put a stop to the excited vascular action which was the primary source of the mischief, it might lessen the disposition to contract in the arteries, upon the return of which to their natural dimensions the restoration of the circulation entirely depends. Thus the indication appears to be two-fold; to reduce the inflammatory action, which is the first cause of the interrupted circulation; and then to restore the arterial system of the brain to its natural condition; that the veins may be set at liberty, and enabled to transmit with freedom the arterial blood. The means of accomplishing these different objects will necessarily differ in some degree, and may even be in opposition to each other: the attempt to relieve the one condition, might aggravate the other. Unquestionably, the first and most important indication to be fulfilled, is the putting a stop to the inflammatory action of vessels; when this is ac-

complished, the arteries may in general be expected to return spontaneously to their natural size; and it is not certain that we have it much in our power to forward this. A certain vigour of action throughout the system, however, would seem to be favourable to the purpose; and it is in this way, I apprehend, that moderate stimulation by wine and the like, may prove really useful.* This appears less hypothetical than the assumption that stimulants act in such cases by giving strength to the system, an opinion altogether inconsistent with physiology. We can easily understand why *small bleedings* should do good in such circumstances of fever, and that they may be advantageously accompanied or followed by a moderate use of cordials and stimulants. The observations of Dr. Mills † afford very sufficient evidence of the utility of small and repeated abstractions of blood in the

* This seems to be illustrated by the well-known effect of a moderate use of wine in relieving the head-ach, and other marks of disorder in the brain and general system, *consequent* to intoxication.

† An Essay on the Utility of Blood-letting in Fever. By Thomas Mills, M.D. Physician to the Fever Hospital in Dublin. 1816.

advanced and *oppressed* stage of fever; and these are confirmed by the advantage derived from the application of leeches in these cases, with the view of relieving *topical congestion*, as it is called, a practice frequently employed by those who are averse to general bleeding. In such cases, warm fomentations of the head would probably assist, in disposing the arteries to assume a natural mode of action; and I have frequently employed them, with apparent advantage in the advanced stage of fever, as in other deep-seated inflammations. It is to be recollected, too, that the brain, in these cases, is unequally affected by the disease; some of its functions being in an active though disordered state, others suspended or greatly oppressed; so that different indications may be required with regard to different parts of the organ. This furnishes an additional argument in favour of the combination of remedies above alluded to.

In the *comatose* state of fever here described, it is the general practice to employ extensive *blistering*, with other active sti-

mulants. in order to rouse the system. The object, in this respect, has been attained; but seldom, as far as I have been able to perceive, with any real or durable advantage. Such practice is directed merely to the relief of a symptom, and does nothing towards obviating the cause; on the contrary, it seems rather calculated to increase this. The state of oppression in the brain may not improbably tend to repress the violence of arterial action; and thus prove remedial, by checking the further progress of the inflammation. If so, all attempts to rouse the patient prematurely, are more likely to prove injurious than the contrary. I believe that many of the cures which the practitioner takes credit to himself for, in this state of fever, are spontaneous, and quite independent of his efforts, which are often rather prejudicial than otherwise.

On some rare occasions, the *oppressed* state of brain, above described, takes place at the very outset of fever, a state that has been not unaptly compared with intoxication, and which strongly resembles some

varieties of apoplexy. In so early a stage of the disease, I should not hesitate to have immediate recourse to blood-letting, and with the greatest freedom, for the purpose of diminishing arterial action in the brain, and thereby lessening distension. At this period, the relief might be expected to be immediate. At the same time, I should endeavour, by warmth and general stimulants, to excite a greater degree of vascular action over the system at large, in order to lessen the determination of blood towards the brain.

Now and then it is observed, in these violent and malignant cases of fever, especially in old subjects, that the febrile symptoms decline, and the brain appears to have its functions restored in a considerable degree; yet the patient sinks rapidly, and sometimes dies without any mark of active disease, and when all apprehensions of danger have perhaps subsided. In this case, the pulse becomes extremely soft and weak, so as to be compressible by the slightest force; the flesh altogether is relaxed and flabby; the extremities cold, unless their

heat is artificially supported ; the tongue is moist and free from fur, yet not of a healthy appearance ; the secretions in general are copious, as if from relaxation of organs ; the eyes are dull, the pupils large, and the features all relaxed ; there is freedom from pain, the patient appearing to be overwhelmed by languor ; he is slow in answering, but yet intelligent ; and sometimes there is great restlessness.

This state occurs most frequently, as before observed, in old subjects, but is by no means confined to such ; I have seen it at various ages. If ever the term *collapse* admits of proper application, it is to the state of system just described, which seems to arise in part from *exhaustion*, the consequence of long-continued excess of action in the vessels of the brain ; but partly, also, from the effects of this, namely, effusion, and perhaps alteration in structure,—the natural results of inflammation. On some occasions, I have thought it proceeded from a too free use of the *digitalis*, the operation of which always requires to be attentively watched.

Such a state requires the use of stimulants and *tonics*, in regular but guarded doses, together with the exhibition of such food as is most grateful to the patient; by these, conjointly, the system will not only be temporarily excited, but its powers gradually and permanently recruited, in proportion as the waste the body has sustained is supplied.

It is very possible, however, to misapply the means of relief, either by irregularity or excess in the employment of them. Such a state as I have described is certainly not one for the use of large quantities of wine, or other stimulants. A spoonful or two of wine, (making allowance for previous habits,) and that at intervals of an hour or two, will be generally found sufficient: and the intervals should be prolonged, as the signs of returning energy appear. The *ammonia* and spices, and probably the *cinchona*, in small quantities, are likewise proper; but nauseous and disgusting medicines of all kinds ought to be avoided. If there is much restlessness, five or six drops of the *tincture of opium* at night, or even once or twice in the day, will be useful, in addition to the other means.

OF THE COMBINATION OF THE DISEASE WITH
OTHER AFFECTIONS, CONSTITUTING THE
COMPLICATED STATE OF FEVER.

A LARGE proportion of the cases of the present epidemic, that have come under my observation, have been complicated with some other inflammation, a circumstance that often modifies considerably the character and tendency of the disease, and which has been the cause of introducing much confusion into the pathology of fever in general.

The previous existence of another disease has not always secured the patient from the present epidemic. I have seen it attack persons in an advanced stage of phthisis, and in various other diseases. When this has been the case, the fever itself has not in general assumed a more unfavourable character, while it has commonly had the effect of suspending, or at least of mitigating, the original disease. This effect, however, has lasted no longer than the fever itself.

The throat, the skin, the air-passages, the thoracic and abdominal viscera, and their respective coverings, and likewise the ligamentous structure, have all, in turn, suffered; and probably no part is altogether exempt. It depends much upon season, which of these parts will be attacked. In the winter and spring seasons, when catarrhal, pulmonic, and rheumatic affections are apt to prevail, these are the combinations generally observed. Towards the end of summer, and in the autumn, the abdominal viscera have most frequently suffered, the fever then being frequently combined with cholera, diarrhoea, or dysentery, and sometimes with inflammation in the liver, or peritonæum.

These *accessory* inflammations have sometimes accompanied the fever from its commencement, and have even appeared, on some occasions, to precede it; or at least, have been the first noticed: more frequently, however, they have arisen during its course. On many occasions, they have subsided again, while the fever has continued its progress.

Such combinations are in general easily detected, by the pain and disordered function in the part thus secondarily affected ; in addition to the brain-affection, which constitutes the fever itself, and which is always present. We are more likely, in such cases, to overlook the fever, than the secondary inflammation, on account of the greater degree of pain which the latter often gives : hence it is that, in explaining the disease, an undue degree of importance has been frequently attached to what is merely a secondary and accidental occurrence.

These *complicated* states of fever have sometimes been called by particular names, as if they constituted different and distinct species of disease. The terms *pneumonia typhodes*, *dysenteric* fever, *puerperal* fever, and the like, denote only the combination of fever with *pulmonic*, *intestinal*, or *peritonæal* inflammation. And thus we understand how these diseases come to be contagious ; and what is to be understood by the expression of an “ inflammation assuming a *typhoid* form.”

Inflammation, arising in the course of fever, may affect it very variously; sometimes adding to the danger of the disease, sometimes the contrary. On some occasions, it proves *critical* to the fever; the latter immediately subsiding, as the accessory disease appears.

The combination of catarrhal, or even pulmonic inflammation, if slight, with fever, has not always seemed to add to the danger of the latter; sometimes rather the contrary, the brain affection appearing to be mitigated by the combination. This is still more decidedly the case, when *rheumatism* has taken place along with the fever; the secondary affection in this case being in itself of little moment, while it tends, by a kind of *counter-irritation*, to diminish the violence of vascular action in the brain.

The combination of *abdominal* inflammation with fever, is of a more formidable nature; at least when either the alimentary canal, or the *peritonæum*, is the part affected.

In the autumnal season, when all the forms of abdominal inflammation are frequent, they are often found in combination

with fever; of which many instances fell under my observation during the last autumn. Inflammation of the stomach, or of the intestines, I believe to be rare, as an accompaniment of fever in this country. Inflammation of the lining membrane of this canal, in the form of *diarrhœa*, is however very common; and in most cases it has appeared to mitigate the violence of the fever. Hence, I have been always cautious of restraining such discharges, and have had occasion, more than once, to regret my having done so. There is no doubt, however, that *diarrhœa* may proceed to too great a length, so as even to be followed by ulceration of the internal membrane, of which instances have appeared upon dissection.

Peritonœal inflammation not unfrequently arises in the course of fever, especially in the latter stages of the disease; such a combination is always an unfavourable one, and attended with more or less of danger. It ought always to be suspected, and narrowly watched for, by frequent examinations of the abdomen; more especially as the patient

at this period is often unconscious of, or incapable of describing, his sensations.

It is worthy of notice, how much, on some occasions, the pain of inflammation is obscured by the presence of fever; so that the inflammation often proceeds to a fatal length before it is even suspected. An example of this is afforded in the extensive gangrene that occasionally takes place on the parts of the body subjected to much pressure; and the same is not uncommon even in the abdomen.

In several instances, towards the close of the fever, and when it has begun to decline, signs of inflammation have appeared in the chest; as cough, with slight pain, and difficulty of breathing. The *pyrexia*, or febrile symptoms, are thus excited afresh; but, in consequence of the cessation of the affection of the brain, they undergo a change in their character. The tongue is no longer brown, but becomes covered with a white fur. The black *sordes* disappear from between the teeth and corners of the mouth; the lips assume a florid hue; the sallowness of the cheeks is succeeded by a bright red; the

headache ceases, and also the delirium ; appetite and sleep return in some degree. The pulse is entirely changed ; from being soft and undulating, it becomes hard and contracted ; and hectic symptoms, altogether, make their appearance.

This change takes place almost imperceptibly ; and we are apt to wonder, why the patient makes so little progress towards recovery. The reason is, that a new disease has arisen, while the fever was silently withdrawing itself ; and the patient thus often slides insensibly into a state of irremediable *phthisis*, before the nature of the new disease is suspected or understood, and before the fever is even believed to have ceased. The danger is much enhanced in this case, if wine or other *stimulants*, and *tonics*, are persevered in, with the mistaken view of supporting the system under the debility occasioned by the preceding fever. The only chance of arresting the progress of the new disease, is the immediate adoption of the *antiphlogistic* plan ; as by very small bleedings, the *digitalis*, and blisters to the chest ; allowing the use of plain food, as the appe-

tite may require, but prohibiting every thing of a *stimulant* nature.

When blood is drawn in this state, (which I have often directed to be done, to the extent of two or three ounces at a time, with the most decided advantage,) it is always found *buffed* and *cupped* in a high degree; whereas, as long as the fever lasts, the blood when drawn presents a very different appearance, as I have before remarked: the *crassamentum* is covered with a gelatinous crust, which is often tinged of a reddish colour; and it is never *cupped*, or contracted on its surface, but spread out to the whole extent of the vessel into which it is received.

As fever is often accompanied by other inflammations, and is probably the direct cause of them, by the disturbance it occasions in the general vascular action of the system, so the reverse of this sometimes appears to take place. Erysipelas thus often appears to excite fever; and the same is now and then observed in pneumonia and other inflammations. Such an occurrence is discovered by the accession, at some

period of the disease, of the proper symptoms of fever, such as headache, restlessness, delirium, prostration of strength, and a brown fur upon the tongue, together with softness and feebleness of pulse. The inflammation is then said to assume the *typhoid* form.

In the treatment of fever in general, when complicated with other inflammations, we must be guided by the nature and importance of the accessory affection; and also, as in *simple* fever, by the stage or period of the disease.

When the secondary affection is in itself of little moment, it of course requires no particular attention to be paid to it. If otherwise, it must be treated by the common remedies of inflammation, but with greater caution in reference to the stage of the disease. When it appears at an advanced period of the fever, *blood-letting* will be either not admissible, or must be very sparingly used. I know, however, from experience, that, unless in very unfavourable circumstances, the taking away two, three, or four ounces of blood, in such

cases, is in itself unattended with danger, while it is a most powerful means of relief. The fear of its adding to the danger, by increasing debility, is in general without foundation. The danger arising from the continuance of the inflammation is of a far more formidable kind. I prefer *general* to *local* bleeding, by leeches, in these cases, because a smaller loss of blood will suffice; while we avoid all uncertainty as to the quantity taken. In slight cases, fomentations, blisters, or other local means, may be all that is required.

The frequent combination of fever with inflammation, has led many so far as to admit that fever, as I have contended, is *always* a consequence of inflammation; but they deny that the *brain* is the seat of this, more than other organs.

Others again, while they allow inflammation to be a frequent, and even general attendant of fever, deny it to be any thing more than an accidental circumstance; whether it be found in the brain, or in any other part.

There is a third class also, who suppose the fever to be the *primary* disease, and the *cause* of the inflammation of the brain, which they allow to be often present.

Thus, all make approaches, though on different sides, towards the doctrine I have endeavoured to support. All connect *fever*, more or less, by a kind of relationship, with *inflammation*; though they differ in the degree of *consanguinity*, which they suppose to obtain between them.

Were there any general conformity of opinion, as to the nature of fever, among those who have opposed the doctrine which I have espoused, it would much weaken the confidence I feel with regard to it. But as the contrary is obviously the case, I am not disposed to abandon a theory, which, while it is consistent with physiology, appears to me to offer a more satisfactory explanation of the phenomena of the disease, and to suggest a more rational, as well as successful, mode of cure, than any other that has yet been promulgated.

To those who deny the brain to be more frequently, or essentially, the seat of the in-

inflammation which produces fever, than other organs, I reply, that in every fever, *simple*, as well as *complicated*, signs of disorder in the brain appear, and are *pathognomonic*, or characteristic of the disease. They exist throughout the whole course of it, and are always in proportion to the violence and danger of the fever. Whereas inflammation in other parts, during fever, is of casual and uncertain occurrence, and discoverable by the ordinary signs; which are always *in addition* to the *proper* symptoms of the fever itself. And hence the *complicated* form of the disease. In fevers that are really of a *simple* kind, there may be much general disorder observed in the system at large; but there are no signs of existing inflammation excepting in the brain; and *here*, I maintain, they are never wanting.

Or, to state the proposition in a different way:—In *simple* fever, the *brain* is the only organ that exhibits signs of topical inflammation; as indicated by *increase of heat, pain, and disordered function*. In *complicated* fever, the same signs of disordered brain are observed, but combined with marks of

inflammation in some other part. These last indeed, by the greater pain they occasion, throw a degree of obscurity over the fever, so as often to cause it to be overlooked. But no one of accurate observation can confound a simple inflammation of the lungs or other organ, with the same disease as combined with fever. The symptoms, taken altogether, in the two cases, are widely different.

To those, again, who allege, that the occurrence of inflammation in fever is merely *accidental*, and not *essential* to it, I would oppose the general history of the disease. There are observed in every case the usual signs of inflammation, viz. topical pain, with disordered functions ; together with *pyrexia*, or that general train of symptoms that constitutes the *febrile state*, and which so generally attends inflammation wherever seated.

As to the third class of objectors, or those who believe fever to be primarily an universal disease, or disease of the whole system ; and that it *causes*, rather than is *caused by* inflammation ; it appears to me, that they have not studied sufficiently the first ap-

proaches of the disease, nor the order of occurrence of the different symptoms.

Wherever I have had an opportunity of watching the earliest symptoms of fever, I have invariably found the *local* to precede the *general* ones; and this sometimes for many days. Persons sufficiently intelligent and attentive to their feelings, who have gone through the disease, have, when questioned, always expressed to me their conviction, that such was the case; and in the number of those, I include many medical practitioners, whom I have attended, while labouring under fever, during the course of the present epidemic.

REMARKS ON SOME OF THE OPINIONS OF DR.
BATEMAN, ON THE SUBJECT OF THE PRESENT
EPIDEMIC.

THE little success that has followed all the attempts hitherto made to discover the intimate nature or essence of fever; that is, to show its relation to *physiology*, or in what it *physically* consists; has thrown a discredit upon *theory* altogether, as if it were incapable of reflecting any light upon the nature of disease, or of promoting the success of our means of cure. We are accustomed to hear *observation* and *experience* cried up, as the only sure and safe guides to successful practice. If this were true, the rules of our art at this time of day, instead of being vague and uncertain, would be precise and determined; for we have had no want of acute *observers*; nor is there any deficiency of recorded *experience*. On the contrary, the result of this, under the name of *facts*, has been accumulating from age to

age, till it has become an overwhelming mass of confusion ; so blended with misrepresentation, and distorted by exaggeration, that it is difficult, and almost impossible, to deduce from it any general principles, or to distinguish truth from error. It is full time to rest here, and to make a select use of the materials already in our possession ; without further encumbering the subject with a laboured detail of individual cases, and ever-varying circumstances. Description may be more minute than useful. It is, I conceive, *general views* that are wanted ; and from these, our *rules of practice*, as well as our *theories*, must be drawn. Diseases remain intrinsically the same, however they may be varied in complexion by external and trivial circumstances.

The fevers of *hot* climates are *essentially*, that is, *physiologically*, the same as those of *cold* ones, and must be treated upon the same general principles ; though with modifications, which experience has sufficiently pointed out. The same I believe to be the case with the *epidemics* of one period,

as compared with those of another. The general doctrine, with regard to them, is the same, however they may differ in the minuter points of character and treatment. But this difference can only be ascertained, by observation and experience, at the time of their occurrence, and cannot be made the subject of any general rule.

If the attempts hitherto made towards the investigation of the nature of fever, have been less successful than many other subjects of medical enquiry, the cause is to be found in our comparative ignorance of the brain and its functions. But are we, therefore, to abandon the pursuit as hopeless? Has no step been gained towards the attainment of the object? I venture to answer, there has. It is doing much to have shown, or at least to have rendered it probable, that the *brain* is the *primary* and *essential* seat of the disease; and that the affection, (if it be not actual *inflammation* of this organ, as I believe it to be,) at all events, admits of the treatment adapted to inflammation; and, under proper circumstances, with the most unequivocal advantage. These

are points in which I am joined in sentiment with a large number of physiologists and experienced practitioners of the present day. Nor is it required, that we should stop here. We are, I feel confident, in the true path of enquiry, and shall ultimately, I trust at no distant period, attain our object.

The frequent connection of fever with inflammation begins now to be generally seen and admitted. How little was this the case, even but a few years ago! The nature of the connection, however, is not yet sufficiently understood; and to this, in my opinion, is to be attributed a less efficacious use of remedies, in themselves proper, but which require the adoption of the general principle for which I am contending, in order to our deriving from them the greatest possible advantage. This, I think, is the defect of the otherwise valuable work of Dr. Bateman, upon which I am about to submit the following observations; which, being made with an entire conviction of their practical importance, will, I have no doubt, be candidly received

by him. I shall confine myself nearly to the order which he has himself adopted.

Dr. Bateman contends for the identity in nature of the various fevers which, at different periods, have prevailed, to a greater or less extent, and with more or less violence and fatality, in this country, and to which a variety of appellations have been affixed; such as *putrid, malignant, pestilential, jail-fever*, and the like, together with the *slow nervous* fever of Huxham; all of which, he thinks, spread themselves, under certain circumstances, by *contact* or *effluvia*, and are essentially the same disease; modified by changes in our habits and manners; and by the means of cure, and of prevention, which have been in use at different times.

This opinion, I conceive to be sufficiently probable; and, if true, it is plain that all the varieties mentioned admit of an application of the same general principles. A different opinion, however, has often been entertained; namely, that the cause of the disease is different in its nature at different times; and that upon this depends the difference observed in the character of different

epidemics. This is a point which it is obviously not possible to decide absolutely. It is at least certain, that various other circumstances are sufficient to modify greatly the character of the disease, and to influence, more or less, the treatment.

The arrangement of fever by Dr. Cullen, into different *genera*, under the names of *synocha*, *synochus*, and *typhus*, is here condemned, and I think, with justice, as exalting what are not even *species*, (in the language of *Natural History*,) but mere *varieties* depending upon external and accidental causes, into the rank of *genera*; and thus suggesting the idea of a radical difference, where none in reality exists. Dr. B., with some late writers, rejects *synocha*, or *simple inflammatory fever*, altogether, as having no existence in nature. It appears to me, however, that this variety has just as good a claim to be retained as any of the others here enumerated.

If, as is admitted to be the case, *synochus* is the same disease as *typhus*, only with more *inflammatory symptoms* in the beginning, *synocha*, I contend, is likewise the

same, with still stronger marks of *inflammatory action*, which continues throughout the disease. Dr. Gregory, as here quoted, was no doubt justified in asserting, ‘that, during 30 years’ practice, he had never seen a purely *inflammatory fever*, unconnected with *acute inflammation of some organ* ;’ for febrile symptoms, I believe, never arise, but as a consequence of topical inflammation. But I have often seen, in young and vigorous subjects, in the spring season, and in the country especially, a fever strictly corresponding with the definition of *synocha*, as given by Dr. Cullen* ; where the ‘*sensorii functiones parùm turbatæ*’ served to mark the organ principally affected, while the general increase of heat, and violence of circulation, indicated sufficiently the nature of the disease, as consisting in active inflammation. There is nothing wanting in Dr. Cullen’s definition, to make it complete, but *pain in the head*, which, in this form of fever, is probably a more constant symptom than any other.

* Definition. *Synocha*. Calor plurimum auctus ; pulsus frequens, validus, et durus ; urina rubra ; sensorii functiones parùm turbatæ.

Dr. Bateman thinks the term *typhus* the best appellation, not only for the present epidemic, but for continued fever in general, as it appears in this climate. In my opinion, it is one of the worst that could have been chosen, as a *generic* term; since, whether we regard its derivation, or the use that has been made of it at different periods, it admits of no general application to the disease; applying merely to some of the varieties, and those by no means the most frequent. In its original application, it referred only to stupor, which is far from a frequent attendant of fever in this country, and which, when it does occur, is commonly one of the latest symptoms.

As defined by Dr. Cullen*, the term *typhus* admits of a still less general application, at least to the present epidemic; which, in numerous instances, appears to originate in *other* causes than *contagion*, and in which

* The definition of *typhus*, given by Dr. Cullen, is—
“ Morbus contagiosus; calor parùm auctus; pulsus parvus, debilis, plerumque frequens; urina parùm mutata; sensorii functiones plurimùm turbatæ; vires multum imminutæ.”

the heat of the body is often *greatly* increased,—the pulse *full* and *strong*, especially in the beginning of the disease,—the urine *variously* altered,—and the sensorial functions, in many cases, but *little* disturbed,—all in opposition to the definition quoted below.

The greatest objection, however, to the term *typhus* is, its being intimately associated in the public mind, and even in that of many practitioners, with the idea of *debility*; in consequence of which, stimulant remedies suggest themselves for the cure, with the avoidance of all debilitating means, but particularly blood-letting; although it is now proved incontrovertibly, that such means, properly applied, are of the greatest efficacy in subduing the disease, and the only ones, indeed, that are decidedly curative. I can see no reason, therefore, why such an unmeaning term, as *typhus*, (unmeaning, as applied to the greater number of cases, and as to all, during a great part of their course,) should be allowed to supersede the more familiar and intelligible one of *infectious* or *contagious fever*; or even the terms *putrid*, *malignant*, *slow-nerv-*

ous, and the like; all of which express some obvious character of the disease, and which cannot well be misunderstood.

In the third section of his work, Dr. Bateman treats of the '*character and varieties of the fever.*' In this are contained his general views of the nature of the present epidemic, and, indeed, of fever in general. It will here be seen, I think, that the conclusions he has arrived at, are not exactly those which his premises warrant; and that, according to the admissions he has made, he ought, in consistency, to have gone further, and adopted the whole of the doctrine I have supported.

Taking the term *typhus* as the generic appellation, he has followed the division made by some recent writers, into *simple* and *complicated*—the *simple* being that "in which no organ is particularly deranged;" the *complicated*, that "in the course of which, 'some one or more of the important organs become affected by inflammation.'"

Agreeable to this division, he proceeds to describe the *simple* typhus, or that in

which, as he had before observed, “no organ is particularly deranged:” and he makes a sub-division of the subject, according to the *degree* in which the symptoms present themselves.

In the *mildest* form, — such as occurs principally in children, or young subjects*, — along with general febrile symptoms, are mentioned, “*headache, universal pains, lassitude*, and sometimes, though seldom, a *tendency to delirium*, or slight *confusion* on waking.” Now, are not these, I would ask, “signs of an organ particularly deranged?” Can such symptoms be referred to any other source, than a disordered state of brain?

* I think it of importance to remark here, that, according to my observation, the disease has not been, generally speaking, milder, or attended with less danger, in young subjects, than in adults. Some of the worst cases I have seen have occurred in youths and girls at school, from the age of twelve to fifteen. Blood-letting in these, in some mode, and according to the habit of the patient, I consider as imperatively called for; and I have practised it with freedom in these cases, and with the utmost advantage.

In adult subjects, Dr. Bateman observes, the symptoms were generally *somewhat more severe*; and then, in addition to severe headache and pains in the limbs, he notices *sleeplessness, or disturbed dreams*, in general, without actual delirium. And after the headache had subsided, or been much alleviated, "the head has still," he remarks, "been the seat of distressing sensations; as loud *tinnitus*, or *vertigo*, or a sense of weight or load, or of increased bulk, the head seeming 'as large as two,' according to the language of the patient." Here again we see, that just in proportion as the disease increases in violence, the affection of the brain becomes the more strongly marked. The disease, it seems, existed in one or other of the mild forms here described, in nearly two-thirds of the cases that came under his observation.

The author proceeds next to describe the "*more severe or complicated* form of the disease." I cannot avoid noticing in this place, the obvious impropriety of using the terms *severe* and *complicated*, as if they were synonymous. A fever may be *severe* with-

out being *complicated*; and *complicated* without being necessarily *severe*.

An observation is here made, of importance, both in regard to the theory and the treatment of the disease; namely, that the greater severity of this form was not, in general, manifest from the early symptoms. During the first week or ten days, or even later, the disease exhibited the ordinary appearance of the more *simple*, or, as I should say, the *slighter*, degree of fever, above mentioned. Then, "a considerable increase of languor and debility was complained of, and was very obvious to the attendants. The voice became more feeble, and the speech slower; the tongue dry, with a brown streak in its centre; the pulse smaller, and more compressible, (but quicker and with a slight sharpness in its beat); the eyes dull, and the countenance heavy; and the patient sunk more decidedly into the supine position. This change was speedily followed, or actually accompanied, by more manifest disturbance of the *sensorium*, and of the functions dependent upon it. The first sign of this was, a

confusion or wandering of mind, especially in waking from slumber: this at length increased, so as to continue through the night, or even during the day. In many cases, this intellectual disturbance was an approximation to stupor, rather than to active delirium; a dulness or inaptitude to receive external impressions: the patient was slow in comprehending questions put to him, as well as in replying to them; would give an incorrect answer, or half an answer, and then appear to be lost, or mutter a few unintelligible words. In some cases it amounted to actual aberration, or incoherence of ideas; the patient either misapprehending, or being altogether insensible to questions, and muttering constantly, even when not addressed. With this state of confusion and slighter stupor, there was often a considerable degree of drowsiness, particularly in the day-time; sometimes a great disposition to moaning; though the patient was commonly unable to refer to the particular seat or cause of complaint."

“ In a few cases, the delirious rambling about the ninth or tenth day became much more violent, the patient incessantly talking louder, singing, roaring, and making various noises, night and day, but more especially during the night. This more noisy delirium, as well as that accompanied by continued muttering, was usually attended with great watchfulness. In this case, also, there was generally a great restlessness and disposition to pick or pull about the bed-clothes; and, where the strength permitted, to get out of bed. There was commonly also a slight degree of *subsultus* of the muscles; and the tongue, when protruded, trembled.” The cases in which the fever existed in the severe form, now described, amounted, it appears, to about *one-third* of the whole. It should be observed here, that the different forms described above by no means comprise the whole extent of the epidemic, or even a majority of the cases that occurred. There are a great number, as I have before remarked, in which the disease is never fully formed; where the patient complains only

of slight headache, listlessness, loss of appetite, and, perhaps, disturbed sleep; which subside altogether after a few days, often spontaneously. In these, the disease retains its *local* character throughout, without proceeding so far as to disorder, by sympathy, the *general system*. Such cases, of course, are rarely seen in hospitals; though they frequently occur in dispensary-practice.

The truth and accuracy of the picture exhibited above, will be readily admitted by every one at all conversant with the disease. And it cannot be denied, that the symptoms mark still more decidedly than the former, the disordered state of the *sensorium*; they are, nevertheless, of precisely the same quality or kind with those before described, as occurring in the milder forms of the disease; and thus we find that in every degree of the fever, the disordered condition of the *sensorium*, (the brain,) is made apparent, in the disturbed state of its functions: how then can it be said, "that no organ is particularly deranged?" If there were a single instance, even of the

most *complicated* kind, in which such symptoms, in greater or less degree, did not occur, I should abandon the doctrine for which I am contending, as untenable. Till this, however, is shown to be the case, I think myself justified in considering the brain as the principal and essential seat of the disease.

Having noticed some other circumstances of the fever, as the state of the tongue and pulse, with the occasional but rare occurrence of *petechiæ* and *vibices*, Dr. Bateman proceeds to mention the affection of *other* organs.—This, it may be observed, is what alone properly constitutes the *complicated* form of the disease.

“ At various periods of the disease, sometimes on the second or third day, more frequently in the second week, or still later, towards the close of the fever, some of the important organs of the body, become more particularly disturbed in their functions; manifesting signs of a greater determination of blood, or of a certain degree of inflammatory action. The brain, and the organs of respiration, appear to be

most liable to this partial injury; but not unfrequently the intestinal canal, the liver, the peritonæum, the tonsils, the trachea, and even the bladder, suffer in a similar manner."—P. 61.

We might be apt to imagine from this statement, that the organs here enumerated were affected indifferently, no one *necessarily* more than the rest, and all in their turn, agreeable to the idea generally entertained, of the *universality* of fever. But it is only necessary to refer to the history quoted above, in order to prove that a disturbance in the *functions*, and consequently a *disordered state*, of the brain, existed in every form and stage of the fever; whilst a disorder of other organs was occasional only, and by no means a *constant* or even *general* attendant of the disease.

This is a circumstance that makes the widest difference between the brain and other organs, in regard to fever; and furnishes the principal argument in favour of the *topical* nature of the disease, as seated in the brain. In the *simplest* form of fever there is often no perceptible mark of

disease in any organ, excepting the brain; neither in the organs of respiration, nor of circulation, nor of secretion. These functions, indeed, may be, and often are, imperfectly performed; because all parts depend more or less for their energy upon the brain. But the brain is the chief and almost constant seat of complaint in fever, at least as long as the patient is capable of describing his sensations; while the *functions* of this organ are *invariably* observed to be deranged, throughout the whole course of the disease, in greater or less degree; and always in proportion to the violence and danger of it; as is evident from the account here given, as well as from the general history of fever.

In endeavouring to explain the symptoms above described, Dr. Bateman observes, that “in all the instances of the more *severe* forms, in which considerable *confusion* and *delirium* occurred, the brain *must undoubtedly be considered as the seat*, at least of much *over-action* and *over-distension*, if not of *some degree of inflammation*,” and he adds, — “the decided relief afforded by

antiphlogistic remedies, if applied sufficiently early, leaves little room for question upon this point." — Pp. 61, 62.

Now it appears from the author's statement, that *one-third* of the cases were of this *severe* kind ; and, therefore, according to his own admission, the fever, in this proportion at least, must be considered as *connected with*, if not *dependent upon*, inflammation of the brain, or something very near akin to it.

But it is well worthy of remark, that the signs by which he here determines inflammation to be present in the brain, are not those usually attributed to *phrenitis*, or which enter into the definition of this state, as given by nosologists; they are merely the *ordinary* attendants of fever, whenever it exists with a certain degree of violence. It is plain, therefore, that the *phrenitis* described by writers is not, in the opinion of Dr. Bateman, the *only* form of inflammation of the brain; and, admitting this to be true, as is undoubtedly the case, we are compelled to conclude, either that the symptoms observed in the *severer* forms of the fever

arose out of, and were produced by, the inflammation of the brain, which is here admitted to have taken place; or else, that extensive inflammation of the brain may exist, without giving rise to *any* symptoms—a monstrous supposition. The inference, then, seems perfectly fair, that the symptoms of *severe* fever are the symptoms of *inflammation of the brain*; and, consequently, that *violent* fever is nothing but inflammation of this organ.*

In a subsequent page, the author speaks still more decidedly of the connection between the aggravated state of fever and inflammation of the brain. His words are, “the apparent debility is, as I have before stated, the result of an *oppressing* cause; which being removed, the system recovers

* This opinion of the nature of *violent* or *malignant* fever was entertained by Mr. John Bell, and expressed in his usual impressive manner. “Nothing else,” he observes, “but the most violent inflammation of the brain can possibly account for the symptoms of malignant fever.” Such testimony, from an observer unbiassed by any pre-formed theory, must be allowed to have much weight.

by its own powers ; and *that this cause is a state approximating to inflammation in the sensorium*, both the condition of its membranes after death, and the manifest benefits of moderate evacuation, local and general, which I have witnessed, seem to me to prove beyond all dispute." This is a sufficient answer to those who still contend, that the debility observed in fever is the direct effect of the contagion operating upon the *vis vitæ*.

Upon the above passage I would remark, that as the symptoms which led the author to draw such a conclusion are the never-failing ones of fever, whenever it exists with a certain degree of violence, so as to threaten a fatal termination, or even to become dangerous ; as they are *not* the symptoms usually ascribed to *phrenitis* ; and as they are the same in *nature* with those that attend the disease from its commencement, however different in degree ; it would have been no great stretch upon probability to have considered the disease altogether as the result of *inflammation in the sensorium*. Indeed, I do flatter myself, that when " the

struggle between the prejudices of education and the staring conviction of opposing facts," to which he elsewhere alludes, has subsided, not only will "practitioners," as he says, "be compelled to a gradual but material change in their views;" but that he himself will find little difficulty in adopting altogether a theory, to which it is evident, from what has been already stated, as well as from what follows, he very nearly approximates.

The admission, however, that the more violent forms of the disease were essentially connected with inflammation of the brain, would not go the length of proving that all fevers consist in this, nor even the present epidemic in all instances. The milder cases, amounting, as we have seen, to two-thirds of the whole, would still remain to be accounted for.

Now it is not possible, I apprehend, to draw an absolute line of distinction between the *milder* and the *more severe* cases of fever. The difference is merely in degree, and there is every possible gradation between them. The *slightest* affection of the sort is

only a lower degree of the *most violent* The theory of one is the explanation of the other. The *essential* or *pathognomonic* symptoms are of the same nature in both cases ; and consist, either in *uneasy feelings* of the brain, or in a *disturbed state of its functions* ; but commonly in both together. The *general* disturbance observed in the rest of the system consists either in the state of *pyrexia*, which is the common result of inflammation, wherever seated ; such as frequency of pulse, heat of skin, and foulness of the tongue ; or they are the consequence of that influence, which the brain exerts over every individual part of the body ; so that each in turn is liable to have its functions impaired or disordered, from a previous affection of this organ.

That the symptoms characterizing the present epidemic (as laid down in the work under examination, the accuracy of which no one will be disposed to question) are *essentially* the same, and differ in *degree* only, in the various forms of the disease, will be rendered evident by contrasting them with each other in a *tabular* form. Dr.

Bateman, as we have seen, marks *three* degrees or *varieties* in the character of the disease: 1st, the *mildest*, occurring principally in children or young subjects: 2dly, as it occurred in adults, when the symptoms were commonly *somewhat more severe*; and 3dly, the *more severe* or *complicated* form, as it is called.

The particular symptoms described as belonging to each will be seen in the following table. I have omitted the general febrile symptoms, as being common to all.

<p>First, or mildest State, (described at page 33.)</p> <p>Headache — universal pains — lassitude — sometimes, though seldom, a tendency to delirium, or slight confusion on waking — frequent deafness — mucous fur on the tongue.</p>	<p>Second, or somewhat more severe, (page 50.)</p> <p>Severe headache — pains in the limbs — sleeplessness, or disturbed dreams — in <i>general</i>, without <i>actual</i> delirium. In this form, after the headache has subsided, the head is still the seat of distressing sensations; as loud <i>tinnitus</i> or <i>vertigo</i>, or a sense of weight or load, or of increased bulk, the head seeming to the patient “as large as two” — frequent deafness — mucous fur on the tongue.</p>	<p>Third, or the <i>severe</i>, or <i>complicated</i> Form, (page 52.)</p> <p>During the first week, or later, the <i>same</i> symptoms as in the milder forms — then, <i>increased</i> languor and debility — evinced by feeble voice, slow speech, dull eyes, heavy countenance, and supine position — <i>more manifest</i> disturbance of the <i>sensorium</i> and its functions; as confusion or wandering of mind, dulness or inaptitude to receive external impressions — slow comprehension — stupor, or active delirium — frequent deafness — tongue dry and brown.</p> <p><i>Furious</i> delirium, <i>subsultus</i>, and <i>petechiæ</i> or <i>vibices</i>, occurred very rarely.</p>
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From this comparative statement of symptoms, it must appear, I think, to every unprejudiced mind, that there is no essential difference between them, as they occur in the different forms of the disease. The difference is merely in *degree*. There is no such change of symptoms noticed in any stage of the disease, as to indicate a *new* condition arising in the brain. If inflammation is allowed to take place in the more *severe* and *advanced* stages of the disease, there is no difficulty in admitting it to exist in the *earlier* and *milder* forms; for the disease, at all periods, must be radically and essentially the same. And it cannot be denied, that the symptoms observed in the first or *mildest* degree are such as would naturally arise out of an inflamed state of the brain; namely, headache, sleeplessness, disordered sensations, tendency to delirium or slight confusion of ideas, — all of them signs of deranged *sensorium*; and, together with these, general febrile symptoms, or *pyrexia*, the common attendant of inflammation, wherever seated.

Dr. Bateman observes, (p. 62.) that in a few cases, "unequivocal symptoms of *acute* inflammatory action in the brain appeared;" by which is meant, that the disease assumed a *phrenitic* form, agreeing with the definition of *phrenitis* usually given. This occurred in young and stout subjects; and it appeared, he says, in proportion to its acuteness, to be more under the influence of remedies; as all who were seen early, recovered.

I have already remarked, that inflammation of the brain, even in the opinion of the author, may take place without giving rise to the symptoms ordinarily ascribed to *phrenitis*. The same is further proved in the case of children, where inflammation of the brain often terminates quickly in fatal effusion, without ever being attended with *phrenitic* symptoms. I cannot admit, therefore, that *furious delirium*, or the other parts of the definition of *phrenitis* usually given, are at all better evidence of "*acute* inflammatory action in the brain," than the symptoms of *ordinary fever*, when violent.

The difference between *phrenitis* and *fever* is to be sought for, probably, rather in the particular *seat* which the inflammation occupies in the brain, than in the *degree* of it. At the outset of *phrenitis*, the functions of the brain are performed with *increased energy*, though at the same time in a highly disordered manner. The brain is rather *irritated* than *impeded* in its functions at first; as might be expected to happen, supposing the inflammation to affect principally the investing membranes. But, in violent *fever*, the *functions* of the brain suffer greatly from the first. They are not merely *disordered*, as in *phrenitis*; but very *imperfectly* carried on, and, at length, annihilated. The organ suffers more from *oppression* than from *irritation*, though, in some degree, from both.

It is evident from this, that the *efficient* part of the organ, the *medullary substance*, is the part principally suffering in fever. When *phrenitis* terminates fatally, provided the termination is not sudden, as by general convulsions, or apoplexy, the patient sinks gradually into a state of imperfect

coma, with the *muttering* and *subsultus tendinum* that characterize violent or *malignant* fever in its advanced stage. The explanation of which is, that the inflammation, which was at first chiefly membranous, dips at length into the substance of the brain, which then becomes no longer capable of carrying on its most important functions. In short, the last and fatal symptoms, and consequently the actual condition of the organ, are in both cases very nearly the same.

In most of the cases of this description which have come under my observation, the *phrenitic* symptoms appeared in rather an advanced stage of the disease; and when an attempt was made to relieve them by blood-letting, the furious delirium was quickly succeeded by stupor, and the patient soon died. When treated by *opium*, or other means, the event was not more favourable.

Dr. Bateman accounts for the *sleeplessness* and imperfect slumber that occur in fever, by referring them to the *disturbed state of circulation* in the *sensorium*. But is not, I would ask, the same cause sufficient to ex-

plain the *headache*, the *universal pains*, the *lassitude*, and the *tendency to delirium*, which are described as essentially characterizing the disease ; and is not a disturbed state of circulation the natural and necessary consequence of inflammation ? The long and profound sleep that so often takes place at the close both of fever and phrenitis, seems to indicate a previous excited state of the brain, and so far shows an analogy between these affections. Nothing of this kind is observed in other diseases. *

* I cannot resist the temptation of transcribing, in this place, what few, probably, will hesitate to admit as a striking and faithful delineation of one of the commonest forms of fever of the present day, whether arising from contagion or otherwise.

“ The patient goes about his usual occupations ; he feels a depression of spirits, and a confusion of head ; a want of appetite, and loathing of food ; he is in a faint, languid, and nervous condition ; his hands tremble, and his head swims upon being hurried in exercise, or disturbed with any unusual emotion ; and he passes the night in unquiet sleep and terrifying dreams, from which he wakes in indescribable confusion from time to time. In a few days, the fever is more conspicuous, with slight horrors and shiverings ; the confusion is more distressing, attended with pain, and a sense of girding in the head ; he can no longer endure the light ; the eyes are red,

An important remark, and a very just one, as it appears to me, is made respecting the indication afforded by the *tongue*. (P. 42.) Dr. B. conceives, that the *brown* and *blackish* appearance of this organ has no decided connection with the state of the *primæ viæ*; but is more distinctly indicative of the state of the *sensorial power*, than of the condition of the *primæ viæ*. The observation might have been carried farther. A furred tongue is never, in my opinion, the direct effect or sign of any peculiar state of stomach; but

swollen, and gummy (the *oculi sub-pingues* of the ancients); the pulse is quick and weak; the tongue foul; the skin parched; the visage pale and ghastly, with a hectic flush upon the cheek; the urine pale, and sparing in quantity; the hands, and the tongue, when he puts it out, tremble. He knows not what ails him, but is night and day in a state of indescribable confusion. During the night, roving, grinding the teeth, and slightly delirious; and during the day, desponding, oppressed, and sick."

Such is the animated picture drawn by the most descriptive of medical writers, Mr. John Bell, when detailing the approach of suppuration of the brain, in consequence of external injury.* What is the inference?

* Principles of Surgery. 4to. p. 470.

always denotes *pyrexia*, or febrile action in the system, and therefore the existence of inflammation in some part.

When Dr. B. observes, (p. 45.) that the *dark brown hue* of the tongue *seldom*, and the thick-coated and chappy state of it *never*, occur in the *simpler* forms of the disease, he evidently again confounds *simple* and *mild* together, as he had before done the terms *severe* and *complicated*. The most violent and *malignant* fever may be perfectly *simple* in its character, (by which can only be understood, not complicated with other affections,) and yet, beyond a doubt, be accompanied with the state of tongue mentioned. Such a state of tongue serves to indicate the *violence* of the disease, and not the *complicated* nature of it.

The appearance of *vibices*, with stupor and extreme prostration of strength, Dr. Bateman thinks, may justly be referred to “ a state of *venous congestion*, with *feeble* or *oppressed* action of the heart and arteries.” After having admitted, in a former part of his work, that where the brain was much disturbed, this organ “ must undoubtedly

be considered as the seat of at least much *over-action* and *over-distension*, if not of some degree of inflammation ;” and considering that vibices, and extreme prostration of strength and the like, never appear without such disturbance of the brain ; it might have been expected that he would have rested satisfied with this explanation, without adopting from others the unfounded notion of “ *venous congestion*, with feeble action of the heart and arteries.” Upon this subject, I shall beg leave to repeat my former remarks.

An unusual quantity of blood in the *veins* of any part during life, can only arise from increased arterial action, or from some cause impeding the return of blood to the heart ; the *arteries*, at the same time, continuing to urge onwards their contents. If, as is here supposed, the action of the heart and arteries were *feeble* and *oppressed*, the blood would be carried into the veins with less force, and therefore in less quantity. The reverse of a state of *congestion* would ensue. And with respect to obstruction to the return of venous blood to the heart, (by which, in-

deed, *congestion* might take place in the veins,) no such cause is alleged, or can be supposed to exist, in regard to other organs than the brain; and therefore the phenomena referred to such a source must have a different origin.

With respect to the brain, the case is somewhat different. An impediment to the return of blood by the veins may take place here; but it can only be from the pressure occasioned by the distended state of the arteries, the consequence of their increased action, as already explained.

Admitting the existence of *venous congestion* in the brain to the extent before mentioned, that is, as far as regards the superficial veins, it is still but an *effect*, a consequence merely of increased arterial action; it is not the disease, nor the cause of the symptoms referred to it. Accordingly, in all the known and undisputed instances of inflammation of the brain, where the arteries are excited to the utmost; as in *phrenitis*, and the acute hydrocephalus of infants; such a turgid state of veins is constantly found.

Interrupted circulation in the brain, then, is the great cause of the interruption and suspension of functions, which are observed in the worst stages of *malignant* fever. It is a state of *oppression*, the result of previous *excited arterial action*, of which there are the strongest proofs, especially in the early stage of the disease. The suspension of functions, and the general disorder throughout the system, will be in proportion to the extent to which the circulation is interrupted. Hence proceed the stupor, and insensibility, and loss of muscular power, from the suspension of the *immediate* or proper functions of the brain; hence, the stupid stare of the eyes, the countenance altogether expressing a total obliteration of mind; hence, the paralysed state of many muscles; the feeble action of the heart; the diminished tone, or want of cohesion, of the solids in general, evinced by the flabbiness of the flesh, and the pappy feel of the pulse; the want of contractility in blood-vessels and canals, in consequence of which their contents are allowed to escape, producing *ecchymoses*, passive hæmorrhages, and in-

voluntary discharges: hence, also, the *putrescent* disposition of the fluids — all evidences of deficient *vitality*, proceeding from the *oppressed* state of the brain, in consequence of which, every part of the body is robbed of its vital energy, as far as depends upon this source.

Such symptoms and appearances have been referred, by some, to the directly poisonous property of the contagious virus acting upon the principle of life; by others, to its operating as a *septic*, on the general mass of blood. But it is a sufficient answer to these opinions, that such symptoms are not the first or immediate effect of the application of the virus to the body, but appear often at a late period of the disease; that they are in most cases preceded by marks of violent vascular action, both in the general system, and, more especially, in the brain; that they occur most frequently in vigorous subjects, and in such as were previously in high health; and, lastly, that they have often occurred where no contagion was suspected, or could have existed; as where fever has been brought on by a

coup de soleil, or other causes quite distinct from contagion.

Dr. Bateman is disposed to refer the disordered state of certain organs, observed at different periods of the fever, to irregularity and disturbance in the balance of circulation. Such disturbance, he observes, is indicated "by signs of a greater determination of blood, or of a certain degree of inflammatory action;" if so, it was unnecessary to have recourse to the supposition of *venous congestion*, to which they are elsewhere referred.

With respect to the appearances found upon dissection after death, Dr. Bateman observes, that "many circumstances concurred to render such examinations inconvenient or impracticable, during the prevalence of the present epidemic; so that *only one* investigation of that sort has been instituted." His opinion of the *seat* or *nature* of the disease, therefore, must have been drawn from other sources; and could only be so, from the symptoms that presented themselves during life. How far these indicate the existence of inflammation in the

brain, I have endeavoured to show above, from his own admissions. The case of dissection alluded to, is, as far as a single case can go, decisive in proof of this. It was, he says, “ a well marked case of the disease, characterized by *great prostration of strength, a black mouth, muttering, and subsultus*. The *only* changes which were observed on inspecting the brain, were a slight *effusion under the arachnoid membrane*, and an apparent *increase of the number of bleeding points*, on making a transverse section of the cerebrum ; no other appearance, different from those observed after death, in many cases where no contagious fever had occurred, could be detected ; and all the other viscera exhibited a natural character.” P. 86.

In this passage, the author uses the term *only*, as if he thought the appearances observed were not sufficient evidence of the previous existence of inflammatory action in the brain. As however similar appearances, and not at all more striking, have been found in many cases of acute and admitted inflammation of the brain, (as we learn from *Morgagni* and other writers on

morbid anatomy,) such appearances, coupled with the symptoms of *brain-affection* always present in fever, together with the absence of marked disease in other organs, must be held, I think, sufficient proofs of both the *seat* and *nature* of the disease.

I may add, that in no one of the cases of the present epidemic which I have had the opportunity of examining, (and they have been not a few,) have the appearances above described been wanting; they have indeed been generally present, to a much greater extent than is here mentioned; namely, a more considerable degree of *effusion*; a higher state of vascularity; thickening and opacity of membranes; and, often, preternatural *adhesions*; with occasional florid redness of the *pia mater*, more especially towards the basis of the brain. Such appearances are surely sufficient proofs of pre-existing inflammation. We are not warranted in measuring the effects of disease and injury, in such an organ as the brain, by the same rude scale we employ in those of other parts.

Although the opportunities of the author for observing the state of the brain or other organs after death, during the present epidemic, have been so scanty; he has, he says, in the course of former years, “examined, from time to time, the state of the principal organs, *in cases like the preceding*; and the appearances have *always* been nearly similar,” though more strongly marked; for “there has generally been,” he adds, “a more decided effusion between the external tunics of the brain. In one instance, the serum amounted to about five ounces; and there has generally been some appearance of opacity, and also partial adhesions, of the same membranes; and more appearance of turgescence in their vessels. The substance of the brain also exhibited the same appearance of many bleeding points, and occasionally appeared to be rather firmer than natural, in one case even 40 hours after death, when the abdominal organs were far advanced in a state of putrefaction.” (Page 87.)—This last is a circumstance I had long ago noticed in fevers of the *putrid* kind. It is the natural

consequence of the induration which inflammation generally induces in parts, and by which the tendency to decomposition is lessened. It may not improbably be owing also to the greater degree of *vitality* imparted to them by the same cause; in consequence of which they are the longer enabled to resist the putrefactive tendency.

In some few instances, marks of disease were found in the organs of respiration, and in the abdomen; but it is to be observed, that such cases were always accompanied with evident signs of inflammation in those organs during life; so that the appearances mentioned might be predicted. They were in reality *complicated* cases of fever.

I shall only remark farther upon this head, of the appearances observed upon dissection, that it is inconceivable that Dr. Bateman should have made such admissions, as are to be found in the foregoing paragraphs; and yet should hesitate to connect inflammation *essentially* with the fever, as its cause.

It is a remarkable circumstance, which I have before noticed, that during the course

of fever inflammation often arises, and proceeds to even a destructive length, without producing any but the slightest symptoms; so that the mischief done by it has only been discovered after death. An example of this is here alluded to; “in which the fever went on to a considerable length, without any *material** disturbance of the *sensorial functions*; and where small ulcerations, with slightly elevated edges, penetrating to the peritonæal coat, were found occupying the internal surface of the small intestines; although no local symptom existed referrible to this part, but a *relaxed state of the bowels*.” P. 88.

From the similarity of circumstances, I imagine the case here alluded to, is one that occurred several years ago at the *Fever In-*

* The term *material* here employed, at least implies some disturbance of the *sensorial functions* to have existed. I take this opportunity of remarking, that the *proper* symptoms of fever are generally diminished, in some degree, by combination with other affections; many of which are more painful, and therefore excite more attention, than the fever itself. Hence the chance of overlooking the fever altogether when complicated; which I have often seen to be the case.

stitution, in Gray's Inn Lane; the dissection of which, through the kindness of the author, I attended.* The examination was conducted by Dr. Farre; and we were not a little surprised, I recollect, at discovering so much organic lesion, where nothing existed, during the course of the fever, to excite suspicion of the mischief that was taking place. There had been, however, a considerable degree of *diarrhæa*, or *relaxation of the bowels*, as it is often improperly called, which is generally the result of inflammation of the mucous membrane of the small intestines. That it should have proceeded to such a destructive length, with such trivial symptoms, can only be accounted for, as it appears to me, from the insensibility induced by the fever, or *brain-affection*.

I have no doubt that many cases prove fatal from the occurrence of such *secondary*

* I gladly embrace this opportunity of bearing my grateful testimony to the liberality of Dr. Bateman, in affording the utmost facility to those who expressed any desire of witnessing the practice at the *Fever Institution*, the records of which were at all times open to their inspection.

affections in an advanced stage of fever, where the fever itself is so mild as to excite no apprehension of danger, or even where it has already begun to decline. In such cases, it is natural to expect, that the marks of disease in the brain after death will be trifling; while the observation of morbid appearances in other parts may easily mislead our judgment, in regard to the pathology of the disease.

The subject of the next section is the *Method of Treatment*.

Through the liberality of Dr. Bateman, I have had an opportunity of observing the method of treatment pursued at the *Fever Institution*, whilst under his care, and can bear testimony to its general success. I was, indeed, much struck with the simplicity of the practice, as compared with the ordinary treatment of the disease; and have no doubt, that, generally speaking, it was the best that could have been employed, under the circumstances present. There was no mystery, no affected complication of prescription; and the object in

view appeared to be as simple as the means that were employed to attain it.

It is to be recollected, however, that the fever was in general far advanced in its course before its reception into the hospital; the time of admission, upon an average of all the cases, being as late as the eleventh day, hardly any having been received earlier than the fifth or sixth days of the disease. At such periods, there are but few instances in which an active mode of cure can be undertaken with advantage, or even with safety. The treatment *then* is little more than palliative, and consists in conducting the disease as quietly as possible through its course.

Dr. Bateman appears to be fully aware of the great importance of an active mode of treatment at the outset of the disease; for he says, "that whatever differences of opinion may exist, as to the presence or absence of inflammation at the *commencement* of fever, experience has fully decided, that the occurrence of more or less of *inflammatory excitement*, general and local, in the course of its subsequent progress, is

the chief object of apprehension, and the great source of danger ; and that *the only practicable means of anticipating this event, or of speedily abridging the term of the disease, are such as enable us to subdue other inflammatory disorders.*”—This is language which would not have been tolerated, nor probably used by Dr. Bateman himself, a very few years ago. Such is the change of sentiment that has lately taken place on this point! The prejudice against blood-letting, as a *real cure* for contagious fever, is rapidly subsiding ; and the following candid avowal of Dr. B. will tend materially to its annihilation :—“ I am fully conscious,” he observes, “ of the extent to which my own practice has been cramped by this prejudice, and of the reluctance with which I have admitted the evidence of my senses ; till frequent repetitions, and the sanction of other authorities, had rendered it irresistible.” P. 98.

The first measure proposed by the author, in all circumstances, is, the prompt evacuation of the stomach and bowels. This, in young persons, where the attack has been

very mild, he has seen reduce the fever to a slight *febricula* of five or six days. In two cases only, did he find it put an immediate stop to the disease. The effect of vomiting and purging combined, in interrupting the course of the fever, has been much greater in my practice than is here stated. But this is probably to be ascribed to my having generally seen the disease at an earlier period.

After the first or *chilly* stage has ceased, and when that of *excitement*, as it is termed, has come on, then more active *antiphlogistics*, such as *blood-letting* and the *cold affusion*, may still, it is said, cut short the fever, if resorted to on the third, fourth, fifth, or even sixth day. Dr. B., however, acknowledges that his own experience with regard to blood-letting has been very limited; but as far as it goes, it appears to be decidedly in favour of the practice. "Few opportunities occur," he observes, "among the patients of the *Fever Hospital*, of employing this remedy before the sixth day: but several patients had been bled previous to their admission, in all of whom

the headache had been removed, or greatly relieved, by the venesection; and though the fever could not be said to be cut short at once, it was always abridged in its duration, and exhibited no unfavourable symptoms during its course."

The circumstances under which Dr. Bateman thinks blood-letting is called for are worthy of notice.—"It seems to me," he says, "that *if*, on the third or fourth day, the headache is acute; or *if*, without severe headache, there is much watchfulness, a hurry of thought, and rapidity of speech, and an unusual sensibility to light, especially if the pulse be 110 or 120; such symptoms mark a condition of the sensorium, *bordering on inflammatory action*; and blood-letting is the most effectual mode of anticipating the morbid changes which are likely to follow, and which sometimes come on so suddenly, as to inflict an irremediable injury on that delicate organ, the brain."—"This," he adds, "often occurs even by the ninth or tenth day, or sooner; and it too often happens, *that no alarm is*

taken, till this *unconquerable* evil is already produced." P. 100.

I have quoted the passages above, as affording room for most important observations. They serve, in my opinion, to show how lame and defective mere *experience* is, without the aid of general principles, or *theory*, as it is called, to direct and limit it.

Dr. Bateman, like many others of the present day who have recommended blood-letting, employs it to relieve *symptoms* merely, and not to *cure* the disease; to abate the headache, the watchfulness, the too great sensibility to light, and the too rapid pulse; but not to remove the condition of the brain, out of which such symptoms spring: consequently, when the object of relieving such symptoms is attained, the practice is discontinued. But I would observe, in the first place, that if we limit the practice to the relief of certain symptoms only, the still-existing disease will probably advance again, as I have often experienced, and perhaps assume a different and more unfavourable character. And, in

the next, that such symptoms as are here supposed to call for blood-letting, often do not make their appearance till a late period of the disease, when blood-letting becomes at least an equivocal remedy. There are likewise many cases of fever, in which acute headache, great watchfulness, excessive sensibility, and great frequency of pulse, never occur at all; and yet the disease is attended with much danger. Hence, by considering such signs as the proper indications for blood-letting, and waiting for their appearance, this evacuation is likely to be deferred to too late a period, or omitted altogether, in cases where it may be imperiously called for.

Whereas, when we are guided by the general principle, that *the disease consists essentially in inflammation of the brain*, we conduct our practice in a very different manner; and, I venture to say from experience, with a degree of success far beyond that which attends the merely *palliative* mode of cure. We then have recourse to blood-letting, the moment the nature of the disease is ascertained, without wait-

ing for this or that particular symptom ; coupling it with other means adapted to the same end, that of subduing inflammation ; and this will be kept in view throughout the disease, or till the object is accomplished. We shall not be deterred from bleeding, merely because the pulse is weak or small ; but shall be led to enquire into the *stage* of the disease, and from this, chiefly, determine, whether the apparent debility is the effect of *depression* only, or is the result of *exhaustion* from protracted disease ; and use the remedy accordingly ; knowing, that if the former is the case, the powers of the system will rise, as blood is taken away.

We shall likewise be induced, in many cases, to bleed in the beginning, especially in vigorous subjects, although the symptoms should be mild at first ; because when inflammation is once excited, we are never sure of the extent to which it will run, and because we have no *absolute* power over it ; knowing, at the same time, that our chance of success depends greatly upon the early application of our means, while the utmost danger may result from delay.

With the principle, for which I am contending, in view, we shall be disposed to *invert* the common mode of proceeding; and, as in other inflammations, have recourse to the most powerful means at first; and not, as is generally done, employ trifling medicines in the beginning, reserving the most efficient till the disease assumes an alarming aspect; for then it is often too late. By acting in this way, we shall, in most cases, insure the safety of the patient, though now and then, perhaps, blood may be drawn in cases not absolutely requiring it. But as such cases cannot be distinguished at the outset, we adopt, in preference, the safer mode: for precisely the same reason, that we recur to inoculation in small-pox; that is, in order to guard against the possible contingency of a severe disease, and not from any prior knowledge we have of its necessity in the particular case.

It is impossible to adduce a more forcible argument in favour of this practice, than Dr. Bateman has here brought forward; namely, as “the most effectual mode of anticipating the morbid changes which are likely to follow, and which sometimes come

on so suddenly, as to inflict an irremediable injury on that delicate organ, the brain."

To produce such fatal changes in the brain as are here alluded to, would require, I should imagine, not merely a state *bordering on inflammation*, as Dr. B. expresses it, but *actual* inflammation itself, and that in a high degree. Now, if such morbid and fatal changes, the result of inflammation, can take place "*before any alarm is taken*," that is, before any considerable inflammation is suspected, there can be little difficulty in admitting the existence of inflammation in the brain, even in the early stages of the disease; although no symptoms of *phrenitis* should be actually present to denote it. Nor need there be much more, in supposing inflammation, in this delicate organ, to be the essential part of the disease, the real cause of the phenomena, to which we give the name of fever; especially where so many signs exist of such a state, to wit, pain in the head, heat, and throbbing; together with a constant disturbance in the *sensorial functions*, and precisely that state of system

(*pyrexia*), which inflammation so commonly induces. In short, we find the author constantly dreading the affection of the brain, as the great source of danger in fever, yet hesitating, in spite of evidence that to me appears irresistible, to admit that this is the organ primarily and essentially suffering.

Among the more powerful *antiphlogistics*, tending, like blood-letting, to cut short the fever, the *cold affusion* is mentioned, but in no terms of commendation; it not having appeared, in the author's practice, to abridge the course of the disease; and he gives a preference to the sponge. (P. 95.) The sudden affusion of cold water on the body is, however, a quite different remedy in its effects, from a more gradual application of cold by sponging or ablution: the one, by the sudden shock which it occasions, tends to interrupt the course of the disease altogether; the other, serves only to carry off the excess of heat from the body; thereby moderating, indeed, the diseased actions going on, but having no power to break their continuity. The one, therefore, is by no means a substitute for the other. The advantage of the *cold affusion*, I may remark,

is very differently estimated by other observers. A physician in extensive military practice in Ireland assures me, that he has lately employed it with great success. But in the advanced stage of fever, as it generally appears in the *Fever Institution*, I do not believe it would often be of any avail, or even generally safe. The carrying off the heat of the skin, when excessive, by sponging or other sufficient means, is at all times proper.

“ If the early symptoms should not indicate the propriety of resorting to this evacuation (blood-letting), still,” Dr. B. observes, “ the same principle must be followed in a minor degree. No appearance of languor or debility should induce a disposition to swerve from a steady pursuit of the antiphlogistic plan, in diet, regimen, and medicine.” (P. 102.) Of the propriety of this recommendation, I, for one, cannot doubt. Yet it is difficult to see upon what other principle, than that of inflammation, such suggestion can be founded. The author, indeed, assigns a reason for this mode of proceeding. “ The frame and physical

energies of the patient," he says, "cannot as yet have been impaired, or even partially exhausted; they have merely sustained a sudden depression, from which they speedily recoil, if the oppressive load of the disease be quickly removed." (P. 102.) Such language, I confess, is too figurative for my comprehension, — to say nothing of the incongruity of the images employed by way of illustration. I can readily understand how a general depression of the powers of the system should be the result of an oppressed state of brain, induced by overaction of its vessels; and how blood-letting, and other analogous means, by diminishing the vascular action of the brain, should set the organ, as it were, at liberty; and thus restore energy to the whole system. But I can form to myself no idea of such beneficial effects following blood-letting, upon the supposition that the symptoms above alluded to are the result of either *venous congestion, with diminished arterial action*, — or of *collapse* of the nervous energy, — or of the *sedative* operation of the contagious virus upon the nervous system, as it is expressed.

Dr. B. says, (p. 83.) "it is *indisputable* that, after the termination of the first week of fever, no means which our art affords are adequate to arrest its progress, or very materially to shorten its duration." This is rather too general, though it no doubt accords with the author's experience. I know, from the observation of many cases, that the disease may sometimes be arrested in its progress, or apparently shortened in duration, by the means above described, at a period later than the one here mentioned. Still, I admit the practice of blood-letting under such circumstances to be hazardous, and by no means fit for general adoption. If it be at all advisable at so late a period, I think it is where the disease is proceeding in a quiet, uniform manner, without any considerable derangement of the sensorial functions, such as delirium, stupor, or *subsultus tendinum*; for till these appear, we have little reason to apprehend either effusion, or any material change in the organization of the brain. But when such symptoms have already occurred, there is often (to use the words of the author), "so

much organic *lesion* produced in the delicate texture of the common *sensorium*, as to baffle the best directed operation of physical agents."

I agree entirely with Dr. Bateman, that in the early and middle periods of the disease, *stimulants* and *tonics* should be religiously avoided ; but not, I think, precisely for the reason he states ; namely, " that as they augment the *morbid excitement*, which is, in truth, the *depressing cause*, they are ill calculated to impart strength." (P. 103.) If by the term "*morbid excitement*" the author means the *pyrexia*, or increased vascular action that is going on throughout the system, I contend that this is not the cause of the depression observable in the state of the voluntary power, and other sensorial functions ; for we often observe a much higher degree of general excitement of the vascular system, without any such depression ; as in acute rheumatism, pleurisy, and many other active inflammations, where the brain itself is not in a morbid state. On the other hand, if he applies the term to the brain, and considers the vascular

action of this organ to be greatly in excess, then he grants almost all I ask; for such a state cannot be far removed from actual inflammation.

Speaking of the use of *antimony* in the early stage of the disease, Dr. Bateman questions its diaphoretic property, unless when it excites *nausea*; "a state," he says, "certainly not to be desired in this disease." There are, however, no means by which I have so frequently put a stop to fever at its commencement as *emetics*, repeated at short intervals for perhaps two or three days, where the disease did not sooner yield. But a distinction is intended to be made between *nausea* and *actual vomiting*; and undoubtedly the effects are considerably different. I speak, however, of both combined; for I have not attempted to separate them in practice; nor is it easy to do so.

In the second week of the disease, when *bad symptoms*, as they are called, begin to make their appearance; that is, when either active delirium, with rapid or continued talking, or attempts to get out of bed; or a more quiet confusion and slowness ap-

proaching to stupor, with moaning, &c., take place ; Dr. B. recommends the taking away a small quantity of blood, whatever the state of the pulse may be. (P. 112.) This is agreeable to the practice of Dr. Mills, who has adduced numerous and striking instances of its advantage.* For myself, I have seldom employed blood-letting at so late a period of fever ; partly from having entertained doubts with regard to it, but more from having generally used it earlier in the disease ; by which such symptoms have been anticipated and prevented.

The last stage of the fever is described with great force of language ; and in his observations on this part of the subject, the author appears to me to approach very nearly to the doctrine for which I have all along contended. “ Although I cannot,” he says, “ altogether assent to the doctrine of a recent writer, Dr. Mills, who maintains that the symptoms just mentioned are the result of an inflammatory action of the

* Op. cit.

brain and its membranes, which is to be cured by repeated general, though small, blood-lettings, as in the earlier stages; still I am persuaded that his view is an approximation to the truth, the boundaries of which he may have slightly overstepped." P. 118.

It does not distinctly appear, whether the author's admission that Dr. Mills's opinions are an *approximation to the truth* was intended to apply to the supposed nature of the disease as consisting in *inflammation*; or to the utility of *blood-letting* in the cure. But when he denies (p. 119.) that *subsultus tendinum*, picking of the bed-clothes, and a tremulous tongue, are essentially the concomitants of debility, or by any means indications for the use of stimulants: when, on the other hand, he asserts that they are distinctly symptomatic of *cerebral irritation*; (though, at the same time, he questions whether the irritation be of a *sufficiently inflammatory character* to require blood-letting;) and further, when, in the succeeding paragraph, he notices "the *sub-acute character of the inflammation*,

as indicated by the feeble action of the heart and arteries," it is impossible, I think, not to observe a very near approximation to the theory which I have endeavoured to support, and which Dr. Mills has in part adopted. *

It seems impossible indeed to reconcile the treatment which Dr. Mills employed, namely, small *general* bleedings; or that which Dr. Bateman would substitute in place of them, viz. *local* bleeding, by means of leeches, and *blistering*; with any other supposition. It may be said in reply, that the admission of inflammation in the brain being the cause of such symptoms is no proof that such inflammation was essential to fever, or any thing more than an accidental occurrence. But such symptoms, it is to be observed, are the almost never-failing attendants of fever, when the disease is proceeding to a dangerous length. If, therefore, inflammation in the brain is

* It is proper to observe, that Dr. Mills admits the doctrine that fever is always dependent upon inflammation, as its cause; but he does not think the brain to be always, or necessarily, the seat of it.

the cause of them, it appears most reasonable to suppose, that such inflammation has some essential connection with the disease throughout, instead of being merely a casual or accidental occurrence.

With regard to the treatment of the last stage of fever, when attended with the symptoms mentioned, my own observation would lead me to concur with Dr. Bateman in condemning the *stimulant* and *tonic* plan of cure, if carried to any great extent. I must avow, however, that I have not seen so much of this practice, as to warrant my speaking of it in very decided terms. I have been accustomed, on the authority of the late Dr. G. Fordyce, to give small and regulated quantities of opium, wine, and other stimulants, in the very advanced stages of fever, when the pulse is much reduced in strength and fulness; when the muscles are tremulous, and the mind in an active but disordered state; in a word, when *irritation* rather than *torpor* prevails. In such circumstances, these means have often been attended with much advantage. At the same time, I am not prepared to

call in question the numerous statements that have been heretofore made, from various respectable quarters, in favour of a more liberal allowance of such remedies; under the use of which, undoubtedly, recoveries have often taken place.

Dr. Bateman endeavours to reconcile the contradictory testimony adduced in favour of such opposite modes of treatment as have been pursued at different periods, by supposing that the character of the fever, which our predecessors had to treat, and the condition of their patients, (although both, in a great measure, influenced by, if not resulting from, their own proceedings,) might actually require a considerable difference of medical treatment. But it is difficult to conceive that a mode of practice which, when employed early in the disease, is found to create such *malignant* symptoms, should be a fit remedy for them at any period. Experience alone, however, can decide the point.

Some again are of opinion, that the present epidemic is of a more inflammatory character than some preceding ones; and

therefore that, admitting the evacuant plan of cure to be as beneficial as stated, in the present epidemic, it is no proof that the same treatment is adapted to contagious fever in general, as it appears in different seasons. I have even recently heard it asserted in a public assembly, by a hospital physician and teacher of considerable celebrity, that a case of real *typhus* has not occurred in London for 20 years past; and that the fever now prevailing is not at all the same with that which he had witnessed in his practice, 25 or 30 years ago.

It is difficult to reply satisfactorily to this mode of argument, because we have not the evidence sufficiently before us, to enable us to decide the point. It is certain, that the character of epidemic diseases varies in different seasons, and on their different returns; whether from a difference in the nature of the unknown cause inducing them, or from changes taking place in the condition of the human body itself, we have no means of determining. The fact, however, was noted by Sydenham, during several succeeding years; and he found the

difference to affect not only the character, but likewise the treatment of the disease ; for it was a chance, he says, but that he lost at first one or more of his patients, till he had ascertained the particular genius of the fever in each year. If this be true, it shows the uselessness of minute and laboured descriptions of diseases of this kind, which never present exactly the same appearance. Our judgment, in regard to *practice*, can only be formed upon general principles ; hence *theory* is indispensable ; and we have only to take care that it be founded on just physiological grounds.

It may be remarked, that, in judging of the effects of remedies in the treatment of fever, sufficient allowance has not in general been made for the stage of the disease in which they have been administered. The same means that are useful at one period, may be injurious at another. As long as the doctrines of *debility* and *putrescency* prevailed, the first stage of the disease was either improperly treated, or allowed to pass away, with little or no attempt to cure ; all evacuations of an active kind being prohi-

bited, from an apprehension of the coming symptoms of debility, or, as it would now be termed, the *typhoid* state, which many practitioners seem always to have in prospect before their eyes. It is not by studying the writings of this period, that the utility of evacuations can be judged of; since it is in the early stages of the disease almost exclusively, that they are admissible.

As to the question, whether the present epidemic merits the name of *typhus* or not, it will turn out, I believe, to be little more than a dispute about words. If such symptoms as are described above, as appearing in the last stage of fever, and which arise out of an *oppressed* and *irritated* state of brain, the consequence of increased and inflammatory action in its vessels, (namely, blackness and fœtor of the mouth, constant muttering and delirium, *petechiæ*, *vibices*, tremors of the muscles, and the utmost prostration of strength,) are entitled to this denomination, then I say that I have repeatedly witnessed it during the course of the prevailing epidemic; and, doubtless, should much more frequently have done so,

had a mode of treatment, of the nature of that which was practised 25 or 30 years ago, been generally pursued. *Typhoid* or *malignant* symptoms were then, as they commonly are at present, *created* by the practitioner himself; either by neglect of the first stage of the disease, and the avoidance of active evacuations; or by the premature and injudicious use of *stimulant* and *tonic* remedies. In some individuals, the tendency to the disease, from the first, appears to be so strong, that no means, however judiciously applied, are capable of checking its fatal course. But this is equally the case with other diseases.

CASES.

THE chief purpose of the following cases, is to show the power of blood-letting, on many occasions, and under certain circumstances, to interrupt altogether the course of fever, (whether proceeding from contagion or a different source,) and to mitigate the future progress of the disease, where, from the tardiness of its administration, or other causes, it does not absolutely cure. With this view, I have selected from a great number, a few of those which appear the least equivocal, and which rest upon the most indisputable grounds. If it be objected, that a partial selection of cases can contribute but little towards establishing the general character of any remedy, let it be recollected that my principal aim has been to establish a just pathology of the disease, rather than to advocate the use

of particular means of cure. It is the want of this that makes all our experience of so little advantage to us, either in regard to theory or practice. Practical illustrations are issuing almost daily from the press, yet it does not appear that we are advancing a single step, towards either a consistent theory, or uniform practice, in fever. As much of hesitation and uncertainty exists upon these points, as at any former period. The cases here adduced serve to show, that by treating fever, in its different stages, like ordinary inflammation, the same result is obtained (allowance being made for the peculiar nature of the organ affected). Hence a most important inference may be drawn, with respect to the nature of fever; in addition to a host of other arguments, derived from the character and course of the disease, as well as from other sources that have been particularised. It is to be observed, that in blood-letting, as a remedy for fever, there is nothing equivocal, as is the case with many of our other means of cure. Upon the supposition that fever is inflammation of the brain, we readily understand the effects of this remedy, and are prepared to expect from it the most signal advantages. While, upon other views of the nature of fever, the use of blood-letting, however beneficial, must often appear unintelligible, in a disease

that exhibits so many marks of debility in the general system. These cases serve further to show, that blood-letting is not confined to the relief of particular symptoms, which are only of occasional occurrence ; but that it is absolutely and decisively curative, from the very outset of the disease. While I urge thus much in favour of blood-letting, as a remedy for fever, I do not profess to be by any means sufficiently acquainted with all the circumstances that influence and limit the use of so powerful an agent, and I am quite sure that they are not to be found in any treatise hitherto published on the subject. The enquiry, as it appears to me, must be instituted afresh, and pursued with the utmost caution ; without which, infinite mischief may result. In doing this, physiology must be our guide ; to the ignorance and neglect of which, the innumerable wild hypotheses that have been engendered on this subject, are to be wholly ascribed.

In the year 1805, low fever, or *Typhus* as it is called, was very prevalent in the metropolis, and furnished me with many opportunities of witnessing the power of blood-letting over the disease, in cases where it unequivocally originated in contagion. The following series of

cases, occurring in the same family, are in proof of this : they are from notes taken by me at the time.

CASES I. to VII.

March 12th, 1805. Jane Butler, *æt.* 9, having been slightly unwell for several days, was taken particularly ill on rising in the morning of the 10th instant. She complained of violent pains in the head, back, and limbs ; she likewise vomited. The eyes became suffused, and intolerant of light ; the pupils were rather contracted. Noise was likewise distressing to her. Skin hot and dry, pulse 120, full and of tolerable strength.

On the 15th she felt better, and walked out, but was much worse afterwards, and became delirious in the night.

A leech was applied on the first day. Occasional purgatives, with antimonials, constituted the rest of the treatment. The fever did not subside till after the 14th day.

March 19th. S. B. *æt.* 5, sister to the above, had similar symptoms. In this case, the usual simple treatment was adopted, and the disease began to decline about the 11th day, but did not altogether cease till the 14th. *

* I have witnessed so many terminations of fever on the 14th day, and likewise on the 7th, 11th, and 21st, as to

March 26th. J. B., a stout active boy, *æt.* 7, was observed to be very poorly on the 23d, but was pretty well again on the following day. On the next, the 25th, he appeared very ill — he vomited much — the headache was very severe — and the pulse between 120 and 130, together with all the other symptoms of fever.

The vigorous habit of this patient, was an inducement to take from him 5 oz. of blood. He grew faint, and vomited while the blood was flowing, but his head was immediately relieved. He afterwards sweated profusely, and fell into a quiet sleep, which continued some hours. The fever continued the next day, but in a less degree; on the following, it was still less, and in two days after he was free from complaint.

March 28th. The mother of the children above was taken ill yesterday, and to-day has all the symptoms of *Typhus*, as it is called. The pulse is frequent, but not strong. She is at the full term of her pregnancy, and when taken ill yesterday, had had labour-pains for 24 hours

convince me, that a tendency to observe critical days exists in regard to fevers in this climate, as well as in the warmer countries of Greece and Italy. This, I have no doubt, would be more frequently observed, were not the natural tendencies of the disease so often interrupted, as they are, by the powerful agency of the remedies in modern use.

previously, and a midwife in attendance on her. On the attack of the fever, the pains left her, and have not since returned.

On the next day, the 29th, the symptoms of fever were more strongly marked, and she was unable to sit up. She complained of great pain over the forehead. The pulse was only 60, moderately full and strong. There was no stupor. No labour-pains had recurred. She was now bled to 8 oz. with the effect of immediately relieving the headache; and on the following day the pulse had risen to 70.

The symptoms continued in a reduced degree till the 8th day from the attack, when they ceased, and left the pulse at 75. Labour did not come on again till after a fortnight, when it terminated favourably.

March 31st. The husband of the above had for several days the usual symptoms of fever, but so mildly as to admit of his continuing his occupation. He was bled to the amount of eight ounces. The blood was covered with a gelatinous crust, and had little firmness: the serum was intensely coloured. The symptoms gradually declined, but did not leave him altogether for several days.

Two other children, younger and rickety, were also affected. The symptoms were milder

in these, but declined more slowly without the use of any active remedies.

These cases, as well as the following one, were all seen throughout by my very intelligent friend, Mr. Sawrey; to whose zeal and anatomical skill I am very particularly indebted, for numerous opportunities of investigating, by dissection, the effects of fever.

CASE VIII.

April 20th. M. G. *æt.* 30, who had attended as nurse on the family of Butler, throughout their illness, after complaining for some days preceding of slight headache and languor, was seized suddenly and violently about 9 in the evening of the 18th instant with cold shivering, followed by intense headache, and universal pains, and was scarcely able to walk about from weakness. She described the pain in the head as situated above the eyes, and darting through the brain. The pulse, 96, and weak, so as not at all to indicate bleeding, according to the common mode of judging. The good effects of it in the other cases were, however, a sufficient encouragement, and blood was drawn to 10 ounces, viz. on the third day of the disease. The blood had no buff on its surface.

The relief she experienced was so considerable, that it was expected the fever would terminate altogether in two or three days. Two days afterwards, however, the pain of the head returned in the night, with as great violence as before, and all the symptoms became aggravated. The pulse rose to 110. She was again bled to eight ounces, and the blood had a thick buff on it. The relief was as great as before. The pulse fell to 92, and in two days more she was convalescent. She complained for several days, however, of occasional pains shooting through the head, especially on coughing; and her sleep was disturbed with frightful dreams. In this case it is probable that a more active treatment would have proved still more successful.

CASE IX.

July 2. 1805. A man-servant in the family of Lady Harriett Gill, of Weymouth Street, had fever, which continued for nearly four weeks; and from which he recovered with great difficulty, after suffering the usual bad symptoms, viz. low muttering delirium, *subsultus tendinum*, dark brown crust on the tongue, with the utmost prostration of strength. He was allowed to take wine about the twelfth day, which was followed by aggravation of symptoms, from which he was

relieved, in some degree, by the repeated application of leeches to the temples.

CASE X.

July 19th, 1805. A female-servant in the same family, a robust young woman, was affected in a similar way, and with considerable severity, about a fortnight after the other. She was bled to eight ounces, at the end of the first week, with considerable relief to the headache. The pulse was fuller and stronger after than before bleeding; and she felt considerably stronger. Among other observations made at the time, was the following: "From the state of the pulse to-day, compared with what it was yesterday, it would seem as if bleeding were capable of converting the *low nervous fever* into one of the *inflammatory* kind;"—a paradox now easily solved.

The disease went on, fluctuating in violence, and attended subsequently by diarrhœa, for the suppression of which, opium and astringents were administered. There was after this an immediate aggravation of symptoms; and to this injudicious interference, as I now consider it, I cannot but ascribe, in great measure, the fatal termination of the disease, which took place on the 31st, about the eighteenth day of the fever.

CASE XI.

July 21. 1805. The coachman in the family, who had communicated frequently with the others, was attacked with the same general symptoms of fever, attended with sore throat, to which he was very liable. He complained much of the head and back. He was bled on the second day to 10 ounces, and on the following, the pain in the head had left him, though he turned giddy on getting out of bed. He sweated much, the night after the bleeding. He continued only slightly indisposed for four or five days, when the symptoms recurred with their former violence. He was again bled to eight ounces. This was repeated, by way of precaution, after the interval of a day, although he was as decidedly relieved on the second as on the first bleeding. He said the headache became worse, immediately after the third bleeding; in all other respects, however, he appeared better after it, and in three or four days he was abroad again, without complaint. He was ill altogether about ten days.

These cases, while they prove the utility of blood-letting, show likewise, that it might probably have been carried much further with advantage.

CASES XII, XIII.

The two following are adduced in order to show how much, and how variously, the condition of the brain is changed by fever, as evinced by the state of its functions.

July 13. 1805. A boy, five years of age, was seized the day before yesterday with the symptoms of fever. The following night, the symptoms were violent, and he became very delirious. The next day, the delirium subsided, and he quickly recovered.

While the delirium lasted, he spoke with great fluency and rapidity, talking almost incessantly; whereas, when in health, he never could speak without great hesitation and stammering. The stammering returned as the fever left him.

Jan. 7. 1815. Mr. D., a surgeon, *æt.* 40, had laboured under very slight symptoms of hemiplegia for a month, when he was attacked with the usual symptoms of fever. He remarked afterwards, that while the fever lasted, his articulation in speaking was much more easy and distinct than before.

CASE XIV.

I quote the following to show how little agreement there is among practitioners, even of the present day, in regard to the treatment of fever; and that if a caution is necessary in regard to the employment of wine and other active stimulants, it is no less requisite at present, than at former periods; for I believe the practice described below has still many followers.

I was called into consultation, a few months back, with two other physicians of no small celebrity in this town, on the case of a gentleman at the most vigorous period of life, who was then in the 14th day of fever, the origin of which was attributed to cold. He was lying stretched out at full length on his back, muttering constantly to himself, and was with difficulty made to answer questions. The muscles of his arms were in a continual state of tremour, so as to make it difficult to count the pulse; his lips and teeth were covered with black and offensive sordes; and there were numerous *petechiæ* on the skin, especially of the legs.

On enquiring into the former treatment, I learned that, from the commencement of the fever, he had been enjoined to drink Madeira wine *ad libitum*, of which he had actually drunk

from a pint and a half to a bottle daily. In the course of the disease, the symptoms above described gradually made their appearance, and were deemed a reason for more active stimulation; brandy, therefore, was added to the wine, and of which he had taken two wine-glassfuls during the day and night before I saw him. I need not add, that the medicines employed were in unison with this, — bark, ammonia, and camphor, as largely and as frequently as the patient could be induced, or rather forced, to swallow them. The event of the case will be readily anticipated; he died two days after. Many at present will suppose, that the *malignant* symptoms in this case were created by the means used to prevent and remove them.

CASE XV.

May 23. 1815. J. S. *æt.* 31, (a Dispensary patient,) a stout and healthy man, was attacked two days ago with the usual symptoms of fever, — cold shivering, sickness and vomiting, pain in the head, with sore throat. At present he complains of violent throbbing pain over the forehead, great prostration of strength, and total want of sleep. The pulse is at 100, soft and weak. The tongue is covered with a thick yel-

lowish crust. He attributes his disease to getting wet the day before he was taken ill.

He was vomited and purged actively by the elaterium, with some relief; but this being temporary, he was bled on the 24th, (the fourth day of the fever,) to eight ounces, and experienced almost immediate relief. The blood was much buffed, but not contracted. On the following day, he had not slept: his mind was active, grotesque figures constantly presenting themselves to his imagination; but of the absurdity of which he was fully conscious. The febrile symptoms were lessened, and his strength so much recruited, that he came down stairs, a thing he was quite incapable of the day before. The pulse had become fuller and stronger, and the crust on the tongue was altered from yellow to white. Repeated doses of ipecacuanha and rhubarb had been given (gr. v. of each), and had occasioned some sickness with frequent purging.

I judged it more safe to repeat the bleeding, which was done to eight ounces, from which he fainted. The blood again presented a buffy appearance. Sleep returned the following night; and from this time he was convalescent.

This case evinces how soon the powers of the system are reduced by fever, even in the strongest subjects. The action of the vascular system was

equally depressed with the voluntary power ; a circumstance that does not always take place, the general vascular action being often violent, where the muscular is much enfeebled. It shows likewise the utility of blood-letting and other evacuations, under a state of system which has generally been thought to prohibit them.

The following serves to show that a similar mode of treatment is not inapplicable, even in a comparatively feeble state of body.

CASE XVI.

Feb. 9. 1819. Robert Antis, *æt.* 13, a boy of delicate make, the son of a poor widow, to whose support he contributed by his labour, and who lived scantily, seldom eating animal food more than once in a week, became a patient of the General Dispensary on this day. He had been ill four days with the ordinary symptoms of fever, which came on suddenly in the evening, with cold shiverings. He had no pain but in the head, which was extremely hot, as usual. Slight delirium appeared the last night. The pulse was 112, very quick and small. The tongue was thickly covered with a drab-coloured fur.

It could hardly be doubted, from these appearances, that a severe and probably protracted

state of fever was about to follow in this case, unless the disease could be checked by active measures. And the great distress that I knew would be experienced in consequence, induced me to direct blood-letting to the extent of 10 oz., to be followed by brisk cathartics frequently repeated. No faintness succeeded. He slept good part of the night, sweated much, and expressed himself quite free from headache in the morning. The pulse had fallen to 75, and was rather unequal. He ate his breakfast with appetite. The tongue was much cleared of its fur.

Two days afterwards the symptoms had all recurred. The pulse was 110, the skin was hot, and he had again some pain in the head. Four oz. more of blood were drawn, which had slight traces of buff on its surface. Purging was again resorted to, and he took small doses of the digitalis. The good effects of this treatment were as remarkable as before. On the following day, he made no complaint; the pulse was 66, and very unequal. From this time, his recovery was uninterrupted. The cause of the disease has not been ascertained.

I was at first in doubt, whether the slowness and inequality of pulse might not be ascribed to the use of the digitalis; but as the inequality was observed in a slight degree after the first

bleeding, and before the digitalis had been begun; and as it went off entirely in two days after the second bleeding, although the digitalis was continued; they are rather, I think, to be attributed to the state of the brain induced by the disease and its subsidence. At the age of this boy, fever is very apt to be followed by some degree of effusion in the brain, marked by slowness and irregularity of pulse, and dilated pupils.

CASE XVII.

May 30th, 1815. I saw this day George Matthews, a robust young man, *æt.* 24, a patient of Mr. Murley, St. Paul's Church-yard. He was in the ninth day of fever; complained of extreme lassitude and headache, was slow in answering questions, and had laboured under diarrhœa for several days. The pulse was 90, soft, and of middling strength. The tongue was dry and thickened in its substance, so as to be put out with difficulty; and it was covered with a brownish yellow crust. He was so weak as to be unable to sit up in bed without support, and had been delirious before I saw him. He had been bled to 10 oz. on the sixth day of the disease, with temporary advantage.

I advised further blood-letting to 6 ^oz. only, but 8 were taken. Ipecacuanha and rhubarb

were administered at intervals to excite vomiting and purging. He was much better on the following day, but was bled again to the same amount, and the same medicines continued. The pulse fell to 75, and continued so, with little variation, throughout the disease; which however went on till the 17th day, but with very mild symptoms, no delirium appearing after the last bleeding. The treatment here, being applied at a late period, reduced the violence of the disease, and probably ensured the patient's safety, though it did not absolutely cure.

CASES XVIII. XIX.

Sept. 27. 1815. I attended, with Mr. Burrows of Bishopsgate-street Mr. J. K. of Gracechurch-street. He laboured under mild but well-characterised fever for 11 days. The first sign of his being unwell was, his awaking in the night affrighted; he got out of bed, and fancied he heard strange noises of persons breaking in, and it was a considerable time before his apprehensions could be allayed; and then his heart palpitated strongly.

As the fever was leaving him, severe pain in the side arose, apparently from inflammation in the liver. Blood was drawn to 12 oz. and the pain left him while the blood was flowing. The

blood was exceedingly buffed and cupped. By this, with active purging, the disease was quickly subdued.

Sept. 20th, 1817. I also attended with Mr. Burrows, a gentleman in Bishopsgate-street, whom we had occasion to bleed as late as at the end of three weeks, on account of pulmonic inflammation appearing at the end of fever. He had been bled repeatedly in the earlier part of the disease which terminated as favourably as could be wished.

These, with many other cases that have come under my observation, serve to show that blood-letting may be often necessary at the close of fever, on account of supervening inflammation, and that the debility induced by the fever, unless extreme, is no sufficient objection to the practice, if it be well accommodated, in point of quantity, to the actual state of the system.

CASES XX. XXI. XXII.

January 18th, 1818. I was desired to see Mr. W. A. T. a student in medicine. He had been ill of fever for seven days; and at this period he was sitting up; had not much headache, which however had been severe at the commencement of the disease; the skin was hot and dry; pulse 108, weak and small. He spoke

feebly and tremulously, and his hand trembled as he held it out for examination. He had scarcely slept from the beginning. He knew of no cause, unless that his brother, who resided with him, had had a slight attack of fever ten weeks before, from whom it might be suspected he had received infection.

Nothing active had been had recourse to, nor did it then appear justifiable, to either Dr. Bateman or myself, to employ other than palliative treatment. The fever went on till the 21st day when a favourable change, indicating recovery, took place. The pulse came down to 90, and the tongue became clean, appetite and sleep also began to return.

Two days after this, from over-exertion, as it seemed, in sitting up and writing, a recurrence of fever took place, attended with symptoms of pulmonic inflammation. These, however, were relieved by blisters applied to the chest: but the fever went on, and assumed subsequently the most malignant characters. He died on the 13th day from the time of relapse, and the 36th from the commencement of the disease.

I did not hesitate, from what I had before repeatedly witnessed, to predict that marks of inflammation would be found both in the brain and in the chest. The following is the report

of the dissection, as made by Mr. Sawrey, who examined the body.

“ *An Account of the Appearances observed in the Head, on Dissection, of Mr. W. A. T., 42 Hours after Death.*

“ Nothing remarkable in either scalp, cranium, or *D. M.* There was an effusion of a serous fluid between the *D. M.* and *tunica arachnoidea*. This tunic was semi-opaque, much thickened, and unusually strong; and the cellular tissue connecting it with the *P. M.* was distended with a fluid of a brownish colour. This fluid was particularly abundant over the middle and posterior lobes. The membrane was every here and there marked with white specks, about the size of a millet seed.

“ The veins of the *P. M.*, lying in the *sulci*, were turgid with dark-coloured blood; but the upper part of the convolutions was of a vermilion colour, and resembled in appearance a moderately-inflamed *tunica conjunctiva*. These diseased vascular appearances were more manifest on the middle and posterior lobes; and the under surfaces of those lobes were particularly marked by turgid dark-coloured vessels.

“ The ventricles contained a small quantity of water, and the membrane lining them was

thick, strong, and quite opaque in portions, especially where it was stretched between the *corpus striatum* and *thalamus*. The *plexus choroides* were of an unusually florid colour, and the vessels of the lateral ventricles very conspicuous.

“ The *tunica arachnoidea* at the basis of the brain was much thickened, and partially opaque. The *P. M.* covering the *crura* of the brain, *corpora albicantia*, *pons varolii*, *crura cerebelli*, *medulla oblongata*, and *spinalis*, was as if finely injected with a vermilion-coloured injection. There was water in the fourth ventricle, and in the *theca vertebralis*.”

Besides the above, there was adhesion, with other marks of inflammation, on the side of the chest, where the pain had been chiefly seated.

Feb. 8. Mr. H. T. and Mr. G. T., brothers of the above, who had attended on him most assiduously throughout the disease, both sickened about the same time, and with the ordinary symptoms of fever. The former, who had gone through fever in a favourable manner, about twelve weeks before, as above stated, had the disease so slightly as not to be obliged to keep his bed. It lingered on for about ten days, and then subsided, under the use of purgative and saline medicines only. It is not improbable,

that the disposition to the disease may have been lessened by the attack he had so recently suffered.

Mr. G. T., the third brother, had a violent disease, which, after running out to the 23d day, proved fatal. The symptoms, during the first week or more, were so mild, as to excite no apprehension of danger in either myself or Dr. Bateman; and I believe it is matter of regret now to both of us, that we were so lulled into security, as to neglect the employment of active measures in the beginning, (*blood-letting* especially,) which both the age and habit of the patient would have fully justified.

In the advanced stage of the disease, when the most malignant symptoms presented themselves, the usual routine of wine and other stimulants, with strong liquid food, was employed with great freedom and assiduity. But the only advantage, if any, that I could perceive from this practice was, the substituting a degree of stupid quietude for the restlessness and agitation before present.

CASES XXIII. XXIV. XXV.

The three following appeared to take their origin from the cases last described.

March 10. 1818. Mr. J. Walne, a medical student, was seized with the usual symptoms of fever on Sunday the 3d instant. When I saw him on the following day, he complained of severe, throbbing headache, with slight soreness of the throat. The tongue was thickly covered with a light-brown fur. It appeared doubtful to what cause the disease was to be referred. He had been wetted, by rain, the day before he was taken ill, so that the disease might be referred to that source. On the other hand, his brother, with whom he lived in intimate communication, had been daily, for three weeks or more, in close attendance on Mr. W. T. and his brother, whose fatal cases are described above. Hence it is possible the disease may have been derived from contagion, though his brother was not himself as yet affected.

On the day after the attack, he was bled to the amount of 22 ounces by weight. On the third to 14 ounces, on the fourth to 12 ounces, and on the sixth to eight ounces, — in all, 56 ounces. There was much *size* of a reddish tinge on the blood of the three first drawings, but the crassamentum was flat on its surface. The headache was relieved after each blood-letting, and scarcely any symptom of fever remained after the sixth day, except a furred state of the

tongue, which did not disappear till after the eleventh day. Calomel and the digitalis were employed, but neither of these produced any sensible effect.

March 15. Mr. G. W., a surgeon, (now apothecary to the *Fever Institution*,) brother to the last-named patient, went to bed last night feeling unusually well, although he had had slight catarrh for a week or more previously. He got up well this morning, and took his breakfast as usual. Soon after, while standing before the glass in dressing, he turned faint and pale, and felt dizzy. This was soon followed by headache, with throbbing of arteries, and a sensation of heat in the head. He went abroad, however, on his usual business, but had frequent chilly fits, which continued through the day. There can be little doubt that the source of this disease was contagion; for he had been in daily attendance upon his friends, W. T. and G. T., whose cases are related above, and which terminated fatally. (See Cases XX. and XXII.)

At four o'clock, (only six hours after the attack,) he was bled to the extent of 30 ounces. This was done, although his pulse at the time was so weak and small as would generally be considered as contra-indicating loss of blood. An active cathartic followed. His headache was immediately relieved, but he remained faint

for some hours. On the following day, the headache continuing, and the tongue having become thickly coated with a light-brown fur, and the pulse beating near 100 in a minute, it was evident the disease was not subdued, and 16 ounces more of blood were drawn. The headache having increased towards evening he was again bled to eight ounces, and by the slipping of the bandage during the night, five or six ounces more were lost.

He slept well during the night, the headache ceased, the pulse fell to 90, and was decidedly fuller and stronger than before the first bleeding. The tongue had become cleaner; the appetite had returned, even voraciously. From this time, viz. the third day, he was entirely convalescent. The quantity of blood drawn in the two days was 60 ounces.

March 16. A young woman, servant to the above, who had attended on Mr. J. W. for a fortnight during his illness, was attacked this day with very distressing headache. She shivered often, and her tongue was dry and furred. She had felt slightly unwell the day before.

She was immediately bled to 26 ounces by weight. The next day, the symptoms continuing, though mitigated in violence, she was again bled to 24 ounces; and on the following to 12

ounces. It is remarkable, that the blood on the third drawing only was sisy.

Every symptom disappeared, and from this time she was convalescent, having lost 62 ounces of blood.

CASE XXVI.

The next is the case of a student of medicine, who had frequently visited Mr. W. A. T. during his illness, (Case XX.) I cannot give the account better than in his own words.

“ The first indication I had of my late illness was on retiring to rest on the evening of Tuesday, March 10th, when I felt a slight pain and confusion in my head, with general languor and uneasiness ; but these were so very mild, that in all probability I should not have recollected them, but for the subsequent continuance and aggravation of my disorder. On the following morning, after passing a restless night, from my sleep being disturbed by disagreeable dreams, I awoke with considerable pain and giddiness, restlessness, and indisposition to fix my attention on any subject to such a degree, that I was obliged to make several efforts before I could resolve to count my pulse a sufficient length of time, to ascertain the frequency of its beat. I

had also pains in my limbs and back, — thirst, — furred tongue, and occasional flushings of heat over the whole surface of the body. At 9 o'clock, I took an ounce of sulphate of magnesia, which is generally sufficient to purge me briskly ; but this having no operation, I repeated the dose at 2, and in about an hour after, I had a very copious motion. Finding the symptoms, however, becoming gradually more severe, particularly the pain in my head and back, at 5 o'clock I had seventeen and a half ounces of blood taken from my arm, which produced deliquium, and soon after I had another plentiful evacuation from the bowels. I took large quantities of soda water and oranges to allay my thirst.

“ I passed the following night with tolerable comfort, and obtained several hours of sound sleep ; but about 10 o'clock on Thursday morning the pain in my head returned with increased violence : it was most severe in the forehead ; and the throbbing was so excessive as to give me a sensation at every pulsation of the arteries, as if my head had been raised from the pillow, and an effort made from behind to propel my eyes from their sockets. My skin was very hot and dry, particularly in the forehead and temples ; my tongue thickly coated with a white fur ; I had intense thirst. I took three grains of calomel, and

10 of jalap, and a similar dose three hours after. Towards afternoon, all the above-mentioned symptoms continuing to grow more violent, and my mind becoming so disordered, that I was apprehensive of approaching delirium; you were desired to visit me, and advised me immediately to lose more blood. When about 18 ounces had flowed, the gentleman who bled me was desirous of stopping it, but I requested him to allow it to continue to flow, as I already felt much relieved. Twenty-four ounces and a half were taken, when I became faintish, and soon after fell into a composed and tranquil sleep for about three hours. On awaking I was free from pain, and had no disagreeable sensation except a continuance of the thirst, and a nauseous taste in my mouth. The medicine operated twice, and the evacuations were of a greenish-brown colour, and extremely offensive odour.

“ Friday morning. I have had a good night, and to-day I experienced only a general lassitude and feverishness, with occasional paroxysms of pain and heat in my head. I took in the course of the day four and a half grains of calomel, 15 of jalap, and half an ounce of sulphate of magnesia, which purged me effectually, the fœces having the same characters as yesterday. I sat up about an hour in the evening without material inconvenience.

“ Saturday. I was much better, and took for breakfast a cup of coffee and a biscuit. This produced a considerable glow and excitement over my whole body, and I had a temporary return of pain and throbbing in my head. I sat up two or three hours in the evening.

“ Sunday. Convalescent ; no pain ; but still some thirst and feverishness. My tongue was coated and dry towards its base, and my pulse 80.

“ To-day, Monday, I feel quite well except a trifling degree of debility.

“ Thus, Sir, I have endeavoured to trace the history of my indisposition. That its seat was in the head, every sensation I experienced firmly convinces me ; for there was not a depraved feeling in the whole system which was not fairly attributable to the brain as its source. The lassitude and incapacity to mental exertion plainly indicate a disordered state of the seat of sensation and intellectual function ; and as a further proof, I would instance the morbid secretions into the intestines, which I am convinced were the consequence of the irritation of the purgative medicines operating on a deranged condition of nervous energy, there being no previous fullness or tension in the abdomen ; while the deep-rooted pain and throbbing in the head, and the increased temperature of the forehead and temples, with the other general symptoms of in-

flammatory fever are sufficient evidence that the disease itself was inflammation. I ought perhaps to state, that I was repeatedly exposed to the infection of fever about six weeks before the commencement of my illness, and that the last time I saw a patient in fever, was exactly thirty days before my own attack.

“ A. WOOD.”

CASE XXVII.

Mr. Oram, of St. Paul's Church-yard, *æt.* 23, after complaining slightly for ten days of pains in the limbs, listlessness, and want of appetite, and which he ascribed to having taken cold, was taken decidedly ill with the usual symptoms of fever, which soon confined him to his bed. The case was treated at first by sudorifics, as camphor mixture, &c. He was seen on the seventh day by Mr. Pettigrew, who then took away 14 ounces of blood from the arm, and applied eight leeches to the temples. Delirium had previously come on. The blood was covered with much size, and after standing twelve hours the crassamentum was contracted into a globular form. The common antiphlogistic regimen was employed for some days. The disease, however, continued its progress, and all the usual bad symptoms appeared. The pulse becoming

weaker and more frequent, viz. 110, on the tenth day wine was given in regulated quantities; at first to the amount of half a pint, and subsequently a pint, in 24 hours. Stimulants of other kinds were likewise resorted to, as the debility increased. He died on the fifteenth day, having suffered all the ordinary symptoms of violent fever, but in the most simple form.

We examined the body 24 hours after death. The mouth, tongue, and teeth, were covered with a black crust. The arachnoides was raised from the pia mater by interposed serum, and the membrane itself opaque and thickened. The surfaces of the hemisphere below the falx adhered together so firmly as to tear the pia mater from the brain in separating them. The vessels on the upper surface of the brain were extremely numerous, and turgid with blood. The medullary substance, when divided, exhibited a faint tinge of red, losing the whiteness of health, and approaching somewhat to the colour of the cineritious part. Very numerous bleeding points appeared on the divided surfaces. The lateral ventricles contained two or three drachms of limpid serum, and the posterior ventricles were considerably distended. The septum ventriculorum was opaque, thick, and very firm. The velum interpositum was pressed upwards by the

serum contained in the third ventricle, and was as thick and firm in its substance as the septum lucidum. The vessels on the floor of the ventricles were large and distended; the plexus choroides large and florid in colour. There was much fluid effused below the tentorium, and down the channel of the spine. The pia mater covering the pons varolii and neighbouring parts at the basis of the brain was exceedingly firm in its texture, and bloodshot, like an inflamed tunica conjunctiva; and all the vessels round about were distended with florid blood. The substance of the brain altogether was remarkably fresh and firm.

A suspicion exists that this case originated in contagion, as the patient, about a fortnight before he was taken ill, assisted in carrying his sister up stairs, while she was labouring, as was supposed, under typhus fever, from which, however, she recovered.

The existence of inflammation in the brain in this case will scarcely be questioned; yet none but the ordinary symptoms of malignant fever were present. The employment of blood-letting on the seventh day, to the extent stated, gave no sensible relief.

CASE XXVIII.

Feb. 13. 1818. A stout and robust man was taken ill of fever, accompanied with sore throat and catarrh. He attributed his illness to his having been chilled while sweating in his work, which was that of a carman. He continued to follow his work for two days, and nothing was done for his relief for two days more. I then directed 16 ounces of blood to be drawn, and this to be followed by a repetition of emetics and purgatives combined. A few ounces only of blood were obtained. The next day he was somewhat relieved, but grew worse on the following, delirium having taken place. Cupping was prescribed to the extent of twelve ounces; but this also was ineffectually done, eight ounces only being drawn. At this time, he became furiously delirious; his senses quickened; eyes brilliant, though before they were dull and inexpressive. This phrenitic state did not last more than twelve hours, when he sunk into a state of stupor, and died on the ninth day of the disease.

An examination after death was made by Mr. Sawrey, and the appearances corresponded so nearly with those he had before observed in Mr. W. A. T.'s case (XX.), as to make a particular

recital of them altogether unnecessary. The only additional circumstances worth notice are, that the *centrum ovale* was of a brownish colour in some places, giving it a striated appearance ; and the *pons varolii*, when cut into, was of a pink colour. The vessels in the ventricles were not so conspicuous, nor the lining membrane of these cavities so much thickened ; nor was the *pia mater* covering the different parts at the basis of the brain so highly vascular as in the former case of Mr. T., where there were no phrenitic symptoms.

CASES XXIX. XXX.

Feb. 22. 1818. Mr. W. Radnor, *æt.* 16, son of Mr. R., apothecary in Surry-street, was attacked with fever yesterday, which probably proceeded from contagion, as he had been in attendance for some weeks previously on some patients ill of fever in the St. Clement's work-house. The head, as usual, was the part chiefly complained of. The symptoms altogether were so severe as to render him unable to sit up, though he attempted to do so.

He was bled this day (the second) to fourteen ounces, when he fainted. The headache was relieved in some degree. The tongue had a brownish crust. On the 3d, he was again bled

to eight ounces. On the 4th, blood was drawn by cupping to six ounces; and he lost four ounces by hæmorrhage from the nose. Eight leeches were likewise applied to the temples. The pain in the head was nearly removed by the bleeding, but was renewed on the slightest movement. The febrile symptoms continued till the 7th day, and then terminated by profuse sweating.

March 3. 1818. Mrs. R., mother to the above, and who had attended on him closely during his illness, was herself attacked suddenly with the usual symptoms of fever. I first saw her on the third day, and at this time her pulse was so soft, weak, and small, that if this alone were to be considered, blood-letting must have been deemed inadmissible. She was weak also, from much previous anxiety she had suffered. Consistently with the view I entertained, however, of the nature of the disease, with its short continuance, I felt no hesitation in advising blood-letting, which was done to the extent of 14 ounces by weight, instead of eight, which I had recommended. The blood was very sizzly. She felt so much relieved the next day, that it was not judged necessary to repeat the bleeding.

On the following, however, the symptoms continuing, 10 ounces more of blood were drawn,

and had the same appearance as before. The headache was again relieved, but it increased the next morning (the 6th), and she was again bled to 12 ounces. The blood was more sily than before. The headache notwithstanding was so severe as to make her moan continually, and she dreaded the approach of night. Although the pulse was weak, the severity of the pain and the appearance of the blood seemed to me to warrant further bleeding, and considering that if the disease was allowed to proceed, the worst consequences might be apprehended, I advised blood to be taken away till the headache abated, or till some other decided change was induced. When about 16 ounces were drawn, she grew easier, and passed afterwards a quiet night.

The fever went on, with occasional exacerbations of pain, but no delirium till the 11th day, after which she became convalescent. She experienced much relief on the 8th from the application of leeches to the temples. When the fever subsided, she looked extremely pale, and exsanguine, and had a difficulty in keeping her extreme parts comfortably warm. She took, besides, the digitalis, and latterly mercury ; but both without any sensible effect.

Although this case did not immediately and entirely give way to the blood-letting, it went on without exhibiting any of the usual bad

symptoms, such as delirium, and subsultus tendinum; and from a consideration of all the circumstances, I think it may be fairly concluded that the termination would have been less favourable under a different treatment.

CASE XXXI.

The following is a communication from Mr. Blegborough, to which I have subjoined a few remarks. The bearing it has upon the general question, as to the seat and nature of fever, needs not be pointed out.

“ Dean-Street, Finsbury Square, Jan. 1818.

“ I thank you very cordially for your kind attentions; how valuable and consolatory they were to me, you will be well able to appreciate, who know how entirely I enter into your views of fever. I shall ever entertain a lively recollection of the manner in which you rendered me services so important; and I feel an increased esteem for my brother, for procuring me your opinion, and for the ready and cordial manner with which he entered into your views of the case.

“ As you wish to have the early symptoms of the case, viz. of the first five days, (it being on the sixth day of the disease that you did me the

favour of seeing me,) I will endeavour to put them down, with such other circumstances as seem to me worthy of remark.

“ For some days previously to Monday 22d December, I had fancied all food to be of bad quality. On 18th and 19th, I had been acutely sensible to noise, particularly to that of carriages in the streets; so much so, that I had more than once supposed that a long-coach had been upset behind me, though it was going at the ordinary rate. On 19th, I had been particularly hurried until dinner time, and, having been put in motion, it seems that I could not be still; for I proposed to my sister to go to the play in the evening. The theatre was very full and hot; we could not get into the pit, and could find no other place than the upper side-boxes, even with the two-shilling gallery, where we were exposed to the blaze and heat of the gas, which were very powerful and unpleasant; we were much amused, however, and sat to the end of the performance.

“ On Monday, 22d, about noon, I perceived that I had some languor, indisposition to move, and giddiness. At 1 o'clock, I took some pills, with calomel, and extract of jalap; at 3, eat some apple-pudding, and at 5 was sick. The pills operated well; at night I had headache and throbbing of the arteries, with increased heat, particularly of the *head*; I could not

sleep, but had no delirium. During the night, I applied cloths wet with spirits and water to the head. There was on this, as on every subsequent night during the continuance of the disease, an obvious accession of fever, from 1 o'clock till 4.

“ Tuesday, 23d. I passed the day tolerably quietly; and the night nearly as the preceding one.

“ Wednesday, 24th. The power of *voluntary motion* was obviously less, the *headache* was greater, and I was again sick. I repeated the cathartic pills in the morning, and at noon lost 1 lb. of blood. I laid down for two hours; I had got up and sat by the fire about an hour, when complete syncope came on, and I was again sick. On this night, added to the other symptoms, I had continued delirium: at first some scenes of a pleasant character passed across the mental canvas; but soon, more unpleasant impressions followed: I suffered much from the pursuit of enemies and madmen, and from various personal dangers and injuries. The cold applications gave me momentary relief only, and that followed speedily by a considerable increase of heat, and of all my distress. At 5 in the morning, hot and suffering beyond endurance, I got up, and sponged myself from head to foot with cold water, for several minutes, till shivering commenced; I returned to bed, and remained cooler till 8, when I rose weary of bed and

harassed by the sufferings of the night. The pills operated well.

“The day and the night of the 25th were passed nearly as the preceding. I suffered so much from one till four that I determined not to go to bed the next night, till five in the morning. I may say, that from the first I had had a pain resembling rheumatism on the top of the left shoulder, extending along the clavicle and the first rib: it gave me no other inconvenience than that of preventing my lying on that side.

“On Friday the 26th, I repeated the pills, with a small quantity of elaterium added to them, which caused them to operate abundantly all night. I did not go to bed till five in the morning. I then applied the cold cloths to my head, and went to sleep for about half an hour, at the end of which time I awoke, burning, delirious, and in the greatest distress: sousing the head in cold water was quite unequal to calming the disturbance, and I was obliged to rise and sluice myself from head to foot with cold water for a considerable time; I then laid down again, but the heat immediately returned in an intolerable degree, and I was compelled to get up.

“In the condition occasioned by this distress and suffering, and by having been up all night, you saw me, Sir, on Saturday (the 27th). I

dare say the state of the pulse, of the continuance, as well as all the other symptoms, gave you the impression that the disease would turn out a very violent case of the prevailing fever; such was, certainly, my own idea of it. You will remember the dread I expressed of following your direction of going to bed. That night I used no cold application; and though the accession was obviously marked as before, yet the degree of heat, of *delirium*, and of suffering, was so wonderfully less than on the two preceding nights, that I scarcely know to what cause to refer so favourable a change. The elaterium had certainly operated very profusely. I had had in the morning a profusion of hair cut off, and a blister, you will recollect, was applied to the neck.

“ The rest of the case passed under your own observation, and your remarks will be much more pertinent than any I can offer. You know that by simply keeping up the action of the bowels how mildly the disease ran its course till Thursday, the 1st of January, and the 11th from the first attack. You will recollect my observing to you that the moment the affection of the brain, be it what it may, which is the essence of fever, had terminated, how quickly and easily I could use muscles which a few hours before had been powerless.

“ I have mentioned above, a pain on the top of the left shoulder and clavicle : on Tuesday 29th, and 9th day of the disease, this pain changed its station, and occupied the left side from the margin of the ribs as high as to the fifth rib : it was obviously pleuritic, and was, as you know, kept under by the application of three successive blisters and of leeches. — Whether this affection of the side is an essential part of the prevailing epidemic, how far it may have had a beneficial effect on the action going on in the brain, and whether it contributed any thing towards suspending that action earlier than it might otherwise have terminated, are questions perhaps not unworthy of consideration.”

“ HENRY BLEGBOROUGH.”

To the above account, as given by the patient himself, I may add that I first saw Mr. B. in consultation with his brother Dr. B., on Saturday Dec. 27th, 1817, and the 6th of the disease. He was then sitting on a sofa ; as he thought his disordered feelings, about his head particularly, were worse when he lay in bed. He had still, however, an appearance of strange sights, with equally strange thoughts, whenever he closed his eyes for a few seconds. He observed that the pain in the head had become less violent, as the mind became more disturbed.

His pulse was about 90, rather below the healthy standard as to strength and fullness; and it frequently intermitted. This, he observed, he had generally found to be the case, when he was unwell. The tongue was thinly covered with a whitish fur. He had a constant feeling of nausea. His countenance indicated much distress. The skin was hot and dry.

He considered his disease to have originated from contagion, for he had, just before his illness, been for a fortnight in daily attendance upon a boy labouring under *typhus* fever in a severe form, and, when paying his visits, had been of necessity placed immediately between the patient and the fire-place; so as to be in the direct current of the effluvia issuing from him.

After the blood-letting and active purging that had already been performed, there could be little hope of at once interrupting the further progress of the fever, by any means; it was therefore determined to adopt a mild, aperient plan of treatment. The extremities, which were cold, were directed to be kept warm by artificial means; and he was strongly urged to confine himself to bed: 5 or 6 grains of rhubarb were directed to be taken once in six hours, or so as to procure at least three or four stools daily, and a blister to be applied to the back of the neck,

in the hope of relieving the distressing feelings about the head.

He was to take the common saline draught at intervals.

Sunday, 28th. He had taken three doses of rhubarb, 6 grs. in each, from which there were 4 or 5 stools procured. The aggravation of symptoms which usually took place about 1 or 2 in the morning, had been much less than before. He had had several short naps, altogether amounting to a couple of hours through the night, but without being at all conscious of the length of time he had slept at once. Thus, after sleeping only for about five minutes, he awoke, and fancied he had been asleep for two or three hours. He was more composed in his mind and feelings, though still disposed to see phantoms, whenever light was excluded from his eyes. There had been a gentle perspiration, and the skin was not excessively hot. The pulse was about 90, unequal both in force and frequency, but without absolute intermission. He complained of but little pain in the head, and that only now and then, towards the back part, where the pain and throbbing were chiefly seated from the first. The countenance was much less distressed than the day before. From this time, the affection of the head rapidly subsided. The pain in the side, alluded to in his letter, was re-

lieved by the repeated use of leeches and blistering; and in three or four days, he was entirely convalescent.

CASE XXXII.

Dr. Blicke, of Great Coram Street, Brunswick Square, a staff-surgeon in the army, while walking on a cold day at the end of December last, and being greatly heated by exercise, received a violent blow from a bar of iron on the tibia, the pain following which was so severe, as to compel him to stand still till he became greatly chilled with cold. This was about noon. The pain of the leg gradually abated, and he continued well till about 8 o'clock in the evening, when, on stooping down to pull off his boots, he felt a swimming in his head; and very soon after, on suddenly rotating his eyes, he experienced a lancinating pain over the forehead. This symptom kept increasing, and he passed a restless night. The next day, the headache and pain over his eyes were very severe, and his taste became vitiated. He, however, went abroad on business, though suffering much. In the evening, he had a severe shivering fit, which lasted two hours. He continued up, however, in company till a very late hour, had a feverish night, and awoke worse the following morning, parti-

cularly in regard to the pain in the head, flushed face, and bad taste. *He felt a distinct pulsation in the centre of the brain.* He remarked that his *vision* was more impaired than his other senses; for when he looked *attentively* at any object, he immediately turned giddy. About noon, he was bled to the amount of twelve ounces, and he lost nearly as much more by the bandage repeatedly slipping in the night.

He fainted on bleeding; and, as soon as he came to himself, was seized with violent spasms of the muscles universally, amounting almost to tetanus, and which lasted for half an hour. These spasms were renewed afterwards in a less degree, for 24 hours, whenever he attempted to put the muscles in action.

He was advised to take small doses of an aperient medicine every four hours; but, by a misreading of the prescription, he took three times the dose that had been ordered. It both vomited and purged him violently, his bowels having been rendered highly irritable by a previous attack of dysentery.

From this period, the pain in the head, and all the febrile symptoms, declined, so that he was able to go abroad on the third day.

CASE XXXIII.

Sept. 27. 1818. Mr. Shipman, surgeon-apothecary, of Aylesbury-street, had been much engaged among fever patients in his neighbourhood, and was himself severely attacked. He had slight symptoms for a week previously, to which he paid no attention. He was bled on the *third* day after the decided attack, to the amount of 12 ounces; was immediately relieved from his headache, and got up the following morning, thinking himself well. Soon after rising, the pain in the head returned, and was aggravated upon the slightest motion; he made an effort, however, to go abroad. He felt all the arteries within the head strongly pulsating, and his ideas were wild and confused.

He was worse the following day, and compelled to give way,—was bled again to eight ounces,—and the same evening, (the symptoms having greatly increased,) to 24 ounces. The relief was very great, and he passed a tolerable night. The pain in the head and other symptoms continuing, though more moderate, he was again bled to 10 ounces, viz. on the sixth day. From this time the fever ceased, and his recovery was rapid, so that in three or four days he was abroad in his business; active cathartics

and the digitalis were employed, in addition to the blood-letting.

Mr. S. says he scarcely regrets having gone through the disease, as it has served to convince him, from his own feelings, both of the seat and nature of it, and of the decisive advantage of blood-letting for its relief.

He has since, he informs me, had numerous opportunities of confirming, in his experience, the truth of these opinions.

I visited two of his servants, similarly affected, after Mr. S. : the disease in both quickly yielded to the same treatment.

In a family of six children of different ages, from 3 to 10, who, with the mother, were all ill of fever in succession, and whom I visited with Mr. Shipman, the contrast observable between different modes of treatment was very striking.

The eldest was not bled, and she was not considered out of danger at the end of a fortnight ; and it was a month before she was able to walk.

The others, with the mother, were bled early and repeatedly, and recovered very quickly.

CASE XXXIV.

Aug. 29. 1818. Mr. Powell, surgeon of artillery, residing in Surry Street, having attended several patients in fever previously, was attacked

with the usual symptoms. He felt the pain in the head chiefly along the course of the sagittal suture. His brain, he said, felt as if too large, or as if strongly compressed with a weight: the slightest cough made the pain intolerable.

At my request, he was bled on the third day to 16 ounces; on the fourth to 14 ounces; and on the sixth, to 10 ounces: in all, 40 ounces. On the following day, the seventh, the fever was cured. The blood *first* drawn was slightly *sizy*; the *second*, not at all so; the *third*, extremely so. The relief to the headache at each time was immediate and striking.

I think it not improbable, that if a larger quantity of blood had been drawn at first, it would have been unnecessary to repeat the operation; by which less blood altogether would have been lost, and the strength proportionably less reduced.

Mr. P. has furnished me with other instances of the power of blood-letting to subdue fever, even when employed at an advanced stage of the disease.

CASE XXXV.

Oct. 5. 1818. I attended, with Mr. Blegborough, Mrs. W. of Cornhill, a delicate woman

of about 36 years of age, who had been much subject to uterine hæmorrhage. The case had been neglected for eight days, when she was bled to twelve ounces. The blood was remarkably sizzly. The pain of the head had disappeared on the following day, and did not again recur. Pain now took place in the abdomen, which was suspected to arise from inflammation of the peritonæum; but it left her the following day.

Her strength declined rapidly after this, but she felt no pain, and always expressed herself as getting better. She sighed much and often; the eyes were slightly suffused. Slight confusion of intellect was observed on the 12th day. She slept exceedingly little. The pulse was feeble, 120 in a minute. Tongue thickly crusted, and becoming darker coloured as the disease proceeded. She gradually sunk. Her respiration was slow; stupor took place, and she died on the 15th day of the disease, without any subsultus tendinum, or muttering delirium, that so generally attend fever that is about to prove fatal.

The body was examined by my friend and colleague in the Dispensary, Mr. Young, who has favoured me with the following report.

“ *Inspection of the Body of Mrs. W., No. 30. Cornhill, æt. 36, thirty Hours after Death. By George Young, Esq.*

“ *Head*: blood coagulated in the longitudinal sinus.

“ No traces of disease of structure in any part of this cavity; but evidence, I think, of disordered action was quite obvious. A quantity somewhat considerable of watery effusion between the *tunica arachnoides* and *pia mater*, opposite the anfractuosities. The duplicatures of *pia mater*, lying in the anfractuosities appeared very vascular; much more so than is ever found when there has not been increased vascular action in the brain; the large superficial veins were quite full of blood.

“ The substance of the brain was very remarkably firm, — on no occasion have I met with it firmer, — very rarely so firm; the number of bloody points seen on cutting through the medullary mass, was very considerable; they were also unusually large. There was remarkably little moisture in the sinuses of the brain.

“ The *pia mater* at the base was even more turgid than on the superior surface of the hemispheres.

“ As usual, there was some gritty matter in the pineal gland.

“ *Abdomen.* All the organs appeared quite sound ; the intestines contained little else than air ; there were not any traces of inflammation.

“ The liver was rather flaccid than gorged.

“ The gall bladder, as usual, contained bile.

“ The urinary bladder contained a great deal of urine, and yet was not on the stretch.”

Oct. 7. A daughter of the above, *æt.* 6, was attacked with fever, two days before her mother's death. The symptoms were such as to portend a severe disease. She was bled on the third day to six ounces, and cathartics administered. The blood was sizzly. The symptoms were in a great measure relieved on the following day, and from this time she became convalescent.

CONCLUSION.

In the employment of so powerful an agent as blood-letting, one that is so capable of doing mischief by mis-application, there are three things that ought to be considered, each of which has been, more or less, the subject of dispute among practitioners, — 1st, the *safety* of the remedy ; 2dly, its *utility* ; and, 3dly, its *necessity*.

As to the first point, the *safety* of blood-letting, as a remedy for fever, under certain cir-

cumstances, — the cases here adduced must be admitted to be decisive.

The *utility* of the practice, on numerous occasions, is, I think, scarcely less certain. The feelings of the patient, and the obvious advantage almost immediately accruing, furnish proof which it would be unreasonable to question.

The third point, its *necessity*, admits of a less precise determination, and therefore is more likely to be a subject of dispute. The question is relative, and one of comparison merely with other modes of practice. It is rather matter of inference than of proof. Those who deny the necessity of blood-letting in any particular instance, can only do so upon the presumption that the case would have terminated equally well under a different mode of treatment; but this, in a particular case, for obvious reasons, can neither be asserted nor denied absolutely. The point can only be decided upon general principles. The known power and efficacy of the remedy in general, — the speedy good effects following its use, — and the frequency of these, — are sufficient grounds to form our opinion upon.

Many who oppose the practice of blood-letting as an absolute cure for fever, have done so from preconceived opinions, rather than observ-

ation. Thus Dr. Cheyne, in his truly valuable Reports of the *Hardwicke Fever Hospital* for 1816-17, says, "*some few young and plethoric patients* were let blood at the commencement of the fever, *to moderate general febrile re-action.*" "Blood-letting," he adds, "in six cases out of seven, was practised to relieve determination to the head, the lungs, the liver, or some part of the alimentary canal." Such testimony, it is plain, neither makes for nor against blood-letting, as a positive and absolute cure for fever. The observation afterwards made, — namely, that "blood-letting often strangled the disease in its birth; and, when practised on a relapse being threatened, it several times restored the patient to health in a few hours," — might have given rise, one would have supposed, to a different train of thinking, and have led to a more frequent adoption of the remedy with this view.

While I am writing, another volume has made its appearance on this prolific subject*, professing to give Practical Observations on the *Pathology of Typhus Fever*, as well as on its Cure and Prevention. What is to be expected with regard

* Practical Observations on the Treatment, Pathology, and Prevention of Typhus Fever. By Edward Percival, M.B., &c. &c.

to the *Pathology*, may be judged of from the author's opening paragraph ; in which he states, that " the science of medicine is hardly yet sufficiently advanced to compass the pathology of fever." Notwithstanding this declaration, however, we have a great deal about *venous congestion* ; and *tough mucus* in the stomach, the effusion of *morbidly-concocted bile*, and *vitiating humours* in the intestinal canal, causing frequency, fluctuation, and compressibility of the pulse. The worst kind of pulse in typhus, we are told, is " its undulating character being lost, so that the artery gives the *impression of uniform dilatation*." (P. 69.) This, one would imagine, would be no pulse at all.

This author thinks the doctrine of " coction," derived from the ancients, to be deserving of more attention than some recent theories which have been supposed to supersede it. And as to the treatment, " the tenor of his practice," he says, " is directed, throughout, to the control, relief, and adjustment of the secretory organs."

Before concluding, I wish to observe, that, under certain circumstances of fever, mercury appears to me to be unequivocally useful. When the disease is lingering on to a protracted period,

undergoing little change from day to day, as is sometimes the case, the exhibition of a grain or two of calomel at intervals of a few hours, so as slightly to affect the mouth, has seemed frequently to bring the fever to a speedy termination.

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