

An Inquiry into the nature and causes of fever : with a review of the several opinions concerning its proximate cause, as advanced by different authors; ... including some observations on the existence of putrefication in the living body, and the proper method of cure to be pursued in fever / by Caleb Dickinson.

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Dickinson, Caleb.
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Publication/Creation

Edinburgh : Printed for C. Elliot and G. Robinson, 1785.

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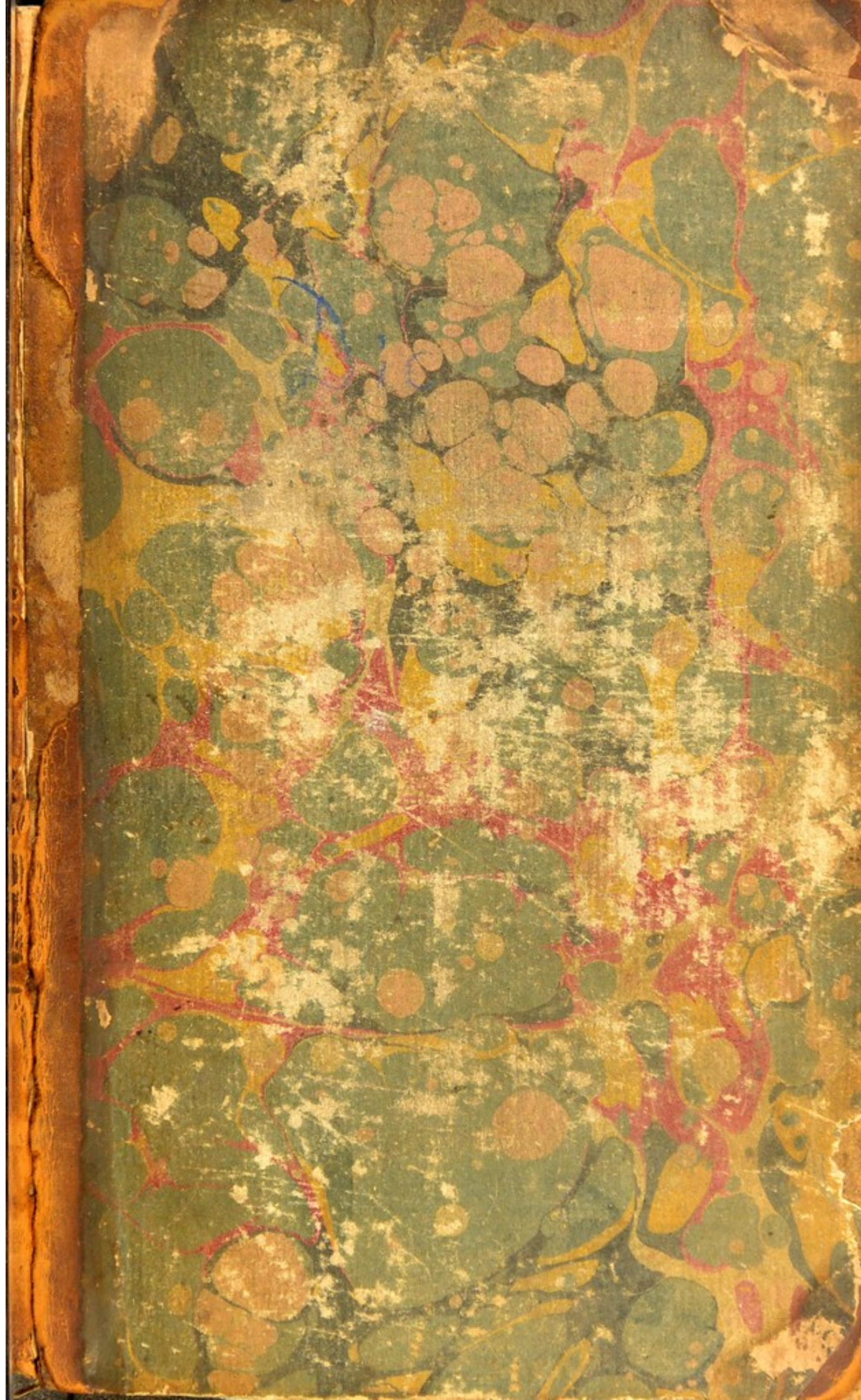
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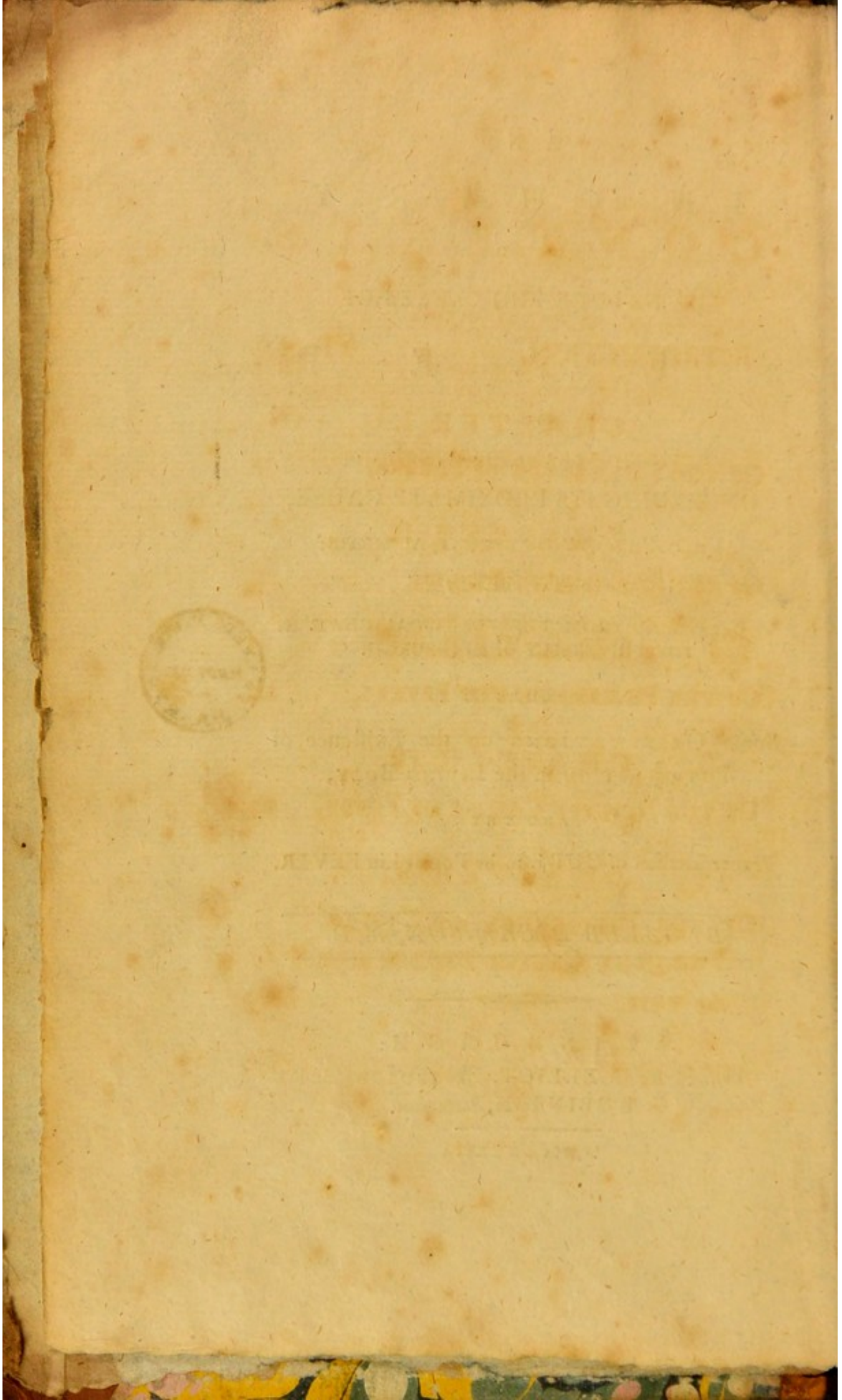
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N A T U R E O F F E V E R.

I N T R O D U C T I O N.

THE diseases afflicting mankind are in themselves numerous, and withal so complicated, that our utmost endeavours to ascertain the truth has fallen far short of our anxious wishes, or the theory founded to explain the phenomena of disease has been opposed for the most part by some very powerful facts. A survey of the several hypotheses, which have prevailed since the earliest ages of physic, affords us a melancholy proof of the assertion; and in particular with respect to fever, many doctrines have at times gained countenance, which at last have sunk into disrepute, partly from their own insufficiency, and

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partly from the greater plausibility of a later system.

To establish a general principle, allowed by pathological writers, in our inquiries, was the motive that actuated me in the present attempt. The principle I allude to, is the foundation of the proximate cause of diseases from the knowledge of the remote; and from a thorough investigation of the latter, we may, I apprehend, generally pronounce what the nature of a disease is, and what remedies will be properly adapted for the removal of it. I have the authority of Dr Cullen for maintaining, that the cure of diseases is to be obtained from the knowledge of their proximate causes; and he observes, that this last requires a knowledge of the Institutions of Medicine, or an acquaintance with the structure, action, and functions of the body in a sound state; of the change it is capable of undergoing; and of the powers which operate in producing this.

It is not presuming on the knowledge we have attained in physic, when I assert, that from the extensive observations already
made

made by authors at different periods, we are not only acquainted with the structure of an animal, at least so much as is connected with the practice of physic; but likewise are versed in many particulars concerning the operation of causes, and which we know will infallibly induce disease. In the present case, then, from the nature and effect of the remote causes operating on the system being well ascertained, it is hoped that the proximate cause of fever, as deduced from these, will not only be satisfactory in explaining the history of the disease, but when extended to practice will evince more fully the justness of my doctrine.

The reader, when he advances in his perusal to the proximate cause, will readily remark, that I have not dwelt particularly on the refutation of the three first systems taken notice of, as being very much out of repute; and my attention is chiefly directed to the consideration of the spasmodic doctrine, now so much admired as being the latest, and which is so ably supported by that ingenious professor Dr Cullen: for

though I can by no means agree in opinion with him, yet I must do him the justice to say, his descriptions together with his arrangement of the phenomena of diseases are by far the best that has been hitherto published, while his rejection of the humoral pathology in very many cases does him infinite honour.

It may be thought in many parts of this treatise, I have been too tedious in reciting quotations from authors; but I must observe, that it is only in particular places, I have been studious to derive authorities, where I have wanted to condemn the pernicious effects of any mode of treatment, or to represent the good consequences from following another. I must however further remark, that I have ever been cautious to support my several opinions by facts from the most respected authors; and if my reasoning should not be admitted, I have this consolation left, that my authorities, on which this is built in some measure, must likewise be rejected. At the same time, I believe, the great loss of most medical opinions is the want of such proper

per vouchers. Advanced as they are, without sufficient authority, and supported only by ingenuity, it is impossible, under these circumstances, they can remain long unshaken.

I cannot conclude these few lines of introduction without expressing my acknowledgements to Drs Lind, Donald Monro, Clark, Millar, and Milman, for many valuable facts and much information derived from the perusal of their works; and I am happy to add, my quotations are chiefly taken from such authors, whose names prefixed to any point of reasoning will always enforce greater conviction.

CHAPTER II

CHARACTER OF FEVERS.

DOCTOR CULLEN, in his System of Nosology, has established a class of PYREXIÆ comprehending five orders. He

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defines

defines the class of Pyrexia very properly, thus: 'POST HORROREM PULSUS FRE-
'QUENS, CALOR MAJOR, PLURES FUNC-
'TIONES LÆSÆ, VIRIBUS PRÆSERTIM AR-
'TUUM IMMINUTIS.'—The first order of this class is the only one that comes under our investigation, viz. Fevers.

The ancients considered heat as a fundamental diagnostic in marking fever. It undoubtedly is one characteristic of the existence of Pyrexia; but it does not imply the essential difference between the class and the order; and therefore, without the assistance of other circumstances, our idea of fever would be but very incomplete.

BOERHAAVE and others conceived, that quickness of pulse was a certain sign of fever, and by it they were regulated as to the presence of the disease. That a quickness of pulse takes place in most cases, there can be little doubt: but there are cases where the pulse does not exceed the natural standard in its frequency of pulsations; in which instances, therefore, provided we had no other more certain diagnostic of fever, we should more than probably pronounce the
non-

non-existence of the disease when it was actually present.

I shall therefore adopt Dr Cullen's definition, as being far more accurate than any other, and which gives us at the same time a very sufficient idea of the disease: 'PRÆ-
' GRESSIS LANGUORE, LASSITUDINE, ET
' ALIIS DEBILITATIS SIGNIS, PYREXIA SINE
' MORBO LOCALI PRIMARIO.'

CHAPTER II.

DIVISION OF FEVERS.

AS fevers frequently change their type, according to climate, method of treatment, obstinacy of the disease, &c. I am inclined to believe that most fevers are essentially the same. I would not, however, with Dr Clark, affirm, there is no inflammatory fever independent of local affection*: for surely the

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fever

* The instances of inflammatory fever, independent of local affection, are, however, very few.

fever that precedes the eruption of the distinct small-pox, when the person has not been inoculated, is undoubtedly a synocha or inflammatory fever; and yet at this period no local affection whatever can be demonstrated: therefore I think we are sufficiently entitled to consider it as an inflammatory fever, and not as a state of pyrexia in consequence of local affection.

The most simple, and, in my opinion, most obvious division of fevers, is into Continual, Remitting, and Intermitting*. The first of these has no manifest intermission; the second remits, or has irregular and imperfect intermissions; and the last has distinct intermissions, during which time the patient is left nearly in a state of apyrexia.

Of the species of continued fevers, it is to be observed, that as inflammatory diseases are of the continued type, and as a
system

* The various distinctions of fevers, into putrid, non-putrid, malignant, &c. that have been at different times spoken of, appear, in my opinion, very abstruse, and are not attended with any material advantage.

system of nosology should lead us to the method of cure in diseases, so we would expect such an arrangement as to accomplish this great and important end. To this purpose it may be remarked, as synocha and typhus are fevers of the continued form, and the method of cure in these diseases so diametrically opposite, I am of opinion a subdivision of continued fevers should be made; the one to comprehend synocha, and the other typhus and its varieties. Respecting remittents and intermittents, we can be at no such loss; and therefore any further division of them would only tend to perplex the memory, without any material advantage accruing from it. It is to be, however, attended to, that in this treatise I am only to speak of those fevers whose remote are debilitating causes, or, in other words, where a state of debility is induced in the system.

CHAPTER III.

OF THE PHENOMENA OF FEVERS.

THE following are the chief phenomena that present on the attack of fever. The patient complains first of a pain in the back and limbs, with considerable weakness in the joints, especially in the knees, and it is with difficulty they can support his body: These symptoms continue generally, though not in all cases, for some time before the accession of the cold fit, when it is ushered in with a slight chillness: the patient finds himself extremely averse to every kind of exercise, being frequently disposed to yawn and stretch his body, feeling altogether as if he was much fatigued and inclined to rest: at the same time the features are much diminished; and in the eye there appears a peculiar dulness and heaviness, expressing
very

very strongly the languor and debility the patient labours under; while in some more dangerous cases of typhus, the eye has been known to be red and inflamed as in ophthalmia, from the very first attack: The surface of the body becomes likewise greatly changed. As the cold stage advances, the skin is found pale and shrivelled, discovering an appearance of constriction in the vessels; and it has been observed the nails turn livid and wan, so that many persons know when the fit is coming on by an attention to this circumstance. These phenomena remaining for some time, advance to that degree, when the shivering is very remarkable, inasmuch that the lower jaw is involuntarily moved upwards and downwards, occasioning what is called a chattering of the teeth: the patient seeks external heat, and is happy when the rigors are by such means alleviated. After this the shivering gradually leaves him, and a heat greater than usual ensues: this generally comes on at first by frequent flushings of the face, which alternate with a chillness, till at last the heat becomes permanent; the
skin

skin now regains its colour, and acquires an additional redness; while the features assume a fulness, and are even so turgid as to appear somewhat swelled and inflamed. At the same time the superficial veins appear distended, which before this period were not easily discovered. As the symptoms of the hot stage come on, a considerable pain is felt above the eye-brows shooting along the temples. This, however, does not always take place; but more or less of an excruciating sensation is constantly experienced near the temples; which, with the beating of the temporal arteries, obliges the patient to toss and tumble in bed, in hopes of procuring an alleviation of his agony. After the duration of these symptoms for a longer or shorter time, a moisture is felt upon the face and neck, which by degrees comes to be diffused in some measure over the whole body; but the extremities are never so moist as those parts which are nearest the heart, and hence liable to be affected in a more powerful manner by its influence. When the sweating appears, the heat of the body is diminished; the pain
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of the head, and the throbbing of the temporal arteries abate, which is more relieved as the sweating increases, till at last the patient is left in a state of apyrexia. But I would not with Dr Cullen assert, that the body is restored to its usual state, and that its functions are not impaired: on the contrary, I would allege, and in this I am supported by the united testimony of many respectable authors, there are marks of a general lassitude and debility; and for the most part the patient is affected with a pain of the head, with a loss of appetite; and in short, there is such a want of activity in the whole system, as sufficiently evinces that upon this occasion the energy of the brain continues still much diminished.

DURING the several stages described, a considerable alteration takes place in many other functions of the body. On the approach of the cold fit, the pulse turns quick, feeble, and not unfrequently intermits. As the hot stage advances, the pulse becomes fuller, stronger, and the pulsations more equal; but when the moisture of the body
enfues,

ensues, the pulse turns softer, and continues full till the sweating ceases to flow, when it returns more to the natural standard, still retaining, however, a quickness, and want of its usual momentum.

Respiration also becomes frequent, short, and difficult; and a dry cough often accompanies it, with repeated sighs. A pain, too, is generally felt in some part of the thorax, mostly in the one side. A sense of oppression is also experienced about the præcordia, which continues till the end of the paroxysm; but this, with the laborious respiration, ceases when the sweat has flowed for some time.

A considerable deviation in the proper exercise of the natural functions, is likewise observable. On the accession of the cold fit, the patient is seized with a total aversion to animal food, the very smell of which occasions a squeamishness and kind of rotatory motion in the stomach antecedent to vomiting: He is, however, extremely desirous of fruit, and especially that of an acescent nature. When the cold fit has continued some time, the nausea that before existed

existed is now so much increased, as to occasion violent retchings: these are so extremely distressing to the patient, that he becomes faintish, and feels as it were a sense of shrinking of the internal parts of his body, especially about the stomach and right hypochondrium. At last these violent retchings end in vomiting: the matter evacuated is generally of a bilious nature; but whatever is lodged in the stomach comes off with the bile, and mostly before it. In some cases of fever that occur, in the warmer climates more particularly, there happens from the beginning of the disease such a terrible vomiting, accompanied with an intolerable pain at the stomach, which at the same time feels hard, tense, and inflated, that the physician is often in danger of losing his patients in the course of a very short time.

A remarkable thirst prevails during the whole paroxysm. The tongue is sometimes whitish; yet this does not take place in most cases, till the disease has existed for some time; but when the tongue is dry and somewhat parched from the first attack, as is sometimes the case in the jail-fever, it
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indicates a more violent disease. There is always, however, from the beginning, a want of the secretion of saliva in the mouth, and the tongue is commonly impressed with a bitter taste.

RESPECTING the other excretions of urine and stool, the former suffers a considerable change. From the beginning of the cold till the end of the hot stage, the urine appears constantly of a more or less red colour, which increases as the hot stage advances, till the sweat has flowed for some time, when it deposits a sediment, previous to which it was clear, and voided only in small quantity. With regard to stools, a costiveness prevails throughout the whole paroxysm, except in some rare cases, as has been alleged by Dr Cullen, where a diarrhoea attends from the commencement of the disease till its termination. He has remarked, too, that analogous to these alterations which happen in the state of the secretions, 'tumours subsisting on the surface of the body, suffer, during the cold stage of fevers, a sudden and considerable detu-

‘ detumescence ; but generally, though not
 ‘ always, the tumours return to their for-
 ‘ mer size during the sweating stage. In
 ‘ like manner, ulcers are sometimes dried
 ‘ up during the cold stage, and return again
 ‘ to discharge matter during the sweating
 ‘ stage, or after the paroxysm is over *.’

Early in the cold stage it is often to be remarked, that the patient becomes exceedingly dejected, fretful, and peevish ; and this is the more observable when he is roused from the languor and drowsiness which particularly attend the attack of the cold fit. On the beginning of the hot fit this drowsiness leaves him, when a watchfulness takes place, and he then becomes extremely impatient of noise. A delirium † seldom occurs in the cold stage: it nevertheless frequently comes on in the hot, preceded by a noise in the ears ; but when the sweat begins to appear, this gradually abates till the paroxysm is over, being then gene-

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* Vide Cullen's First Lines.

† Dr Lind, in his *Hot Climates*, gives us some instances where patients have been at once seized with a delirium and fever without any of the preceding symptoms we have described.

rally relieved of it: there still remains however some confusion of the head, which is more remarkable in the fevers of the warmer countries, and more especially as the fever partakes of the continued type.

These are the principal phenomena to be observed on the attack of fever, and I have endeavoured to point out some peculiarities which occur in a few particular cases. However, upon the whole, I presume that the phenomena I have marked, will be sufficient to convey a proper idea of fever, as I have attempted to apply most of the symptoms to those fevers which intermit as well as to those that assume a continued form.

There are certain circumstances, notwithstanding, that attend some species of fever, which are wanting in others; and of these it might be imagined I should have taken notice: but as my view was only to lay down those that constantly accompany the attack of almost every fever, I conceived it more requisite to mark them; and this the rather, as such peculiarities do not happen in general, till the disease has existed for some time,

to

to enumerate which is foreign to my intention.

The phenomena that have now been described constitute what is called the Paroxysm of a fever; and it sometimes happens that a fever consists of only one paroxysm, termed Ephemera, or fever of one day's continuance. But it more frequently occurs, that a fever consists in a number of paroxysms; the period, however, from the end of the one to the beginning of another paroxysm differs in different forms of fever. Dr Cullen observes, he never saw in a long course of practice any such fever as has been termed continent, or fever pursuing its career without any manifest intermission or remission. I am notwithstanding disposed to believe, that though every fever in the beginning shows marks of remission, yet when it has considerably advanced, and the situation of the patient becomes highly dangerous, I never could observe the slightest alleviation of the symptoms; and I think in such cases the term Continent is by no means improper: for the patient has continually *subfultus tendinum*, with frequent pickings

at the bed-cloaths, and lies on his back comatose for two, three, or more days before death; during which time, in many cases that I have seen, there was no remission whatever; and the disease at that period seemed to be a continuation of symptoms, which, as it hurried on to its fatal issue, became more and more aggravating, till death at last closed the scene.

C H A P. IV.

OF THE REMOTE CAUSES OF FEVER.

AS the phenomena of every affection to which the human body is liable, depend upon the operation of the remote causes inducing it; so it is evident, a knowledge of these enables us to judge with more certainty of the nature of the disease, than what we could possibly entertain provided we had no satisfactory account of the remote causes;

causes; and which, though even we endeavour to ascertain to the utmost, our researches are in many cases attended with very little success. However, in the disease now treated of, from its very frequent occurrence, and from the accurate observations made by many different and respectable authors, no such difficulty arises; and it is therefore presumed, that the causes to be assigned will sufficiently account for the phenomena which have been already described. I shall divide the remote causes of fever into two general heads; and this I consider as requisite, on account of the opposite nature of the several powers which induce it, and, from a general though vague manner of speaking, by which all the remote causes of fever have been considered in the light of direct sedatives. The first division comprehends those that directly debilitate the system; as Contagion, Miasma, Cold, the Depressing Passions, and Hemorrhagies from various parts of the body. The other division has within its view those that indirectly weaken the system; as Excess in Venery, Frequent Intoxication, &c.

CONTAGION and MIASMA have been noted as the most common remote causes of fever. The former has been supposed to be of many varieties, inducing particular diseases according to the nature of the infection. I am much inclined to believe, that the contagious principle acting upon the bodies of persons, produces its effects according to the nature of the constitution to which it is applied, and again to the degree of concentration it possesses. In opposite habits, it is probable, that a very different effect will follow the exposure of the body to a contagious disease: for example, two persons of different constitutions shall be exposed to the contagion of the small-pox; and though they both take the disease, yet I apprehend that the one may have the distinct, while the other has the confluent kind. Nay, there are instances where persons have been seized with a typhus in consequence of being exposed to the contagion of the small-pox*; and on this account the effect of any contagious principle, it would seem, is to be attributed primarily to its
con-

* Vide Lind on Fevers and Infection.

concentration; and secondly to the nature of the constitution. It must be still, however, admitted, that there is something peculiar in the nature of contagion which determines it most generally to produce the same disease; but in what this consists, I confess my utter inability to advance any thing satisfactory upon, and shall therefore proceed to decide upon a question of some importance: Whether a fever can happen in consequence merely of exposure to contagion or miasma; or, Whether a state of debility present, however induced, be sufficient for that purpose? In the first place, it would appear, from the remote causes of fever enumerated by authors, that whatever powers weaken the system considerably, are the means of producing fever. We find, that excess in venery, frequent intoxication, intense study, the application of cold, &c. are constantly mentioned as remote causes; and if contagion or miasma were the only powers that occasion the disease, it would be very absurd to mention any other cause. But I apprehend, that from actual observation it has been concluded, that every debilitating

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cause,

cause, if sufficiently powerful, will itself produce a fever; and on this account I presume it is that different authors have made mention of several different powers, some of which had never been remarked before, and which they found from experience to have given rise to the disease. This is supported by appealing to the cautions of former physicians. Dr Sydenham forbids our administering purges after a fever, as they endanger the return of it; and Dr Cullen assures us, “that when the paroxysms of a fever have ceased to be repeated, they may be again renewed, and are most commonly renewed, by the application of debilitating powers*.” Dr Grant, who has had pretty extensive practice in fevers, gives it as his opinion, that whatever destroys the tone of the solids, and particularly of the first passages whence arise crudities and indigestion, are the sources of fever; and he has therefore reckoned unwholesome provisions, and drastic purges, among the remote causes †. And as a further confirmation of this doctrine, how often

* Cullen’s First Lines, Vol. I.

† Grant on Fevers.

ten does it happen that women newly delivered are seized with fevers from the loss of a considerable quantity of blood? and yet it cannot be conceived any contagion whatever can have operated in effecting this, though notwithstanding it is probable that during the disease a source of infection highly dangerous may be generated. Besides, there are many facts that tend to make this matter almost unquestionable. Baron Van Swieten observes the case of a lady who got a quartan in consequence of a fright; and after she had got well, it returned on account of her being thrown into another fright*. Nay, I have myself seen three cases of continued fever, and one of an intermittent, when upon the most minute inquiry I was certain these patients had not been near any source of fever that could be deemed contagious. And when the French took Bergen-op-Zoom, it was thought that the want of good water, and the scarcity of fresh provisions they found there, was the cause of the epidemic fever which raged with such considerable fury, destroying

* Van Swieten. Comment.

ing a number of the soldiers. From these facts, therefore, I conclude, that whatever causes weaken the system sufficiently, are to be considered as remote causes of fever; and that the interposition of either contagion or miasma is by no means absolutely requisite for the production of this disease.

With regard to the operation of contagion and miasma, it is to be observed, that as their origin depends upon the dissolution of animal and of vegetable matter, so there can be little doubt of their proving sedative when applied to the bodies of men; and the phenomena of fever fully evince this effect to be produced. Doctor Cullen, in speaking of these general sources of fever, supposes, that contagion is the cause of continued fevers, and miasma produces intermittents. It appears to me, however, his supposition on this head does not always turn out to be actually the case. I apprehend that the circumstance favouring the operation of contagion, miasma, or any other debilitating cause, is a weakened state of the system to which these noxious powers are applied. It is more than probable that persons are in
many

many cases in such a habit of body as to almost resist for a considerable time the application of even the depressing power of contagion; but admitting it has the effect of diminishing the vigour of the system in some degree, the debility induced may not yet be sufficient to lay the foundation of a continued fever, without the concurrence of some other weakening cause. In such instances, therefore, it is a probable conclusion, that as the system is not injured so far as to admit of a continued fever taking place, yet it may be fully enough impaired for the production of a remitting or intermitting fever; and in fact, many fevers beginning as remittents and intermittents, while they have been known to arise from miasma, frequently become continued, which should not happen unless the persons so affected had been afterwards exposed to contagion, but any weakening cause will produce the same effect. Besides, Dr Donald Monro mentions, that some agues which came under his care, began in the form of continued fevers, which came afterwards to intermit; and in speaking of the remitting fever,

fever, he expressly observes, there were many cases where the fever went on in a continued form through its whole course, without any signs of remission, though in every respect they had the other symptoms marking the disease; and when the fever came to remit in some, it afterwards changed into a continued type*. The yellow fever of the West Indies, which is of the continued form, and defined by Dr Cullen, "*Typhus cum flavedine cutis,*" has often been known to have arisen from marsh miasma; which is in direct opposition to his idea; and argues, that there the habit of body was such as to favour its operation, and that degree of debility followed as to admit of the disease taking place. We are informed by Dr Lind too, in his *Hot Climates*, an hospital was built at Jamaica, which was denominated Greenwich for its utility and elegant appearance. Its situation, however, was upon a very unhealthy spot, and adjoining to a morass. The consequence of this unwholesome situation was, that when a patient was sent thither with a
mild

* *Monro's Observations*, Vol. II.

mild intermittent, he was seized with a malignant fever, a bloody flux, or some other mortal distemper. The yellow fever raged very much there, attended with the most profuse evacuations; and it was observed, that the recovery of those who had been ill was extremely tedious, while the least indiscretion subjected them to a relapse †. Upon the whole, then, it appears extremely probable, that the effect of contagion or miasma in inducing fevers of the continued, remitting, or intermitting type, depends more on the disposition of the body than on any specific power of contagion producing continued fevers, and miasma intermittents; for if a person labours under any other of the remote causes of fever, and is at the same time exposed to miasma, it is as likely he will have a fever of the continued type as an intermittent. We have a proof that contagion does not always induce a continued fever in Dr Ruffel's work on the Diseases of Aleppo; who observes, that the plague sometimes makes its appearance under the form of an intermitting fever; which argues

† Lind's Hot Climates.

gues what I have before urged, that some constitutions are capable of resisting in a great measure what in others instead of occasioning an intermittent would infallibly be the cause of the most dangerous continued fever. And as a further proof that contagion possesses no specific influence in producing continued fevers, other diseases have followed the application of it; and fevers continuing for some time have at last turned to the bloody flux, dysentery, &c. and these have left the patient when he has again been attacked with the fever*.

COLD is likewise to be considered as inducing fever; but its operation upon the body has been supposed to be so diametrically opposite in different circumstances, and which is still involved in such great obscurity, that I am conscious any attempt to explain it will be attended with many and perhaps unsurmountable objections: I therefore must premise, that upon this head I would wish to reconcile the opposite ideas
which

* Vide Monro's Observations, Vol. II. p. 69. Cleg-
horn's Minorca Diseases, edit. 3d. p. 134.

which different persons entertain concerning its operation; and what is here offered, tends to this end.

Dr Cullen, in the last edition of his work, has properly divided the power of cold into absolute and relative. The absolute power is that by which the temperature of the body is diminished in consequence of its application; and every degree of cold, or, to speak philosophically, diminution of heat below 62 degrees of Farenheit's scale, is found to lower the temperature of the body, while the degree mentioned operates in the proper medium for the preservation of health.

The relative power of cold, again, is that by which a sensation of cold is produced on account of a greater heat being experienced before the application of a less degree of it; and hence it is obvious, that if the opposite sides of the body are exposed to different temperatures, though in fact both these are above 62° of Farenheit, upon this being diminished, a sensation of cold will be felt on that side where a greater heat has been applied immediately before; though at the same time, if the temperature is not
lowered

lowered more than 62° , this sensation of cold will soon go off, and the person will remain in a proper medium, neither experiencing any considerable degree of heat nor cold.

The relative power of cold now spoken of, is not to be taken in the limited sense of a sudden and transitory degree, as this does not operate so powerfully as to be productive of any bad consequences; it is only when the application of it continues for some time, we are made sensible of its morbid effects.

Were we *à priori* to consider, that as heat is so necessary for the support of animal life, and which is not immediately evolved even at its cessation, it might appear absurd a diminution of it should occasion inflammatory diseases; but to explain its operation, it is necessary that we consider those circumstances which particularly determine it to produce in some, perhaps a pleurisy or rheumatism, while again in others a fever of the typhoid species follows its application. It appears not a little surprising that physicians should not in general have paid more

particular attention to those circumstances which favour or resist the presence of hurtful powers, as it would in many cases have afforded a more satisfactory explanation of the effect of causes than have otherwise resulted from their researches. With this view, in the present attempt to explain the operation of cold, I must observe, that whatever difficulty occurs in accounting for it, I primarily have recourse to the particular predisposition of the body, whereby it happens, that if two persons of different constitutions are exposed to the operation of cold, they shall not both be attacked with the same disease; nay, it is probable, the one may be seized with a diarrhœa, while the other gets a pleurisy. And, secondly, if the degree of cold acting, with the manner of its application, is attended to, we may, I conceive, be able to account for its effects.

Cold, then, in its application, may be partial or universal; and it is in the former case more especially, I imagine, it disposes to inflammatory complaints. In the inducing of these, we may observe, that the surface of the body, as more immediately ex-

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posed to its influence, should be primarily made sensible of its effects; and I apprehend this is actually the case. We observe, upon exposure to cold, that the skin becomes pale, shrivelled, and contracted, which thoroughly evinces that the tone of its vessels is considerably impaired. This atony taking place on the surface would be the source of disease to the skin, provided it was liable to be easily affected; but we know for certain, that the outer part of it, the scarf-skin, possesses little or no sensibility, and when the tone of its vessels is destroyed, their diameters will be lessened, if not rendered altogether impervious, which will prevent the cutis vera from experiencing so forcibly the morbid effects of cold. That the cutis vera does sometimes feel its effects, I conclude from the pain which succeeds the application of cold, when particularly the body has been more than ordinarily heated; this pain, however, is merely superficial, and is easily removed by friction, by the warm bath, and by any other means that will restore the tone of the vessels on the surface.

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As the phenomena exhibited on the surface upon exposure to cold, plainly demonstrate that the activity of the vessels is at this time greatly weakened; hence therefore they are rendered incapable of throwing out the perspirable matter. The retention of this then will prove an unusual irritation to the system, and in a more particular manner will affect the internal and larger vessels of the part immediately below that on which the power of cold chiefly exerted itself, causing local inflammation. Thus, for instance, if the breast has imprudently been subjected to cold, a pleurisy will more readily occur than any other phlogistic disease; but if the cold applied has been universal, a general inflammatory diathesis will be the consequence, as in rheumatism.

This effect of cold now treated of, I have no doubt is frequently salutary as well as morbid. I conceive it to be salutary when a considerable heat has prevailed for a great length of time before; and which though it cannot be denied that heat is a highly stimulant power, yet the diseases of the warm countries are far from being of an inflam-

matory nature. A general relaxation is the constant attendant of excessive heat, while in a cold climate a vigour and strength of constitution are to be remarked; and tho' cold be construed in the light of a direct sedative, yet if it be partial in its application, and acts especially on the surface by obstructing perspiration and determining the fluids inwardly, I can readily conceive an inflammatory diathesis may be the consequence: but at the same time I am of opinion it requires a predisposition of the body to these diseases; and likewise that the degree of cold acting be not very much under the temperature of 62° of Farenheit; for provided it is considerably below the degree mentioned and long continued, by weakening the vital principle a palsy or mortification may ensue.

The operation of cold as inducing inflammatory diseases is particularly favoured by the effect of stimulant powers acting in rather more than the common degree at the time. Thus we are sensible, on drinking wine, punch, or any other stimulating liquor, of an increased discharge of urine or
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of perspiration; sometimes the one, sometimes the other, prevails; but whichever secretion is augmented, I apprehend that it tends to obviate any dangerous consequence which the liquor might have if no increased discharge followed the use of such stimuli. Whether, however, this be the case or not, it is notwithstanding sufficiently certain, that persons are at this time more liable to be affected by cold; and the constant effect of it in such is the production of an inflammatory state of the body, as happens in catarrh, rheumatism, and in pneumonic inflammation, &c. At the same time, the certainty of persons being more susceptible of cold when the heat of the body is increased by the use of inebriating liquors, affords us a proof of the manner in which we have attempted the explanation of it as occasioning phlogistic affections, as it is evident that the pores of the skin are then more enlarged to admit of the particles of the perspirable matter flowing with greater facility; and when it may be imagined the application of cold as contracting these, will be more powerful on account of the greater

evaporation going on now from the surface. Another proof, that the power of cold occasioning inflammatory diseases operates on the surface by obstructing perspiration, I apprehend, arises from a fact well known, and taken notice of by Dr Huxham and by every epidemical writer, that pleurifies are most frequent in dry cold seasons, and endemic in high cold situations.

A second way in which cold (for when I use this term, I mean any degree of heat below 62° of Farenheit) may give rise to inflammations, depends on a considerable cold having prevailed for a length of time during the winter, when this has been succeeded by a greater heat as in spring, so that what has been termed cold by authors, and said to have produced inflammatory diseases, may still have operated on the system as an increased degree of heat. This sensation of heat, though perhaps below 62° of Farenheit, depends altogether on the relative power of cold: but whether inflammatory diseases happen generally in consequence of an increased degree of heat, I cannot determinedly say; I am inclined to think
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not. I do not, however, deny the probability of their happening at any time independent of the effect of the air; experience convinces us these diseases are to be met with, though they are not very frequent, in every season of the year: But so far as they are occasioned by heat succeeding cold weather, I would observe, that this sensation of heat, though the thermometer stood below 62° of Fahrenheit, as depending upon the relative power of cold, might be deemed cold to those who had not experienced the greater cold that preceded the increased heat.

Having thus attempted to explain the operation of cold as producing inflammatory complaints, it only remains to account for diseases of debility following its application, and for those circumstances which especially favour it in this. I have hitherto considered cold inducing phlogistic diseases, as operating upon the surface by destroying the vigour of the vessels; and that in an indirect manner it occasions these affections. But where palsies, gangrene, and other diseases, bespeaking an impaired state of the vital principle, fol-

low its application, I apprehend, that in these cases the degree of cold has been considerable, and acted on a large portion or on the whole body; while at the same time a peculiar irritability of system has favoured the operation of this power. There are innumerable instances, where though cold has been only partially applied, yet in irritable constitutions a general indisposition ensues; and when cold is accompanied with moisture, these ailments more readily occur. It has long been remarked, that this sort of weather gives the predisposition to the scurvy, the scourge of our fleets and armies; and nothing has been found more effectual in removing this predisposition than warm clothing. In the rainy seasons of the warm climates, weakly people are then particularly liable to be attacked with the diseases of the climate, which they might in a great measure prevent by wearing warmer clothes than are commonly used in the dry seasons, which is seldom observed; and the generality of persons are often sensible of a weariness, yawning, and shivering, except those who overcome these marks of lassitude by
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such exercises as can be obtained within doors, who keep fires *, and who live in such a manner the most likely to resist the effects of chilly and moist weather.

The DEPRESSING PASSIONS come next under consideration; the effect of them, however, is so well known, that I shall animadvert but little upon it. They have the most directly opposite effect to joy: the latter increases the circulation; while the former diminish the vigour of it, depriving the face of its colour, &c.; and if they are very violent, the powers of life are often suspended for some time. In many cases, they act with such an ungovernable fury as to hurry the

* Upon an average, in most of the West India islands, I do not believe above one house in twenty have fire-places; and yet in no part of the world are fires more requisite than in these islands in the rainy seasons. Besides, many houses, particularly in the country parts, have no cieling; and are only covered by means of pieces of wood, nearly in the shape of what is vulgarly called an oblong square, termed shingles, and these are not united with accuracy, so that the rain comes in through the roof; and I have often myself got into bed, when the sheets have been excessively damp in consequence of the rain coming down from the roof.

the patient very soon off the stage of life, as evidently happens in the scurvy. But in every instance where the unhappy patient is distressed by them, they constantly add very much to the disease he labours under *; and it has been repeatedly observed, when

* Dr Lind, in his preservatives against infection, acquaints us, " It is a received opinion that Fear is a
 " cause of itself sufficient to produce, in certain dispo-
 " sitions, a bad or malignant fever. There are at
 " least many instances in besieged towns, where no
 " other reason could be well assigned for the rise of
 " malignant disorders, than the dejection of spirits,
 " grief and panic of the inhabitants, occasioned by the
 " bombardment, and the apprehensions of a violent
 " death from some sudden assault of the enemy. This
 " much is certain, that such passions of the mind serve
 " powerfully to propagate an infection, even the
 " plague itself. So that on all such occasions, too
 " much art cannot be used to animate with hope and
 " confidence both the afflicted and the sound. Spec-
 " tacles of horror are never to be exposed to the view
 " of sick persons: those therefore who die should be re-
 " moved silently and privately out of an hospital to a
 " proper place, where no idle spectators should be per-
 " mitted to view the ghastly appearance. It is always to
 " be remembered, that every ceremony that is observed
 " relating to a corpse, makes a deep impression on
 " the mind, especially of the afflicted and dispirited,
 " and

when a dangerous fever has made its appearance on board a ship, that the terror and apprehension excited in the breasts of the people, have occasioned immediate sickness. Nor need we be surpris'd at it, when we reflect how much by the agitation of the mind the body is enervated; the appetite is destroyed, the heart palpitates, and such a train of alarming symptoms follow, that no wonder a most malignant fever should be the consequence: and I perfectly agree with Dr Fordyce; who conceives, that when the depressing passions operate upon the system, they afford a more reasonable way of accounting for fevers than all the theory of spasms which have engrossed so much attention †.

HÆMORRHAGIES, if considerable, tend nearly to produce the same effects as the depressing passions. Women in child-bed are often attacked with fevers on account of having sustained the loss of a great quantity
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“ and by such impressions the body is surprisingly affected.”

† Vide Fordyce's Inquiry, p. 54.

of blood during their delivery. And I shall afterwards make it appear, that by the too liberal use of the lancet in fevers many patients have been precipitately hastened to their graves.

OF those remote causes that indirectly weaken the system, I have reckoned excess in venery, and frequent intoxication. They manifestly impair the vigour of the constitution, and will act more forcibly when concurring with any other of the remote causes of fevers. And I believe it often happens, that wrong causes are assigned for the production of a fever, such as probably a trifling exposure to cold, &c. when, upon a more strict inquiry, it might be traced some other more powerful cause had operated in effecting the disease.

CHAPTER V.

OF THE DIFFERENCE OF FEVERS, AND THE
CAUSES PRODUCTIVE OF THIS.

WITH regard to the difference of fevers in their forms or types, it is to be observed, that independent of the inflammatory fever before spoken of, and which I have endeavoured to establish as really existing in some instances, I have given it as my opinion, all fevers are essentially the same; and I have now to add, that the causes which peculiarly induce one species of fever before that of another, must be sought for in the state of body to which these are applied, or in the degree of power these possess in themselves. For that the difference observed in fevers can only be attributed to these circumstances, we may observe, every fever of more than one day's
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continuance, depends still on the power of the remote causes in inducing frequently repeated paroxysms. And respecting the duration of these, Dr Cullen has referred it to a diurnal revolution of the system, as every paroxysm of fever is finished within twenty-four hours; and this revolution he has proposed whether as depending upon the original conformation of the body, or upon the application of powers continually operating upon it, and inducing a habit. But as the Doctor has not favoured us with his opinion in the manner we could wish, I shall therefore endeavour to demonstrate, that on the effect of obvious causes this revolution depends; and that the phenomena constituting it may be made to occur at a more early period than usual, or be postponed till a later time.

It appears to me, that from the earliest date of our existence, while the body is not subjected to be acted upon by foreign causes which afterwards sensibly affect us, we are endowed with a vital principle constantly liable during life to be manifestly influenced

ced by the powers which first produced organization of the parts of the body; and to the support of these parts, the immediate sway of the vital principle becomes indispensably requisite*. What this principle consists in, I apprehend we shall never arrive at the knowledge of. Many have considered it as inherent in the brain. Possibly it may; but I think there can be little doubt of its having existence before the brain or any other part of the body could be demonstrated. We cannot even see how the reception of the male-seed into the vagina and uterus should produce the brain of an animal, supposing its existence necessary to the presence of the vital principle; but in my opinion, though the brain could be demonstrated, it must have owed its being to some power, the knowledge of which the wise Disposer of all things has put beyond the reach of our capacities for the best of purposes, as any
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* Dr Gardiner, in his observations on the animal œconomy, a work very lately published, and which I much regret I had not the pleasure of seeing before this treatise was finished, alledges that the vital principle must be acted on, and he conceives the nerves are the medium through which it is affected.

certainty of it and of future events might prove extremely pernicious and detrimental to our preservation.

WE only know, then, the fact of the continuance of life for a certain time, during which the mind is endowed with a principle influencing in a great measure the action of the corporeal part; but how this effect is produced, or how the mind is affected by injuries the body sustains, we are in utter ignorance of. Our deepest reflections cannot solve it: They only raise our astonishment, while we are bewildered in the research. At the same time that we know nothing of the principle which supports our existence, yet every day's experience convinces us how much this principle is liable to be actuated by the operation of foreign or external powers. For instance, it is necessary for the continuation of life, that we eat to nourish our frames, without which life would not long continue; and many other circumstances are requisite to assist the operation of that inward power which is implanted in us at the time we say an animal

duce their effect in the first place according to the nature of the constitution to which they are applied favouring their operation; and, secondly, to the degree of energy they possess. Therefore from these circumstances we look for the event in affecting the diurnal or other revolution of the system. For as in a state of health the power of causes operates in a sufficient degree for the continuance of it, so we are sensible of the alternations of watching and sleep, of the desire for food, &c. with other changes to be remarked in the intellectual functions which depend upon the time and degree of power applied producing these sensible effects; and that the changes depend upon the causes already assigned is rendered evident from this, that different persons are susceptible of these sensations at different times.

It being thus rendered probable that what we term Diurnal Revolution depends upon the operation of external powers, it only remains to investigate this change that happens in the case of fever, to explain the reason of the repetition of paroxysms. The occurrence of every paroxysm of fever must

be attributed to the effect of the remote causes which have operated or are operating upon the system; and according to the degree of debility induced, their repetition is therefore to be explained. On this principle it appears, that quartans and tertians consist in a lesser degree of debility than fevers of a more continued type; and I would not allege with many, that the periods of the accession of the fit are universally the same, so that the paroxysm of the quotidian commences in the morning, the tertian at noon, and the quartan in the afternoon; for there are many instances where the attack begins at a later period than common, and again it may and actually happens at a more early time of the day. But admitting that the periods of the attack are nearly the same, yet it still implies what I have before endeavoured to inculcate, to wit, the operation of causes upon the body producing disease; for if, independent of this, there was any thing particular in the diurnal revolution of the system affecting the fit of intermit- tents, remittents, &c. every paroxysm, whether of quotidian, tertian, or quartan, should

invariably come on at one and the same period. This, however, is not the case; and I am therefore inclined to believe, that according to the operation of the remote causes inducing a more or less frequent repetition of paroxysms, their attack begins at this or that time of the day: and in support of this it may be observed, that by the administration of the bark, or any other tonic or stimulant remedy, we have it in our power very often to put by the fit for that day; and it is well known, by persisting in the use of these medicines for some time, the disease will be effectually cured. Nay, to render this more clear and evident, I have adduced an instance to prove, when speaking of the remote causes, that a sudden and violent agitation of the mind has immediately induced a fever; and numerous cases have occurred, where persons being at once exposed to a very debilitating power, have been seized with every symptom marking an attack of the disease. Dr Clark observes, that upon visiting patients labouring under a remitting fever, he has often experienced all the symptoms of an attack, though

though not naturally liable to fevers; and if he had not used every method of prevention, it is probable he would have taken the disease *. From which, I think, there can be little doubt, that the circumstances affecting the diurnal revolution of the system, are owing to powers continually operating; for when a person by exposure to debilitating causes gets a fever, he no longer has the desire for food, and the alternations of watching and sleep, with other phenomena, cease to return, or if they do, it is not at their wonted periods; which can only, I apprehend, be imputed to the effect the remote causes have had; for, provided they are highly debilitating, and more particularly if at the same time the constitution of the person favours their operation, the consequence almost instantaneously follows their application.

* Clark on Hot Climates.

C H A P. VI.

OF THE PROGNOSIS IN FEVERS.

FROM the nature of fever, as appearing from the phenomena and the remote causes, it is to be attended to, that our prognostications, on the immediate attack of the disease, or at an early period, should be regulated according to the seeming malignity of it; and from the consideration of the cause or causes known to have induced it; making a proper allowance at the same time for the disposition of the body, whether, on reflection, it is more probable his constitution resisted or favoured the application of them. In continued fevers, a more doubtful prediction should be always formed, than in those fevers which intermit or remit; as when these latter prove fatal, it is generally by passing into a continued type.

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They, however, endanger life, by producing abdominal obstructions, of which the liver is the most common seat; and from hence arise dropfies, and other diseases of the cachectic class, depending on a depraved constitution.

As, therefore, fevers are the more dangerous according as they are continued, I shall in general point out those symptoms which particularly indicate danger; but at the same time, in no one disease to which the human body is subjected, do patients oftener recover, than in fevers, when they have been in the most hazardous situations; and this alteration is often, too, very suddenly effected. In such cases, in the practice I have seen, I observed, that at the height of the disease, the symptoms, though very alarming, have yet continued for a much longer time than ordinarily, till at last the patient has fallen into a sound sleep, when a gentle moisture has prevailed over his whole body; and on waking, to the utter astonishment of his friends and the bystanders, when they expected his death, he has felt a strong inclination to eat, and if he is permitted to satis-

fy his appetite, it is observable he devours his food with the greatest avidity.

The symptoms denoting danger in fevers, I comprehend under the general head of debility arising from the real depressed state of the living principle; and shall follow Dr Cullen in arranging them according as they present in the Vital, Animal, and Natural Functions.

The symptoms expressing a high degree of weakness are,

IN THE VITAL FUNCTIONS:

I. IN THE STATE OF THE PULSE, BEING

- a. Quick.
- b. Weak.
- c. Fluttering instead of distinct pulsations, owing to the impaired action of the heart and vascular system.
- d. And intermitting.

II. THE TENDENCY TO FAINTING,

- a. Manifested when an attempt is made to sit up, as the blood is not then driven in sufficient quantity to the brain.

III. IN THE STATE OF RESPIRATION, BEING

- a. Weak.
- b. Frequent.
- c. Short, and when the air is expelled from the lungs as by a sudden and convulsive motion.

IV. COLD-

IV. COLDNESS OF THE EXTREME PARTS, WITH A CLAMMY FEEL OF THE SKIN, WHEN LIFE IS SO FAR EXTINGUISHED THAT SUCH A RELAXATION HAS TAKEN PLACE, AS TO RENDER THE VESSELS INCAPABLE OF RETAINING THEIR CONTENTS.

IN THE ANIMAL FUNCTIONS:

I. THE INABILITY AND IRREGULARITY TO PERFORM VOLUNTARY MOTION AS APPEARING IN A

- a. Tremulous motion of the hands.
- b. The incapacity to thrust out the tongue.
- c. The picking at the bedcloaths, &c.
- d. Convulsions.

II. THE IMPAIRED STATE OF SENSATION, AS APPEARING BY

- a. Blisters not raising the skin.
- b. The insensibility to light.
- c. The loss of taste.

III. THE WEAKNESS OF THE INTELLECTUAL FACULTIES; as

- a. Loss of memory.
- b. Delirium.
- c. Coma.
- d. Muttering to himself.
- e. Want of sleep.

IN THE NATURAL FUNCTIONS:

I. IN THE STATE OF THE STOMACH; as,

- a. Nausea.

b. Ano-

- b.* Anorexia.
- c.* Vomiting.
- d.* Pain of the stomach, and foreness to the external touch.
- e.* Inflation of the stomach.

II. INVOLUNTARY EXCRETIONS; as,

- a.* Urine.
- b.* Sweat.
- c.* Voiding of the excrements.

III. DIFFICULT DEGLUTITION, DEPENDING ON A PARALYTIC STATE OF THE MUSCLES OF THE THROAT.

And in the general state of the whole body, we may reckon, A, The Hippocratic face; B, The eyes half-closed, with the eye-balls directed upwards; C, Petechiæ, maculæ, and vibices; D, Hemorrhagies; E, The factor of the breath; F, Any local affection of the vital parts, as inflammation, suppuration, &c. All these symptoms, or the greater part concurring, will determine our prognostic: at the same time we should not be too hasty; for from the exhibition of a remedy, a very considerable alteration is produced. The presence of petechiæ is generally considered as leading to a bad omen; and yet Dr Lind has observed, that a single glass

glafs of wine has often effected a great change on these spots: from being black and purple, they have turned red, rough, and scaly; and such has been their mutability at times, that a sudden alteration in the passions of the mind has occasioned their leaving the patient, when a roughness has been remarked in their place*.

With the view of assisting us in our determinations of the prognosis, some physicians have thought there is something peculiar in fevers, whereby they generally remain a certain time, and terminate on particular days, named therefore Critical. These days are the following; the THIRD, FIFTH, SEVENTH, NINTH, ELEVENTH, FOURTEENTH, SEVENTEENTH, and TWENTIETH, which were founded by Hippocrates: but Dr Cullen is almost the only modern author who appears in support of them, and he thinks they apply to the fevers of this country. Notwithstanding of the extensive practice this learned physician has been engaged in, I yet cannot agree with him in opinion. His reasons for believing in the doctrine of
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* Lind on Preservatives against Infection.

critical days are these, and which I shall endeavour to render inconclusive. He first remarks, that the animal œconomy is readily subjected to periodical movements, both from its own constitution, and from habits which are readily produced in it. Secondly, because he observes periodical movements to take place in the diseases of the human body with great constancy and exactness, as in the case of intermittent fevers, and many other diseases*.

Without insisting much on the inefficacy of these arguments, I might at once refer the reader to what has been delivered on the difference of fevers, and of the causes producing it, where I have attempted to prove, that the operation of a proper degree of powers is essentially necessary for the continuance of health; nay, that upon these the discharge of every function both bodily and mental immediately depend; while I have asserted, with marks of the greatest probability, the difference of fevers is to be attributed to the effect of the remote causes; and that this is brought about in
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* Cullen's First Lines, Vol. I.

one of two ways; either they have been violent in their operation, or the constitution has been at the same time susceptible of slight impressions; in which case I have given it as my opinion, a more frequent repetition of paroxysms happens, while a less frequent recurrence is to be accounted for from the nature of the powers operating being less formidable, or when the stamina of the patient by no means co-operated with them. Since, therefore, this effect produced in the constitution depends on causes we can obviously trace, what reason is there for conceiving that the system is still subjected to periodical movements, or to habits occasioned, when these can be readily and satisfactorily assigned to the operation of manifest causes. Besides, I would observe, it is impossible any deviation from a state of health can occur without some power having operated in effecting it. This, however, has often escaped observation; and on this account it has followed, that many physicians have imagined the system readily undergoes certain changes independent of any sensible power: but this is only

a mere conjecture arising from our ignorance, or from not paying sufficient attention to the number and force of those causes we are affected by. Dr Cullen indeed observes, that though the animal œconomy has a tendency to observe the critical days, yet it is by the operation of some violent cause, the terminations happen on the non-critical *. It is evident, therefore, from this sentence, the doctor must on proper reflection conceive, that those powers determining the critical days may occasion the termination of a fever on a non-critical preceding a critical one, or it may be postponed to a non-critical immediately succeeding a critical day. In the former case, the causes have had a greater effect, while in the latter their operation has been probably resisted by the constitution; and admitting this, which cannot be well denied, it is only an accidental circumstance that they may operate in a medium between these two effects, when the termination of the disease will be on a critical day. But what can be said in those fevers particularly

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* Cullen's First Lines, Vol. I.

occurring in the warmer countries, where the progress of the disease is so rapid, that many patients are cut off even before the first of the critical days, as marked by Dr Cullen. There is no periodical movement or any acquired habit which have operated in these cases, but an increase of those violently depressing powers hurrying on the fatal event. For when that degree of debility is induced in the system which is perfectly incompatible with life, death will ensue at that period, whether this be on a critical or non-critical day. Dr Lind, in his *Hot Climates*, gives us a most melancholy picture of the effects of a remitting fever, which occurred to some of his Majesty's ships at Batavia. Many were attacked very suddenly with a delirium, and were carried off in the first fit, but none got the better of the third. The captain of the Panther died though the bark was early administered on account of its extreme malignity*. And Dr Clark, in treating of the remitting fever, tells us, he was informed by a surgeon at Calcutta, that many patients died in the first

* Lind's *Hot Climates*, p. 98.

first fit highly delirious *; which I think admits of sufficient explanation by supposing, that at whatever period the system is so much exhausted as is not consistent with life, death then will inevitably happen. Nay, I would allege, that by different modes of treatment, the disease shall be prolonged to an unusual length, or its termination shall be effected infinitely more early. If for instance a patient is bled once or twice in a fever, I will maintain that patient will be hurried to his grave sooner than another where this evacuation has not been performed, without any regard to critical days; and again, when tonic or stimulant medicines are administered, and when no evacuations have been employed, I am equally confident the disease will be put off to a much longer period than in those patients to whom no remedies of this class are given †. And it is more than probable

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* Clark on Hot Climates.

† Dr Millar, in speaking of the remitting fever, observes, "The duration of the disease is various. It hath sometimes been terminated in death or recovery in a few days; but it hath also been lengthened out for
feve-

bable owing to this circumstance Dr Macbride has asserted, that when the nervous fever destroys the patient, it generally carries him off in the course of fourteen days; if on the other hand he can be supported to the twentieth day or thereabouts, he for the most part recovers †. Nor is it easy to imagine, that when the system is so considerably impaired, that the vital principle is no longer sensible to the stimuli acting upon it; under such a circumstance, the death of the patient will be postponed to the next critical day. Besides, all the critical days, up to the eleventh, mark the tertian period; but from it the quartan is as regularly observed. Dr Cullen himself cannot determine what occasions the periods to be changed about the eleventh day; he, however, takes the fact as certain: but admitting him this, I would ask, what connection

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“ several weeks, or even for some months, with little
“ variation. But when it is neglected, or improperly
“ managed, it generally proves fatal at some period be-
“ tween the eleventh and nineteenth day.” Vide ob-
servations on the prevailing diseases of Great Bri-
tain, p. 64.

† Macbride's Practice.

the tertian or quartan period has with the continued type in determining the next critical day. For in the quartan there are no less than two complete intermediate days between the fits, while in the continued type an exacerbation occurs twice in the twenty-four hours. Not, however, to confine ourselves solely to reasoning, Sir John Pringle tells us he did not perceive any critical days, nor any certain termination of the disease (remitting fever), which was longer or shorter, according to the manner in which it was treated*; and within these four months, a slow fever raged very much here. It was constantly prolonged to the twenty-eighth day; and in some patients I saw the termination did not happen till the patient had been ill five weeks, and even longer. Therefore, upon the whole, I am led to conclude, the doctrine of critical days is not to be attended to; though Dr Cullen has endeavoured to support them by facts collected from the writings of Hippocrates, by *Mr De Haen*. And I am thoroughly convinced

* Vide Pringle on the Diseases of the Army, 7th edition, p. 172.

vinced, that, upon a minute investigation of this matter, were we to note the termination of every case of fever we treat, we should find them concluded as often on the non-critical as on the critical days.

CHAPTER VII.

OF THE PROXIMATE CAUSE OF FEVER.

MANY theories of the proximate or immediate cause of fever have been received; some still are retained by those who were taught the doctrine they at present adhere to: but I shall attempt to point out a few circumstances leading us to reject most of the principal systems that have engaged the attention of physicians: And when we have laid aside these, I shall adopt another, being, as I conceive, more satisfactory in explaining the several phenomena of fever; and as having this farther support, of being

built on the knowledge and effect of the remote causes.

The first opinion that I shall take notice of is, that of a lentor or viscosity occurring in the general circulation, which occasions an obstruction in the extreme vessels of the body, and thus gives rise to the cold stage and the other phenomena of fever. But with Dr Cullen I am perfectly convinced, that the existence of such a lentor or viscosity does by no means appear, or is rendered in any measure probable: for, if we reflect, there are not wanting innumerable instances of fever, where the attack of the paroxysm has been so very sudden, that no alteration whatever could have taken place in the fluids of the body, much less such a viscosity that the blood is prevented from entering the extreme vessels; and hereafter we shall be led to conclude, that the causes act with greater certainty on the sensorium commune.

Another and more universal opinion is, that a hurtful matter, received into the system, or even produced there, is the proximate cause of fever. And further, that the
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increased action of the sanguiferous system is an effort of the *vis medicatrix naturæ* to throw out this offending matter; and more particularly to produce such a change, as to render it perfectly free of any bad effect; or considering it in another light, the making it fit to pass out of the body by any of the excretories. But tho' this doctrine bears a plausible appearance, and is of great antiquity, yet it is founded on principles destitute of what ought to be our conductors in investigating any subject. Besides, fevers are daily induced by cold, fear, excess of venery, &c. with all the circumstances essentially characterising this disease, but at the same time without reasons for presuming on the presence of morbid matter; and it can hardly be imagined the cure of febrile diseases depends upon the expulsion of any such matter, or that it would satisfactorily account for the phenomena; while Dr. Cullen has remarked with great propriety, that the admirers of this system have not satisfied us with the solution how the concoction of the morbid matter is performed, or that any change does in reality take place. And the

remedies to be directed for the cure, it is sufficiently obvious, do not operate upon the fluids. And I shall afterwards urge reasons for rejecting the prevailing notion of a *vis medicatrix naturæ*, when the expulsion of the morbid matter cannot be assigned to this power.

A third doctrine, which has been received, is respecting the quantity of bile ejected by vomiting in intermittent fevers; and this occurs so very frequently, that many have concluded an unusual increased secretion, attended with a peculiar condition of it, gives rise to intermitting fevers: but this, as well as the preceding opinion, appears entirely void of proper foundation; for in other diseases it is observed, that the same circumstance happens where vomiting and purging are peculiar symptoms, and more especially when they concur with the influence of warm climates, and in the hotter seasons of this country. A remarkable instance of this we have in cholera morbus, a disease often appearing without a state of pyrexia; but besides, intermittents, it is generally thought, arise from other causes more

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probable, and to obviate the effect of which the medicines administered give every reason to presume their being of a sedative nature, as the bark and other stimulants are universally known to accomplish the cure; while in a subsequent part of this treatise it will appear, evacuations, directly operating on the alimentary canal, instead of proving beneficial, which they should if a peculiar quality of the bile was the cause of these fevers, have done considerable mischief, and have induced such a purging, as afterwards could be scarcely stopped; and hence the redundance of bile so often remarked in intermitting fevers, I conclude is a mere symptom, connected with and altogether dependent on the state of the system. Dr Mac-lurg conceives, that “ whatever accelerates
 “ the septic animal process, has a tendency
 “ to increase the biliary secretion, since it
 “ favours that change of the blood which
 “ fits it for becoming bile. And is it not
 “ in this light we are to view the large se-
 “ cretion of bile which seems always to be
 “ the consequence of hot weather, and which
 “ shows itself more remarkably in the inter-
 “ mittent and remittent fevers arising from

“ marsh miasma? For that this symptom
 “ is not so much the effect of the intermit-
 “ tent paroxysm, as of a certain state of the
 “ fluids * in hot seasons, appears from an
 “ observation of Mr Cleghorn’s, that when
 “ the intermittents of the summer continued
 “ until the cold weather set in, they lost
 “ their malignity and contagion, and were
 “ no longer attended with the excessive re-
 “ dundancy of bile. And the icteric colour
 “ which characterises the same kind of fe-
 “ vers, is it not to be explained in the same
 “ manner? It is remarked by Dr Lind, that
 “ the serum of the blood distinguished by
 “ this symptom, and named on that account
 “ Yellow Fever, had a bitter taste †.”

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* The considerable quantity of bile secreted in the warm climates, is always an effect, and which accompanies a lax or debilitated habit; for those who are most liable to stomachic complaints, and are more easily affected by trifling causes, experience more of these distressing symptoms marking a bilious disposition, than those who are of a more vigorous constitution. Dr Maclurg has improperly attributed the large secretion of bile to the state of the fluids; but the removing of it does by no means tend to correct any particular condition of them. Bark, chalybeates, and elixir of vitriol, are the medicines generally employed in the cure.

† Vide Maclurg on the Bile.

The next and last opinion which comes under our consideration is that of the spasmodic doctrine, which is so fully illustrated by Dr Cullen. We are told by him, “ The remote causes of fever are certain sedative powers applied to the nervous system, which diminishing the energy of the brain, thereby produce a debility in the whole of the functions, and more particularly in the action of the extreme vessels; such, however, is, at the same time, the nature of the animal œconomy, that this debility proves an indirect stimulus to the sanguiferous system, whence by the intervention of a cold stage and spasm connected with it, the action of the heart and arteries is increased, and continues so, till it has had the effect of restoring the energy of the brain, of extending this energy to the extreme vessels, of restoring therefore their action, and thereby especially overcoming the spasm affecting them; upon the removing of which, the excretion of sweat, and other marks of the relaxation of excretories, take place*.” This is the outline

* Vide Cullen's First Lines, Vol. I.

line of the spasmodic doctrine; and tho' it bears marks of considerable ingenuity, yet I apprehend we are by no means to adopt it. For, in the first place, Dr Cullen acknowledges that debility lays the foundation of fever, and that he can more readily perceive how debility induces spasm than spasm debility; and from hence he draws a very just inference, "that the degree of spasm formed, "and the obstinacy of its continuance, de- "pend in many cases upon the power of "the causes inducing debility, and upon the "debility induced; for the more powerful "the debilitating causes, and the greater the "debility produced, the paroxysms are the "longer, and the more frequently repeat- "ed*." This I conceive to be perfectly just, and

* Vide First Lines, Vol. I.—It has been objected to this, that the quartan is the most difficult of cure of the intermitting types; and were the above account just, it should admit of the most ready cure, because the repetition of the paroxysms is not so frequent. That the quartan does not so soon yield to remedies as the other types, I can readily allow: but still I maintain, that the debility is less than what it is in other intermittents; for we observe, that in every disease, according to the degree of debility, it sooner or later terminates; the greater

and cannot but regret, after his having mention this much, he should have fallen into farther reasoning. It is perfectly sufficient to elucidate by far the greater part of the phenomena that attend fever; and tho' I am thoroughly persuaded that the whole chain of symptoms depend upon debility, yet I allow, some do not admit of so easy an explanation, and it is to be feared we may, respecting such, for ever remain in the dark.

Dr Cullen further tells us, that the spasm is the effect of the *vis medicatrix naturæ* to relieve the debility; but what proof is there brought for the assertion that we possess such a power? I apprehend there is none;
for

greater the debility, the sooner a fatal or happy event. Nor is it easy to conceive, that when the system is extremely injured, it is then capable of withstanding such a shock for a considerable time; the issue must be determined within a short period. The quartan therefore, as a chronic disease, consists in a particular state of debility not in itself great but at the same time requiring the constant exhibition of tonic remedies to overcome it; for the fact is undoubtedly certain, chronic diseases admit not so easily of cure as acute ones, though at the same time no one will allege that the debility in chronic affections is so great as in acute diseases.

for the Doctor appears to have drawn this conclusion from merely considering the phenomena, that when the cold stage has continued for some time, it is succeeded by a hot fit. Why the hot should generally succeed the cold stage, I can by no means satisfactorily explain. We are therefore, then, under the necessity of taking the fact; and notwithstanding Dr Cullen's first indication in the cure, the moderating the violence of reaction, yet I have frequently seen stimulating remedies remove all the symptoms attendant on the hot stage, the anxiety, thirst, the throbbing and pain of the temples, &c. and have been the means of procuring sleep, out of which the patient has awoke in a great measure free of fever, and which could not be supposed to happen if the reaction in any measure required the use of evacuants. Dr Cullen indeed says, that if ever stimulating remedies have been of use, they prove serviceable by their anti-spasmodic power. I shall speak more fully hereafter on these remedies; and from their effect I shall be led to conclude they operate by removing the symptoms of debility, and that
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this is accomplished by directly stimulating. But further, is it consistent in any shape, that if we allow the spasm and the consequent reaction to be an effort of nature to relieve the debility, why at the same time moderate that very power which is operating towards the removal of the disease? Does it not on the contrary appear far more consistent, that we should use our influence in assisting, rather than checking, nature in her effort. I apprehend it does; and if we consult the writings, particularly of those authors who have practised abroad, we shall be deterred from following any method of cure that tends to debilitate the system. Dr Clark tells us, that in three patients whom he bled at Culpee, the consequence was, “ the first did not bear the evacuation, his
“ pulse flagged, and he was very delirious in
“ the ensuing fit, the remissions were very in-
“ sensible, and the exacerbations were only
“ to be known by his delirium. The others
“ were seized very suddenly, and fell down
“ in a deliquium: on opening a vein, they
“ returned to their senses; but before five
“ or six ounces of blood were taken, they
“ became

“ became faint, and the feverish paroxysms
 “ ran higher than in those who did not
 “ suffer the evacuation*.” Dr Lind too, in
 his dissertation on the marsh Bengal fever,
 mentions one of his patients who was not
 only extremely desirous to be bled himself,
 but he excited in the others the same desire.
 The Doctor therefore, to get rid of their im-
 portunities, let blood from him who had
 first requested it; the consequence was, that
 he immediately lost his strength, and within
 an hour, during which time he made his
 testament, he died in the next fit †. The
 further dangerous effects of blood-letting
 are exemplified by Dr Badenoch in the fourth
 volume of the Medical Observations. He
 informs us, that in the fevers of the East
 Indies, this operation is far from being ad-
 viseable; on the contrary, it is extremely
 prejudicial, notwithstanding the fever be
 high. He bled two patients, one of whom
 died the day following, the other fortunately
 escaped by taking the bark. And during
 the rage of the Joanna fever, he always ex-
 pected to procure an intermission, or at least

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* Clark on Hot Climates, p. 136.

† Lind's Thesis, English copy, p. 53.

remission, by the use of evacuations: but he was much disappointed in this; for the fever assumed a continued type, and now and then it had violent exacerbations, under which several were carried off. And being apprehensive he would lose the greater part of those that were then ill, he without loss of time administered to between thirty and forty patients labouring under the various stages of that fever, one drachm of the Peruvian bark with some wine every hour. Several of them were at this time seemingly within a few hours of their dissolution, with the pulse sunk, and an almost universal coldness of the whole body; who notwithstanding, after they had taken a few doses of the bark, turned considerably better, and speedily recovered. And he observed, that this medicine, instead of retarding the natural secretions, on the contrary promoted their discharge, more especially if the patients had not suffered any evacuations previous to its exhibition; and that those who got the bark the earliest, recovered more perfectly than others, who by the effects of evacuants, together with the
severity

feverity of the disease, had been much exhausted before it was given *.

Dr Sandiford too, speaking of an epidemical distemper that was prevalent in the island of Barbadoes, acquaints us, in the hot stage of this disease, (which he denominated a Putrid Remitting Fever) the pulse was so strong, quick, and full, as to have led many to order bleeding: yet such was the effect of this procedure, that the patients so treated were reduced to the most extreme pitch of weakness; and he mentions a case where this evacuation had been repeated in a strong robust man, that he was sunk so remarkably low in consequence of it as to drink several pints of Madeira wine before he was restored to any degree of strength or spirits. The like nearly fatal event, he observes, has been known to succeed the administration of one or two doses of nitre, and even a small dose of rhubarb has induced such a purging as could with difficulty be afterwards mitigated or restrained *.

Other

* Medical Observations, Vol. IV. p. 162 and 166.

† Ibid, p. 308.

Other evacuations, as well as the more debilitating one of blood-letting, have likewise been attended with the worst effects. Dr Lind informs us, “ That large doses of
 “ antimonials, or even smaller ones too frequently repeated, have sometimes brought
 “ on evacuations, which have entirely sunk
 “ the patient *.” And we are assured by Dr Donald Monro, whose extensive practice undoubtedly led him to the observation, that James’s powder has frequently done considerable hurt; for he has known several instances where it has been administered in putrid ulcerated fore throats, and in low fevers, and occasioned such a purging as to hasten the patients to their graves. Neither did Dr James himself think that his medicine was endowed with any specific influence in the cure of fevers; but as far as he conceived a fever admitted of cure, it was to be accomplished by the Peruvian bark: this, we are informed by Dr Monro, was what Dr James repeatedly professed to him †.

As, therefore, such dangerous consequen-

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* Lind on Hot Climates, p. 261.

† Monro’s Observations, Vol. II. p. 13. and 15.

ces arise from evacuations in the warmer countries, I contend that the same sedative power, tho' in a less degree, will take place in the more temperate regions * ; and on this account I cannot sufficiently condemn the evacuating plan which has been so generally employed, as tending to increase the causes of the disease, by debilitating the system already labouring under a state of extreme weakness. And even in this climate, it appears from the two first cases related by Dr Millar in his work on the Prevailing Diseases of Great Britain, that the lancet had destroyed those patients ; but when he altered his plan, substituting the bark for blood-letting, the event was the most happy : and in the third case, when it was certain the repeated bleedings had been of the greatest detriment, yet by administering the bark before that period came on, when probably

* Dr Huxham has known a dose of physic injudiciously given in the beginning of a slow nervous fever instantly followed by excessive languor, syncope, and other bad symptoms ; and he observes, that though it may be requisite at the beginning sometimes to order a vomit or purge, yet every thing drastic aggravates the disease. Huxham, Observ. Vol. I. p. 48.

bably every medicine would have failed, the disease was effectually cured, and the patient restored to perfect health *. Nor need we be surpris'd at the pernicious consequences arising from a number of evacuations when the strength of the system is much impaired, if we reflect how essential it is that it be excited in a proper degree for the preservation of health. If previous to these evacuations, and particularly to the highly debilitating one of blood-letting, it was duly considered how liable we are to be affected by the application of weakening causes, and at the same time how necessary it is that a constant supply of aliment be received into the constitution to supply the abraded parts, &c. and particularly when we daily observe those who eat and drink moderately, and make use of other powers to invigorate the system, prepare no more blood than is consistent with health, while the violent reaction, the consequence of a *vis medicatrix* so much contended for, does not in reality exist; we must be led to conclude how prejudicial these several ways of

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debili-

* Vide Millar's Diseases, p. 91.

debilitating the system must prove, though they were employed in health; then what excuse can be found for those physicians who have recourse to them when the vigour of the system is considerably impaired by the conjoined effects of several exhausting and weakening causes? A certain determined quantity of blood in the system is necessary to give the vessels that proper degree of distention for the due performance of their office; and when this comes to be lessened, a shrinking, paleness, and diminished force of cohesion between the fibres of the body, are the never-failing result. And I would remark, that though there are many diseases which require the use of the lancet, yet it is too often employed, and the quantity of blood drawn on many occasions far exceeds what ought to have been taken. And at the same time, I am very confident, there are diseases, where the phlogistic diathesis is not great, and which would be as effectually removed by the administration of purges, and other medicines of that nature, as by the common and more depressing remedy of blood-letting.

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In further opposition to the *vis medicatrix naturæ*, I would advance, that during the application of debilitating causes, and before that degree of debility is induced in the system which constitutes a fever, this power should be excited into action, either to prevent the disease upon exposure to hurtful causes, or to obviate any dangerous effect which the debility already produced might have: but this is not found to be the case; and during the cold fit, when the *vis medicatrix* is imagined to be operating in forming the spasm, it is at this period that we lose our patients. There are also many symptoms that take place in the course of the disease, which, upon the presumption of spasm, appears to me inexplicable; and how comes it too, in many cases, the disease continues with unabated violence, when we have an undoubted evidence of this supposed spasm being overcome? Dr Lettsom makes mention of a case* where the patient sweated, but without any relief, which could not happen if a spasmodic con-

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striction

* Lettsom's Memoirs, p. 94. See also Gregory's Elements, p. 47.

striction existed on the surface. It may be, however, objected here, that this was a case of putrid fever, as well as the others recorded by Dr Lettsom: but I must beg leave to dissent from this; as the sweating continued for some time, before any symptoms which could be deemed putrescent ensued. We read likewise, in Dr Clark's work on fevers, of several cases where the sweating continued for a considerable time, but without any mitigation of the fever; and the patients did not recover until the bark and other tonic medicines were administered in considerable doses. And as a further proof that Dr Cullen's notion of the *vis medicatrix naturæ*, and of the cold stage, is utterly void of proper support, Mr Cleghorn remarked, that the most severe paroxysms of fever which occurred to him, were those that commenced with a burning heat, without any previous cold fit*. This alone is sufficient to overthrow Dr Cullen's idea. He considers the hot as the immediate consequence of the cold fit; that the *vis medicatrix naturæ*, during the cold, induces

* Cleghorn on the Diseases of Minorca.

induces a spasm in the extreme vessels of the body, which supports the hot stage: but in the instances mentioned by Mr Cleghorn, according to Dr Cullen's own account, it would even appear the vis medicatrix was not excited in inducing any spasm, for there was no cold fit; and yet the paroxysms were more violent than if the attack of the disease had been ushered in with the shivering, as is most generally the case. Dr Cullen would still insist, however, there is some degree of shivering constantly to be remarked; and that in the cases alluded to, this, on account of its being slight, had escaped Mr Cleghorn's observation. But the late Dr Gregory has likewise found the cold fit wanting; and Sir John Pringle, in speaking of the autumnal remitting fever, particularly mentions that the paroxysm returns in the close of the day, but without any cold stage. If, therefore, there was any cold fit to be remarked, it is merely impossible it could have passed unnoticed by these gentlemen, who have been so attentive in noting the phenomena and causes of disease.

Dr Cullen further mentions, that with the

spasm an atony subsists in the extreme vessels: and in speaking of the difference of fevers, we are informed, that the protraction of paroxysms depends upon two circumstances; the one is the presence of a phlogistic diathesis, the other he attributes to a weaker reaction. But Dr Cullen surely cannot conceive, that in typhus, though a continued fever, there are marks of a phlogistic diathesis; for in direct opposition to what he has delivered in one part of his work, he tells us here, " That all the causes of the
 " diathesis phlogistica have a tendency to
 " change intermittent into continued fe-
 " vers," when we were before told, that the duration and more frequent repetition of paroxysms, depend on the power of the weakening causes, and to the degree of weakness induced in the system. Besides, the making synocha and typhus depend on the same proximate cause, to wit, spasm, implies an inconsistency that cannot be admitted. I therefore am of opinion, that in all the species of intermitting and remitting fevers, and in typhus, a very opposite proximate cause induces them than what produces synocha;

nocha ; and wherever a more frequent repetition of paroxysms takes place, it is entirely owing to the presence of a greater state of debility. And this receives additional strength, if we consider what Dr Cullen has himself alleged, that when fevers change their type, it is in the following manner : Both tertians and quartans change into quotidians, quotidians into remittents, and these last become of the most continued kind *. And in further confirmation of this, in those diseases which are truly spasmodic, as the emprosthotonos and opisthotonos, &c. that every remedy which can possibly give rise to a phlogistic diathesis, instead of retarding, we know for certain proves the cure.

And lastly, to conclude my observations upon this doctrine, I would remark a strange deficiency respecting it. We are informed by Dr Cullen, that the paleness and shrinking of the external parts of the body, together with the suppression of the several excretions, may be imputed (and surely very justly) to the weaker action of the heart, in
not

* Cullen's First Lines, Vol. I.

not being capable to propel the blood into the extreme vessels; yet as they frequently remain after the action of the heart is restored, he therefore presumes a spasmodic affection has taken place. Now the question, that would here strike even the least attentive observer, is, when the action of the heart comes to be restored, that as this will have the effect of giving the proper tone to the extreme vessels, and therefore, even according to Dr Cullen's own hypothesis, of relaxing the spasm, what cause is then to be assigned for the continuance of the disease? He conceives an atony of the extreme vessels is a principal part of the proximate cause of fever, and that the relaxation of the spasm depends upon the restoring the tone of these vessels; so it will be obvious, when the action of the heart is re-established in its due degree, as this will remove the loss of tone on the surface, no atony, and of course no spasm can exist, when the continuance of the disease cannot be attributed to either of those powers which he has assigned as making together the proximate cause of fever.

ver *. Besides, I would further observe, Dr Cullen's idea of spasm appears in itself extremely vague. It certainly implies, that the rigidity on the surface is so considerable, and attended with such energy and power, as to resist the impulse of the fluids into the extreme vessels, while they at the same time are deprived of their proper degree of activity. The Doctor seems to be sensible of this objection to his system; but as the explanation of it without such resource would be extremely difficult, he takes it for granted as a matter of fact, and proceeds accordingly. The absurdity of this, however, appears at first sight: it is to say, that a person shall at once be affected with an idiopathic inflammatory disease; while his functions, nay even those functions, the proper exercise of which are immediately requisite for the enjoyment of health, shall be considerably weakened and impaired.

I come now to deliver my opinion concerning

* I cannot see with what propriety any two such powers, as spasm and atony, can be conceived as constituting the proximate cause of a disease.

cerning the proximate cause of fever, and hope to do it with great probability. It is universally allowed, that the knowledge of the remote causes of disease is essential to our entertaining a probable idea of the proximate or immediate cause. Dr Gaubius very justly remarks, “ That
 “ the proximate cause alone constitutes
 “ the whole disease, and is inseparable
 “ from it: it is for this reason it is call-
 “ ed continent, as it contains the whole
 “ sum of the origin of the disease, which
 “ constantly manifests itself when it is
 “ present; and it being taken away, the
 “ disease is likewise taken away. The re-
 “ mote is only a part of this, whatever part it
 “ be; so only produces a part of the disease.
 “ Whoever knows all and every one of the
 “ remote causes, as they conspire together,
 “ has a proper idea of the proximate; and
 “ he who takes away all of them, destroys
 “ the proximate, and with it the disease *.

From

* Gaubius's Pathology, English Translat. p. 17. Dr Gaubius should have said, that if all the remote causes are removed, *provided they have not sufficiently produced their*

From this I aver, that the proximate cause depends entirely on the effect of the remote; and this to be universally the case in every disease afflicting the human body. And at the same time, conceiving this as a general rule without in fact any exception whatever, I am much at a loss to understand how the effect of the remote causes should not always have been adopted as the proximate. Some physicians, merely speculating on the train of symptoms attendant on a disease, have often considered that most of the phenomena were in consequence of some particular one, adopting that indeed which at the same time would admit of the most satisfactory explanation of the other phenomena as depending upon it; and this, though only in reality a mere symptom, they have taken for granted as the proximate cause of the disease. This has been more particularly the case with respect to the spasmodic doctrine: for if any such spasm does exist, it is altogether

their effect, the proximate is destroyed together with the disease. For even before the attack of a disease, all the remote causes shall have ceased from acting, and yet the disease in very many cases follows.

gether connected with and dependent on the state of the system; and hence can only be considered in the light of a mere symptom, and by no means as constituting the proximate cause of fever, any more than the heat, thirst, or anorexia, which are observed to take place. From this it is plain, I build my doctrine of the proximate cause of fever on the knowledge of the remote causes; and as these undoubtedly operate in producing a state of debility, I define the proximate cause to be, That condition of the body induced by the operation of the remote. And in proof of this, I have before alledged that stimulating remedies have frequently removed all the symptoms of fever. But before mentioning the facts to corroborate this, we are first led to inquire into those circumstances which have been hitherto the means of rejecting the use of these medicines. Dr Cullen says, as before-mentioned, if stimulating remedies are of service, that they operate as antispasmodics: But admitting they prove beneficial by their antispasmodic effect, why should not he constantly employ them as such? He tells us, indeed, “ that
“ opium,

“ opium, camphor, and musk, and perhaps
“ some others, have been employed in fevers
“ with advantage; but the circumstances
“ in which they are especially proper and
“ safe, he finds it difficult to ascertain, and
“ therefore cannot venture to lay down any
“ general doctrine concerning them *.” It
is evident, therefore, from this, that the
Doctor conceives they may be sometimes
proper; but the circumstances in which they
are more particularly so, he is at a loss to
determine. But I apprehend, from an in-
vestigation of his own system, we should
not have left the matter so undetermined as
he has done, whether these medicines should
be administered or not, or at least we would
under certain situations have allowed or
condemned their use. He tells us, “ As
“ the hot stage of fever is so constantly
“ preceded by a cold stage, we presume that
“ the latter is the cause of the former; and
“ therefore, that the cause of the cold stage
“ is the cause of all that follows in the course
“ of the paroxysm.” “ To discover the
“ cause of the cold stage of fevers, we may
“ ob-

* Cullen's First Lines, Vol. I.

“ observe, that it is always preceded by
 “ strong marks of a general debility pre-
 “ vailing in the system. The smallness and
 “ weakness of the pulse, the paleness and
 “ coldness of the extreme parts, with the
 “ shrinking of the whole body, sufficiently
 “ show, that the action of the heart and
 “ larger arteries is, for the time, extremely
 “ weakened. At the same time the languor,
 “ inactivity, and debility of the animal mo-
 “ tions, the imperfect sensations, the feel-
 “ ing of cold while the body is truly warm,
 “ and some other symptoms, all show, that
 “ the energy of the brain itself is on this
 “ occasion greatly weakened; and we pre-
 “ sume, that as the weakness of the action
 “ of the heart can hardly be imputed to any
 “ other cause, this weakness also is a proof
 “ of the diminished energy of the brain.”
 “ We shall hereafter endeavour to show, that
 “ the most noted of the remote causes of
 “ fever, as contagion, miasma, cold, and
 “ fear, are of a sedative nature; and there-
 “ fore render it probable that a debility is
 “ induced *.” From a review of the doc-
 “ trine

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† Cullen's First Lines, Vol. I.

trine here delivered, it will at once strike us, that we should have expected the cure to be obtained from the exhibition of tonic and stimulating remedies: for we are told, that the cold stage is to be imputed to a debility produced in the system, and that all the succeeding symptoms in the course of the paroxysm depend upon it; whence the conclusion is obvious, the disease consists altogether in a weakened state of the system. And since this is the case, how unpardonable does it appear in Dr Cullen to have left the use of tonics and stimulants in so ambiguous a manner, as from the foregoing account they immediately strike at the causes and root of the disease? And I will venture to affirm, that in every case of fever, where the remote are debilitating causes, these medicines should be constantly employed; nay, in fact, I have repeatedly seen the administering of them attended with the most happy effects, which there is no reason to allege are owing to any antispasmodic power independent of their stimulant; and moreover, I am perfectly convinced their antispasmodic effect is solely to be imputed to the

stimulant power they possess; as Dr Cullen himself affirms, that the spasm depends upon the application of sedative causes, and that the degree of it formed is in proportion to the debility induced.

Many physicians reject the use of stimulating remedies during the hot fit, which they employed during the cold stage and in the intermission; but I apprehend, no good reason can be assigned for desisting from the use of these medicines. They universally allow, that the cold fit is owing to debility; for, in proportion to the degree of debility, its violence and duration is considered as constituting the very principle on which the cold fit depends. On the commencement of the hot stage, they, however, adopt a very different notion; and conceive now, that the system, instead of requiring tonics and stimulants, on the other hand will allow every mode of evacuation; and the antimonial preparations, with other like remedies they make use of, convince us sufficiently of their putting in practice what they entertain in theory. But I am fully persuaded, that the hot fit depends on the
same

same cause as the cold; and that the same remedies do in reality remove them both. Without regard to this, however, does it appear any way probable, that the system should be so much changed in so short a course of time as takes place from the end of the cold till the beginning of the hot fit, that from labouring under extreme weakness, it should acquire such an invigorated state as to admit of evacuation? I am of opinion, there is not the smallest foundation for this idea; and in conducting our practice, we are not to be led away by any such view; for provided we pay sufficient attention to the symptoms that take place, we shall conclude with greater certainty, that this, as well as every other period of the disease, depends upon debility. It may then be asked me, what occasions the great difference between the cold and hot fit; as it is observed, that the pulse during the cold is quick and feeble, while in the hot stage it turns fuller and stronger. To this I before confessed my ignorance, and was under the necessity of taking the fact; but as a proof of what I then alleged, Dr Lind informs us

of his “ having twenty-five patients labour-
 “ ing under intermitting fevers; he pre-
 “ scribed an opiate for each of them, to be
 “ taken immediately after the hot fit, pro-
 “ vided the patient had then any inquietude,
 “ head-ach, or similar symptom usually
 “ subsequent to the fever. The conse-
 “ quence was, that nineteen in twenty-two
 “ received immediate relief; the other three
 “ had no occasion to take it. Encouraged
 “ by this surprising success, he next day
 “ ordered the opiate to be given during the
 “ hot fit. In eleven patients out of twelve,
 “ to whom it was thus administered, it re-
 “ moved the head-ach, abated the fever,
 “ and produced a profuse sweat; which
 “ was soon followed by a perfect intermis-
 “ sion *.” And I have frequently seen
 the same happy event from the exhibition of
 wine, hartshorn, and other stimulating me-
 dicines, in continued fever; the operation of
 which I conceive to be perfectly analogous
 to that of opium. It may not be foreign to
 my subject to insert what Dr Lind has
 found from experience to be the effect of
 opium

* Lind on Hot Climates, p. 342.

duced. In every disease affecting the human body, the causes I am of opinion operate either in increasing the tone of the system, or on the other hand in destroying it. The effect of these circumstances we are generally made sensible of by the pulse. In the case of typhus, an increased frequency of pulsations takes place, while the momentum at the same time is considerably impaired; and it is generally allowed, that the strength of the system depends infinitely more on the momentum than on the frequency of the pulse. So I would attempt the explanation of the effect of opium in diminishing the heat of the body with the profuse sweats that succeed, in the following manner. In febrile diseases depending upon debility, it is universally known, a considerable degree of heat takes place; and this heat of the body rises above that degree which the very ingenious Dr Alexander hath found requisite to induce sweat: and as therefore this effect can only, with any certainty, be attributed to the increased number of pulsations in the arterial system, and to the diminished momentum; hence it follows, that
those

those remedies which suppress the increased frequency and add to the momentum of the pulse, are the proper remedies to be employed. In the case of inflammation, where both the momentum and number of beatings are increased beyond the due degree, the cure consists in diminishing both the one and the other; and the remedies to be employed must be those which by their sedative effect are adapted to remove the inflammation existing. It is in this way then, that Dr Lind found opium, and Dr Lettsom bark, to be medicines very powerful in exciting sweat, when given in the hot fit of these fevers, by increasing the momentum and diminishing the frequency of the pulse. As therefore the heat of the body was above the degree requisite for sweating, at the time Dr Lind administered the dose of opium to his patients, owing to the increased pulsations of the vascular system; hence it follows, that when these are diminished, and momentum added to the circulation, then sweat makes its appearance. The other effects of opium, of shortening the fit, and of relieving the violent headach, together with procuring

sleep, admit of solution by its obviating the causes of the fever, when the body is so much restored to a healthy state, that these symptoms denoting great debility no longer continue.

Besides the prejudices which have prevailed to the giving stimulants during the hot fit, many physicians have even delayed the exhibiting of them till a perfect remission or intermission has taken place. That this delay is void of foundation, will appear sufficiently obvious from what has been already delivered; and the opinions and facts of some authors, who have seen the necessity of the immediate application of these remedies, will in a great measure put this beyond the possibility of a doubt. Dr Lettsom tells us, “ That after the evacuations he de-
 “ signs have been produced, he commences
 “ the exhibition of the bark, without wait-
 “ ing for remissions and intermissions; a
 “ sedulous attention to which, and to crises
 “ in fevers, he presumes has destroyed more
 “ than famine and Sydenham’s cold. A dry
 “ dark-coloured tongue, a dry skin, urine
 “ without sediment, desipientia, delirium,
 “ dysp-

“ dyspnœa, and continued fever, are the
“ very circumstances which have deterred
“ physicians from using the bark. In a
“ word, these are the very reasons for which
“ he would immediately give it; it pro-
“ motes a mild perspiration, produces a
“ sediment in the urine, and diminishes the
“ quickness of the pulse; it removes deli-
“ rium, by obviating the cause or causes
“ which produce the fever, and effectually
“ removes the breathing *.” And again:
“ Too often the cautious prescriber waits
“ for an intermission, and then ventures to
“ give the bark, when the patient has got
“ well without it; or while he is employed
“ in watching nature as it is called, he fre-
“ quently watches the patient to his grave †.”
Dr Clark likewise informs us, “ The cure of
“ intermittent and remittent fevers in all
“ their varieties by the Peruvian bark, is
“ simple, efficacious, and the practice uni-
“ versally established. In a former publi-
“ cation he has shown, that there is great
“ danger in waiting for remissions; and that
“ this

* Lettsom's Memoirs of the London dispensary.

† Ibid.

“ this noble febrifuge may be given with
 “ the greatest success, not only in the re-
 “ missions and exacerbations of fevers in hot
 “ climates, but even when they become
 “ continued.”—Dr Clark presumes, and
 this I believe with every reason, he has
 had sufficient experience of the good con-
 sequences arising from the like method of
 treatment in those fevers which usually
 happen in Britain *. In his work on the
 diseases of hot climates, he has given it as
 his opinion, that, “ as soon as the intestinal
 “ tube has been thoroughly cleansed, the
 “ principal part of the cure consists in pre-
 “ scribing the Peruvian bark in as large
 “ doses as the patient’s stomach will bear,
 “ without paying any regard to the febrile
 “ remissions and exacerbations. If the re-
 “ missions are distinct, the bark will have a
 “ more speedy effect; but even although the
 “ disease has continued, by its use it is
 “ effectually prevented from growing dan-
 “ gerous and malignant †.” In another
 part of the same book, he obviates an
 opinion

* Clark on Fevers. † Clark on Hot Climates, p. 145.

opinion which prevails against the use of the bark: “ When a fever has distinct
 “ remissions, few physicians will scruple to
 “ prescribe it; but if the disease assume a
 “ continued form, every method is tried to
 “ bring on regular intermissions: If this
 “ cannot be accomplished, and the patient’s
 “ strength begins to sink, alexipharmics,
 “ blisters, and cordials, are employed to
 “ support him. The use of the bark at that
 “ time would be thought highly dangerous;
 “ and has therefore been cautiously prohi-
 “ bited by almost every medical writer since
 “ the days of Sydenham. But in hot cli-
 “ mates experience affords sufficient proof
 “ that this objection has no manner of
 “ foundation, and that the bark may not
 “ only be given with the greatest safety,
 “ both in the remissions and exacerbations,
 “ but even when the disease is continual*.”

In the fourth volume of the Medical Observations we find also the following remark: “ That whereas most writers forbid
 “ the use of the bark absolutely, unless in
 “ the state of remission and intermission,

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* Clark on Hot Climates, p. 147.

“ I found it necessary to give it at any time
 “ of the disease indiscriminately; and success
 “ justified the practice, the patient other-
 “ wise dissolving away irretrievably under
 “ cold colliquative sweats †.”

Another objection to the use of stimulating remedies is, that they have often been considered as increasing the delirium in fevers; and dissections have led to the opinion, that in typhus, an actual inflammation had taken place, and given appearance to the fullness of the vessels in the brain. But it is to be observed, that this seeming inflammation does not depend upon the effect of increased excitement in the system, but, on the contrary, is entirely owing to a deficiency of it; and as a proof of this, we have in life a similar aspect. Ophthalmia, in its commencement, is a disease of a truly phlogistic nature; but I believe this state of actual inflammation does not long continue; and if debilitating powers are carried to excess, a period ensues, when the disease becomes passive, and, instead, of admitting of further evacuation, will be in fact cured by
 tonic

† Medical Observations, Vol. IV.

tonic and stimulating medicines. The redness of the vessels of the eyes in scrophulous people, is more especially a disease of debility; and instead of being cured by evacuants, the remedies found to answer are those of an invigorating nature. The inflammation observed, too, in the gout, I am much convinced, is not of an active nature; but depends on a state of debility, and that stimulating remedies here also are the cure. The scarlet appearance of the fauces remarked in cynanche maligna, affords a striking example of the inflammation I speak of; where it is universally known, that the practice is to support the patient sinking so rapidly under this dangerous complaint. Respecting delirium in fevers, I apprehend there can be little doubt of its being a symptom of debility, and only to be removed by stimulants*; and this opinion

* Dr Millar, in making his observations on the cases recorded in his work, comments upon delirium in the following manner. " This symptom hath often given
 " rise to a suspicion of a phrenitis, as it is called, or
 " an inflammation of the brain; a disease justly reck-
 " oned exceedingly dangerous. But the symptoms
 " ought

nion is confirmed in a letter from Dr Odier of Geneva to Dr Duncan, who not only corroborates what Dr Lind has advanced with regard to the effect of opium when given in
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“ ought to be very different from those which are ge-
 “ nerally ascribed to it. That of a delirium is observed
 “ in almost all remitting fevers, and is only dangerous
 “ when assailed by copious bleeding and the antiphlo-
 “ gistic method of cure. It continues, in maniacs and
 “ melancholics, for many years; and surely no one
 “ would in these cases suspect inflammation. It must
 “ be confessed, that physicians have been satisfied with
 “ very improbable conjectures concerning the diag-
 “ nostics of this dreadful disease, though their practice
 “ hath been as bold as if it were founded on the
 “ highest degree of certainty. But let us rise from
 “ conjectures to facts. What are the symptoms when
 “ an inflammation in the brain is occasioned by exter-
 “ nal violence? Insensibility, coma, and involuntary
 “ motions, are the principal signs; but a delirium is
 “ seldom occasioned by such a cause, and is never so
 “ violent as that which accompanies the remitting
 “ fever. Since then it often happens where there
 “ can be no inflammation of the brain, and an inflam-
 “ mation may exist there without producing such an
 “ effect, should we not be extremely cautious of ad-
 “ mitting conjectural evidence in opposition to estab-
 “ lished truths?” See *Prevailing Diseases of Great Bri-
 tain*, p. 121. et seq.

the hot fit, and which has been already mentioned; but likewise informs us, that he has given it with great success when there is much delirium, which he never found was increased by the exhibition of it, but on the contrary much oftener to be removed, and always to be diminished*.

There is one circumstance I neglected to have mentioned, and now remark it; it is this: I have repeatedly observed the practice of many physicians who employ the evacuating modes of cure in fever, some of whom constantly exhibit nauseating, nay even vomiting doses of emetic tartar; and this they employ indiscriminately, not only in the beginning of the disease, but at almost every period, merely because it is observed that the stomach is frequently disposed to throw out every thing it contains. The absurdity of such modes of treatment will appear evident from what I have already delivered. Nay, I will allege, that the symptoms of nausea and vomiting are rendered more distressing, instead of being mitigated

* Medical Comment. Vol. VI. p. 352.

gated by it *; and not unfrequently, from the use of emetic tartar in an advanced state of a fever, the inclination to vomit is so much increased, as to throw the patient into a deliquium, from which it is with difficulty he can be roused, unless some powerful stimulant is instantly had recourse to. And I shall recite a passage from Dr Clark's work on the diseases of hot climates to shew that these symptoms depend entirely on a loss of tone in the stomach, and not on the effect of causes requiring such evacuation. He mentions, " his having found the greatest
 " advantage from prescribing a full dose of
 " solid opium, when the stomach is weak
 " and squeamish after evacuation; it seldom
 " failed

* Dr Gibson relates the case of a gentleman who had a quotidian attended with such an intolerable pain at the stomach, that he was of his own accord led to apply a smoothing iron so hot to the pit of his stomach as to blister it. Surely this symptom indicated evacuation as well as the symptoms of nausea and vomiting; and yet I apprehend a single vomit administered might have killed him. Dr Gibson ordered him a considerable quantity of bark, a dose of which he took every hour, and by this remedy he was effectually cured. See Gibson on fevers, p. 140.

he concludes, we might not have had five men living in the island* .

C H A P T E R VIII.

CURE OF FEVER, WITH SOME OBSERVATIONS ON PUTREFACTION, &c.

AS I have rejected the several opinions which have prevailed concerning the knowledge of the proximate cause of fever, it necessarily follows, that the indications of cure as drawn from these hypotheses cannot be adopted. And having, it is presumed, rendered it sufficiently probable, debility is the immediate cause of fever, and that every symptom immediately depends upon the weakened state of the system, the only indication therefore I form, consists in the removing of this state. It may be, however, objected here, that in many cases of fever, we have only evidence of the application of one power, to wit, that of contagion, and which has been generally considered as operating

* See Appendix to Dr Brocklesby's Observations, p. 318.

rating upon the fluids in inducing a putrescency of them : hence it might be argued, another indication should be admitted of, that of obviating this putrescency. But I am of opinion, that contagion acts in no other way than as any common debilitating cause ; and that therefore the tendency which has been supposed to take place in the fluids to a putrescent state depends altogether on its effect as such. At the same time, I am much inclined to believe that no putrefaction does in fact take place in the circulating fluids during life. As this is a matter, however, of considerable importance, and leading to a different practice in many instances, I shall urge my reasons for rejecting the generally received opinion that our fluids, in particular diseases, are in a putrid state, which has followed the operation of certain matters acting as ferments in assimilating them to their own, and to us a hidden nature, before I proceed to lay down my directions in the employing remedies for the cure of fever.

The first argument that would make any impression on the mind of a person acquainted with the œconomy of the human

frame, is respecting those parts which, as immediately requisite for the support of life, are liable to be affected with disease. The most simple view of the human body exhibits a complication of various organs composing it, with the degree of sympathy between each of these and the whole, their dependence on one another, with the manner and cause of their action: it will therefore be an obvious conclusion, that where the general system is impaired by disease of whatever nature, those particular parts, which possess the greatest degree of sensibility, and the discharge of whose functions are indispensably necessary to the welfare of the body at large, will be subjected on this account to be most materially injured. To determine therefore where the seat of disease will be in general, it is requisite that we take a slight view of those parts which more immediately compose the body, and of the several purposes which they are designed to serve. These parts are divided into solid and fluid. The former, as made up of many small and innumerable fibres, constitute the basis of the body, and serve for the
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several motions of it. These motions again are of two kinds, called voluntary and involuntary. The latter is indispensable for the support of life, while the former is by no means so: but notwithstanding, however much they differ in this circumstance, yet the motions excited in them depend still upon the same principle; and it will follow, that the strength and contractility of these will be in direct proportion to the number of powers influencing this principle. There are from the beginning of our existence certain powers that constantly are acting upon us, and which are unavoidably connected with life; but as their action is by no means confined, so it may happen their sway on the system will be variously modified upon different occasions, and which will have the effect of producing disease. The causes inducing this state of disease in the fibres of the body will depend upon the diminution of the several powers influencing it; or again, upon the increase of these. The former effect, as implying a want of the natural and accustomed stimulus, admits of the fibre becoming lax, and of the

cohesion between its particles being destroyed, on which the vigour of it so directly depends. The other effect is the very reverse of this, where the tone of the solid is unduly increased beyond that degree which is compatible with its nature. As whatever state the solids of the body are in, whether they possess their proper vigour, or have this in an increased degree; or again, whether they are deprived of it in a great measure; it is still certain, that the motions performed by them are owing to the presence of a principle, whereby it happens, that in consequence of the application of stimuli, it enables the fibres to contract, and endeavour to free themselves of any power, which if suffered to remain might prove destructive to it, and which has been called the Vital Principle of Animals.

Having now endeavoured to establish that the vital principle of animals is inherent in the fibres of the body, and the injuries these may sustain; we shall now turn our attention to the consideration of the fluids of the body, as operating powerfully upon the solids to the due performance
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of their functions. Since the discovery of the circulation by Dr Harvey, the attention of physicians has been engrossed with the most absurd theories of the animal machine, attributing the cause of disease to some change produced in the fluids, and they have been led constantly to consider it as an hydraulic system. On this principle, it will not appear strange, that on many occasions the same remedies were prescribed to the cure of diseases of the most opposite cast, as they were intended to remove particular states of the blood on which the disease was imagined to be founded. Dr Boerhaave's theory of the Humoral Pathology prevailed very forcibly till within these few years, nay, many are still guided by his reasoning, while the constant object of their practice is to remove the supposed vitiated state of the fluids. But to Dr Hoffman we are first indebted for leading us into the investigation of that part of the system which is the subject of disease; and we are still in a more particular manner obliged to Dr Cullen, for prosecuting what Hoffman had before suggested, and rejecting the fanciful, tho'

absurd notion of the humoral pathology, when it was so lately and so fully in vogue. A survey of the formation of the blood from the aliment received into the system, together with its use, will at once convince us how much the due action of the solids of the body is necessary to the enjoyment of health; it will give us the most satisfactory explanation of those parts which are the seat of disease, and will at the same time afford us an opportunity to reject the humoral pathology in the case of those affections whose presence has been most universally considered as occupying the fluids of the body.

After the reception of the aliment into the stomach, (which at the same time must be in a state of health to admit of the sensation of hunger), the sentient parts, in consequence of the irritation applied, are stimulated to exercise their power upon the secreting system for the digestion of it. We see evidently then, how essential it is to the welfare of the body, that upon taking food, and for the digestion of it, the fibres should be in a state fit to discharge their functions. It is certain, in the
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case of dyspepsia, where a loss of tone prevails over the whole system, and particularly in the stomach, that though aliment be received into it, yet on account of its debilitated state, the food frequently remains there undigested for a considerable time; nay, there are numberless instances where it has been vomited up thirty-six or more hours, in nearly as crude a state as when it was first received into the stomach: And though it may have undergone some change towards digestion, yet it is undoubtedly certain the chyle is never so well prepared, as it would have otherwise been if the fibres of the stomach possessed their proper degree of activity. Besides, Dr Huxham, however much he was wedded to the humoral pathology, observes, that in such cases the nourishment is not at all, or not for a long time after, converted into blood.

After the process of digestion has advanced a considerable length in the stomach, the food then passes out through the pylorus to be mixed with the other liquors that are poured forth on the inner surface of the alimentary canal, which probably have still
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the further effect of promoting the solution of the food, in order to render it more fit to be received into the constitution*. And I apprehend it is for this purpose that such a quantity of fluids of various kinds are secreted, that the aliment may have undergone a proper change before its reception into the system; as it is evident, when it comes to be mixed with the mass of blood, if it contained any hurtful quality, it has then less chance of being evacuated. When the chyle has advanced thus far, it then comes to be circulated with the blood, and to be applied to the several purposes of life. These purposes are chiefly, to stimulate the solids to a due performance of their office; and to supply the constant abrasion going on in the system, by depositing that nutritious cementing matter which connects the fibres of the body together, giving them that degree of rigidity remarkable in those who are of a vigorous constitution.

After this short sketch of the component
parts

* The various methods of cookery tend perhaps to the same end.

parts of the body, and of the chief purposes they are meant to serve, it is astonishing that a physiologist of Dr Cullen's reputation should have conceived a putrid diathesis of the fluids to be the proximate cause of any of those diseases termed Putrid; and his retaining the humoral pathology in one or two instances, subjects him to be deprived of a part of the great honour and esteem which he had deservedly attained for the general rejection of it, as he at the same time is well aware that the phenomena of health and of sickness depend upon the state of the primary moving powers; and, though he finds fault very justly with Boerhaave and his disciples for the pathology of the fluids, he yet in the case of scurvy has laid himself open to be attacked on the very same ground.

In his preface, in speaking of Boerhaave, he says, "That if we consider the imperfection of his system, with respect to the state and various condition of the animal fluids; and if at the same time we reflect how frequently he and his followers have employed the supposition of an acrimony or
"lensor

“lensor of the fluids, as causes of disease,
 “and for directing the practice, we must,
 “as I apprehend, be satisfied, that his sys-
 “tem is not only deficient and incomplete,
 “but fallacious, and apt to mislead.” And
 again: “Whoever shall consider the almost
 “total neglect of the state of the moving
 “powers of the animal body, and the pre-
 “valence of an hypothetical humoral patho-
 “logy, so conspicuous in every part of the
 “Boerhaavian system, must be convinced
 “of its great defects, and perceive the ne-
 “cessity of attempting one more correct.”

All this has been applied by Dr Cullen to
 Boerhaave, which I think is applicable in
 every respect to himself; as the reasons
 assigned by him for retaining the humoral
 pathology as the proximate cause of scurvy,
 are not only vague, but totally void of those
 circumstances which ought to form the
 basis of a doctrine.

He has given it as his opinion, that the
 state of debility observed in scurvy is mere-
 ly the effect of the vitiated fluids; but how
 irreconcilable is this with what he delivers
 in his preface? “That the affections of the

“ mo-

“ motions and moving powers of the ani-
“ mal œconomy, must certainly be the lead-
“ ing inquiry in considering the diseases of
“ the human body. The inquiry may be
“ difficult; but it must be attempted, or the
“ subject must be deserted altogether.” I
therefore cannot but repeat, how strange it
is that Dr Cullen should be guilty of such
an absurdity, by retaining the humoral pa-
thology, when he everywhere condemns the
impropriety of that doctrine. Besides, can
it be rendered in any measure probable, that
the debilitated state of the solids, in which
the vital principle is inherent, is only a con-
sequence of the depraved fluids; which, as
we have already seen, operate only by nou-
rishing and giving the solids that degree of
energy necessary for their action, and repair-
ing the waste of the system? And, though
Dr Cullen might not have had many oppor-
tunities of seeing patients labouring under
the scurvy, yet had he consulted any of those
authors who had repeatedly observed it in
all its stages, as Dr Lind, he would have
been sensible, that the debility remarked on
the attack of scurvy, could not be assigned
with

with any degree of probability to the diseased fluids. For when this disease first seizes a person, it cannot then be supposed the fluids have undergone any considerable change, (admitting them to be the seat of the disease); every symptom at this period, however, plainly demonstrates that the fibres are and can be alone susceptible of the disease. The inability to perform the accustomed exercises, the sense of heaviness of the extremities, and upon an attempt to perform any motion, yet the feebleness of it, and the constant effort to lay hold of the most trifling objects to support the body, thoroughly evince the impaired state of the fibres. Dr Milman's opinion on this head appears to me perfectly just, and carries with it sufficient conviction that no putrescency does in reality exist, during life, in the fluids. " It is confessed by some persons, that the
" blood cannot indeed be expected to be in
" an actually putrid state, but that it is the
" strong disposition of the vital fluid to pu-
" tridity which constitutes the proximate
" cause of putrid diseases. This assertion
" seems to me to confound all propriety of
" lan-

“ language, and every rule of common sense.
 “ It is to say, that an inclination in the fluids
 “ to putridity, is the proximate cause of ac-
 “ tually putrid affections. The propensity
 “ to any particular state must surely be
 “ short of the state to which it tends, and
 “ incapable of giving that which it has not
 “ itself attained. The essential nature of a
 “ disease must be constituted by something
 “ absolute; nor is it to be sought for in
 “ any future possible or probable condition
 “ of the body. But, though an inclination
 “ to putridity cannot therefore be consider-
 “ ed as the proximate cause of a corruption
 “ already existing, let us, however, as a
 “ matter of curiosity, inquire, whether such
 “ a proneness to putridity really takes place
 “ in the case before us or not. If it should
 “ be said, that the blood of scorbutic people
 “ is not indeed in a putrid state, but in a
 “ condition nearly approaching it, what is
 “ the answer that might be returned? On
 “ the authority of experiments, Dr Lind
 “ assures us, that the blood of such persons,
 “ kept in the same state of the air, did not
 “ corrupt sooner than that of healthy people.

“ Thin slices of mutton steeped in its serum
“ continued sweet, and free from taint, as
“ long as in the serum of blood drawn from
“ persons in health. Whether I consider,
“ therefore, the nature of the causes pro-
“ ducing the scurvy, or the actual state of
“ the blood in the disease, or the secretions
“ from it in the course of the complaint,
“ they all concur to make me believe, that
“ this disease does not consist in a pu-
“ tridity of the blood. Nor does there ap-
“ pear to be any quality, which can be dis-
“ cerned and defined in the blood of scor-
“ butic persons, by which the particular na-
“ ture of their disease can be characterised*.”

And, even admitting that a putrid matter may be absorbed into the system, it is notwithstanding sufficiently evident, that those parts which alone are endowed with feeling, can only be made sensible of its effect; and to advance that the blood is possessed of this property, would lead us into the grossest absurdities: and at the same time, if the idea was universal, the event might turn out to be the again introducing the once prevailing and universal humoral pathology.

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* Vide Milman's Inquiry, p. 57.

The doctrine of assimilation has, however, been had recourse to in this case, that when putrid matters are present in the fluids, a fermentative process takes place, changing them to their own natures. But this idea is very improbable, and I may add equally impossible, that in the constant round of circulation such a process could be carried on, when undoubtedly rest favours it. Dr Ferris, in a dissertation published here last September, entitled *DISSERTATIO PATHOLOGICA, INAUGURALIS, DE SANGUINIS PER CORPUS VIVUM CIRCULANTIS PUTREDINE*, has given it as his opinion, that rest is not essential to every fermentation, but does not immediately determine whether the change our aliment undergoes depends on this process *. I shall not dwell on this point, whether digestion happens in consequence of a fermentative process, as not being fully determined on, and admitting of doubt: but at the same time I would remark, that digestion goes on while we remain in the most tranquil state; and it is well known in the

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† *Dissertat. Inaug. p. 12. and note.*

fermentation of wines, that they rest perfectly undisturbed: Nay, a person entering a part where a considerable fermentation is going on, would undoubtedly be killed, if not very soon removed. Besides, in Portugal and in France, where great quantities of wine are made, provided that motion forwarded the fermentation, they would certainly hurry on the process by such means. But to strictly confine ourselves to the human body; though a putrid matter was present in the fluids, yet Dr Milman advances with the greatest propriety, that, “In
 “ diseases arising from any specific matter,
 “ tho’ the noxious particles may be absorb-
 “ ed, and, floating in the blood, by stimu-
 “ lating or weakening may produce effects
 “ on the vital power suitable to their pro-
 “ perties, the mixture of the vital fluid is
 “ not much affected. Its sensible qualities
 “ are not changed *.” This reasoning of
 Dr Milman’s, however, is opposed by Dr Ferris in his Inaugural Dissertation. He tells us, “ *Hæc forma loquendi tamen est omnino*
 “ *inepta; estque cur miremur Milmannum,*
 “ *chemiæ*

* Milman’s Enquiry, p. 132.

“ chemiæ ac aliarum scientiarum peritum,
 “ colorem et qualitates evidentes sanguinis
 “ immutatas, pro argumento contra actio-
 “ nem in sanguinem contagionis proposuisse.
 “ Contagio extra corpus adeo est tenuis et
 “ subtilis, uti oculum, imò imaginationem
 “ ipsam, effugiat; quapropter vix credibile
 “ videbitur, eam sanguini admotam, hujus
 “ vel colorem, vel qualitates reliquas evi-
 “ dentes, esse mutaturam. Namque rubia
 “ tinctorum, quam omnibus sensibus subji-
 “ cere possumus, animali sumpta donec ossa
 “ ipsa rubescant, sanguinem ne quidem
 “ omnino mutat, etsi ejus particulæ per eum
 “ ossa ingrediantur oportet. Asparagus
 “ sumptus, ut et terebinthina corporis su-
 “ perficie admotæ ac absorptæ, proprium
 “ odorem tum urinæ cum perspirationi,
 “ impertit; aurumque in oculis ægroti, qui
 “ mercurium devorat, eodem modo, etsi
 “ lentius, quam sub mercurio submersum
 “ afficitur. Interea tamen qualitates san-
 “ guinis, et evidentes et chemicæ, immutatæ
 “ manent, nec ullæ particulæ asparagi, tere-
 “ binthinæ, vel mercurii ibi deprehendi

“possunt *.” It appears not a little surprising a gentleman versed in the knowledge of the animal œconomy, should have paid so little attention in investigating any pathological subject, as to conceive, what evidently appears from the above quotation, that the fluids are in a particular manner affected with impressions from matters taken into the constitution, when he must be sensible that the phenomena, in every condition of the body, are to be explained only from the state of the primary moving powers. I will not deny, that the fluids are to be considered as the most essential stimulus which operates upon the living principle for the discharge of its functions; and also, from the nature of our œconomy, the fluids may receive the tincture of substances absorbed. But at the same time I am equally disposed to deny that the sensible qualities of the blood can undergo such a change as has been imagined to happen in putrid diseases, and much less that this effect follows in consequence of a putrefactive process. The subtilty of contagion is undoubtedly of such a nature
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* Dissertat. Inaug. p. 14.

as not to come within our cognifance ; but why from this infer our fluids are therefore rendered putrid from its action on the fyftem? If it were poffible that the blood, which is totally deftitute of fenfibility, could experience any morbid change, yet thofe parts which poffeffed this in a more eminent degree, and likewise being the firft that receive the impreffion, muft moft affuredly undergo the greateft and moft efferential deviation from that ftate connected with health. Without, however, infifting further on this fubject by reasoning ; if we review the confequences of expofure to contagion, we fhall be perfuaded, that the fibres of the body are the parts which fuffer from the application of it. The fhivering that is felt over the whole body, the ficknefs, anorexia and vomiting, the pain of the head, the wearinefs and inaptitude to exercife in a proper degree the neceffary functions, with the quick weak pulse, &c. all tend demonftratively to verify the juftnefs of my opinion. And to confirm this the more, we may obferve, there are not wanting cafes, where people having been fuddenly expofed

to a contagious principle, have had the powers of life suspended for some time. And what may be deemed an *argumentum crucis*, Dr Lettsom informs us of his having attended several women who laboured under the jail or malignant fever, that gave suck, and continued to do so through the fever, which he justly considers as a proof how little the fluids are primarily affected, as the children escaped with impunity*.

Another argument I would adduce is, we have no very satisfactory accounts of actually putrid blood having been drawn from the vessels during life; and does it not on the contrary appear, that such a state would be altogether incompatible with our existence for the shortest time? Would a putrid gore stimulate the vital principle in a proper manner for the continuance of life; or could it be said, that the blood is then in a fit condition to replenish the waste of the system, or in short adapted for any one purpose it serves in a healthy state? Moreover,

* Lettsom's Memoirs of the London Dispensary, p. 26.

over, we are informed by Dr Lind, that he let blood from an hundred patients when in the scurvy; that the serous or watery part perfectly separated from the red concreted mass; and having examined it, he observes, the serum was as tasteless as the white of an egg; and upon the whole, blood drawn from persons in the scurvy, impresses both the taste and smell with the least perceptible sensation of any blood he ever put to the trial *. And those authors who mention some morbid appearances of the blood † as it flowed from the veins, yet they allow the coagulation of it; but it is very extraordinary, if a putrescency of the fluids consti-

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* Lind's Postscript on the Scurvy.

† Though Dr Rouppe tells us of some morbid appearances of the blood, yet he says in another part of his work, "That tho' our fluids tend very much to putrefaction, yet when extravasated they are not remarkably putrid, though they stagnate in the body many days; and he is surpris'd if there be so great a corruption as often appears from the time they lodge in a part, that by their mixing with them they do not change the whole mass of fluids into a putrid coagulation; and yet the contrary is seen every day." See English edition of Diseases of Seamen, p. 317. and 318.

tutes the proximate cause of what are termed putrid diseases, that no such dissolved state should appear in the hundred cases mentioned by Dr Lind. Neither at the same time would the separation of its parts be complete, if these diseases consisted in a putrid diathesis of the blood: For the very ideas we would form of putrescency and of coagulation, are in themselves as directly opposite as we can possibly conceive. The former implies a total dissolution of its parts; while the latter comprehends those phenomena that attend the separation in a healthy state, and which are altogether unconnected with a putrid disposition of the blood.

Having now endeavoured to prove, that the fluids of the body do not in putrid diseases undergo the putrescency they have been generally imagined; and when of course the debility observed cannot therefore be attributed to this cause, as assigned by Dr Cullen; I proceed to take notice of another circumstance mentioned by him, and which I cannot pass over. He has said, that
“ there is hardly an instance of scurvy ap-
“ pearing

“ pearing unless where salted meats had
 “ been employed, and scarcely where the
 “ long continued use of these did not pro-
 “ duce it.” I will venture to affirm Dr
 Cullen is egregiously mistaken in both these
 particulars. If he had examined Dr Lind’s
 excellent work on the scurvy, he would have
 been sensible of his adducing several instan-
 ces, where sailors have eat for a considerable
 time of salted meats, and notwithstanding
 this have escaped the disease. He assures us
 of his having been three months on a
 cruise, during which time none of the sea-
 men tasted vegetables or greens of any sort;
 and even for a great part of that time, on
 account of a scarcity of fresh water, their
 salted provisions, which consist of beef and
 pork, were boiled in the sea-water; they yet
 returned into port without a single person
 being affected with the scurvy*; and he
 has a note from Kramer, who affirms,
 “ That salted meats have sometimes no share
 “ in occasioning the scurvy; which is demon-
 “ strable from the many Germans in Hun-
 “ gary destroyed by it, who eat neither beef
 “ nor

* Lind on the Scurvy, p. 51.

“ not pork; on the contrary, they have fresh
 “ beef at a very low price *.”

The writer of Lord Anson's voyage remarks, it is generally imagined that plenty of fresh provisions and good water are infallible preventatives of the scurvy: at one period, however, the men were supplied with both these, and almost daily caught such a quantity of fish, that the whole crew frequently lived on them; yet those, who were ill, were not relieved, or the disease in any way prevented from following its career †. And when Lord Anson cruised in the Pacific ocean for the space of four months in expectation of falling in with the Acapulco ship, the men continued in perfect health; when at another period they were over-run with the scurvy in less than seven weeks after having left the coast of Mexico, notwithstanding of plenty of fresh provisions and good water ‡.

While the scurvy has raged without the

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* Lind on the Scurvy, p. 49.

† See Anson's voyage in Dr Lind's book on the Scurvy, p. 446.

‡ Lind on the Scurvy, p. 62.

persons so afflicted having eat of salted meats, is evident; it is also certain, that the disease has appeared even among the daily use of vegetables, but where they have laboured under the effects of the passions, or have been otherwise disagreeably situated. Dr Lind has remarked, that two of his Majesty's ships, the *Jason* and *Deptford*, were employed for a considerable time in convoying the merchant vessels round from Plymouth to the Downs, and calling at Portsmouth in their way. They were rarely longer than forty-eight hours in their passage, and eat no oftener than once or twice in a fortnight of salted meats for the space of twelve months; and though they were constantly furnished with supplies of fresh beef and greens, yet the companies of both ships were much distressed with the scurvy*. And he further mentions, that the scurvy made its appearance in the *Salisbury* and in the channel fleet, when they had been only six weeks at sea after having left Portsmouth, where plenty of all sorts of greens had been obtained. And what renders

* Lind on the Scurvy, p. 273.

ders it unquestionable that the disease was not occasioned solely from the want of vegetables for so short a time is, that the same ship's company of the Salisbury, when on much longer cruises at sea, have been altogether exempted from the symptoms of this disease, where as to fresh vegetables they were in every respect similarly circumstanced; and it had been noticed that in the longest cruise that vessel made, there was only one person on board who was attacked with the scurvy after having had an intermitting fever †. From which several instances I am warranted to conclude, that Dr Cullen is much mistaken when he asserts, there are hardly any cases of scurvy occurring unless where salted meats had been employed; and again, where the long continued use of these did not occasion the disease: And as the principles he endeavours to inculcate are therefore found to be void of foundation, it necessarily follows, that his reasoning built on them cannot be admitted.

I have known it urged as a proof of the
putrid

† Lind on the Scurvy, p. 51. and 52.

putrid state of the fluids taking place, that the several symptoms, which constitute the advanced state of the scurvy, are of such a nature as to leave little doubt in pronouncing from what cause they proceeded. The symptoms chiefly dwelt on were, the effusions of blood under the skin, with its appearance; the fetid breath, and hæmorrhagies from various parts of the body. But I am confident, that the explanation of these phenomena from a lax and debilitated state of the solids, will give us more satisfaction than our having recourse to the fluids as inducing them. The appearance of the effused blood was deemed a most convincing proof of its putrid state: But it is to be observed, that the blood collected there is out of the course of circulation; and when this happens, it instantly becomes liable to undergo the putrefactive process; and hence therefore any particular look the blood puts on in such cases is by no means to be brought as a conclusion of the affection of the general mass. Besides, independent of the cohesion between the particles of the fibres being impaired, how would it happen, that

that they not only admit of the effusion of blood under the skin, but also of the most profuse and dangerous hæmorrhagies? For do not we find, that children, whose solids are lax, and which do not sufficiently resist the pressure of the fluids, are on this account more liable to be attacked with hæmorrhagies than adults? The epistaxis affords a proof of this. And again, are not those adults of the scrophulous constitution more subject to hæmoptysis and phthisis pulmonalis, than others, where, instead of a laxity or debility of the solids, a firmness and rigidity prevail?

SINCE, therefore, it has been rendered probable that the fluids of the body are not in a putrid colliquation when the scurvy is present, and when of course the debility observed cannot be assigned as the effect of a cause which does not in fact exist, I would remark, this state of extreme weakness constantly manifests itself, and this even from the very first attack; whence we should naturally be led to conclude, that so prevailing a mark of disease would most likely happen
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in consequence of debilitating powers. Besides, independent of weakness, how would it happen that symptoms generally called Putrid should occur in the close of a typhus; and more especially too, as we have many instances of this disease appearing, without the persons so affected being exposed to contagion, and when no alteration can have happened in the fluids from the application of it? In the advanced state of typhus, the same putrid symptoms present as in the scurvy, viz. the fetid breath, spots of various appearances, and hæmorrhagies, with the same gangrenous disposition of the body, particularly in those parts where blisters have been applied; and yet Dr Cullen does not take into consideration, in treating of fever, that such a putrid disposition of the blood bears a part of the proximate cause of that disease. Spasm and atony are the powers which, according to him, induce fever: At the same time, however, in reviewing the opinions which have been received on its proximate cause, he observes, that though the cause of fever frequently operates upon the fluids of the body, and
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particularly occasions a putrescency of them, he notwithstanding maintains, that such a state of the fluids is not commonly the cause of fever, and that very often it is an effect only; while in the scurvy, he not only makes the putrid diathesis of the blood to be the proximate cause of it, but likewise conceives that the debility observed is the effect of this depravity. If, therefore, a putrid condition of the vital fluid was the immediate cause of the scurvy, where the same symptoms occur in other diseases, the same dissolution should be assigned as constituting their very natures; but in some malignant fevers, the failure of the *VIS VITÆ* is so amazingly quick, as to contradict any notion of their depending upon a putrid diathesis of the blood, though they be attended with the common marks of putrescence. And that the several putrefactive symptoms depend upon debility, we may observe those persons who are most liable to be attacked with the scurvy, and other diseases of debility. They are, in general, those who have suffered by former sickness, who lead a sedentary life, who are enervated

who, when the men belonging to the *Gorcum* were mustered at *Helvoetsluys*, begged to be dismissed the service. He then had the scurvy; and upon his being assured he would be well long before he went aboard, and that he had sufficient strength of constitution to remain long in his present situation, and that the officers would not permit him to be dismissed, he expressed his extreme concern, and said, he would enter, but was certain he would soon die: and as he prognosticated, so it proved; for he died the very same day.

As it is certain, then, the depressing passions operate very powerfully in inducing the disease, and of hurrying it on when present, it is likewise evident that keeping up the spirits of those naturally depressed has been attended with the best consequences, as happened in the famous siege of *Breda*, taken notice of by *Dr Milman*.—These are, therefore, incontestable evidences of the scurvy being induced, and its having been got the better of, by the effect of the passions; and yet it can hardly be presumed in these as well as in many other instances
which

which could be enumerated, that the passions operated upon the fluids in inducing a putrescency of them, and again in overcoming that. Surely such an idea is vague in the extreme; for in other diseases, the sudden change which has been observed to happen in consequence of the effect of the passions where putrefaction was altogether out of the question, is a sufficient proof that no tendency to such a state in any measure took place; and I am of opinion it only admits of solution, by supposing that the dejection of mind in some manner diminishes the excitement of the system, as it is undoubtedly certain, that those persons who are endowed with a serenity and cheerfulness of mind, are more capable of resisting disease, than others in whom a languor and inactivity prevail.

Dr Lind is of opinion, that whatever tends to impair the constitution, and especially the digestive organs, may operate sufficiently, without any other cause, to introduce the scurvy, in a mild or in a malignant degree, even among such as eat of fresh greens, and who live on the most whole-

some diet, and in the purest air*. And he informs us, cases occur, where five or six hundred men living on salted and on hard meats in a long voyage, are very frequently exempted from the slightest attack of the scurvy; while, upon coming into harbour, and eating of ripe fruits and fresh vegetables, they have been attacked with it in an inveterate manner. Many hundred sailors, thus circumstanced, have been received into Haslar hospital, who have enjoyed good health at sea; yet they on their arrival have been seized with the first symptoms of this disease, after partaking of the wholesome food of beef and greens; and notwithstanding their being released from the confined situation of a ship, and the continuance of of a very nourishing diet, aided by the administration of the most approved remedies, together with the greatest care that could be taken of them, the scurvy has continued in several of them for five or six weeks †. This effect therefore can chiefly, nay perhaps only, be assigned to the irregular way
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* Lind on the Scurvy, p. 516.

† Ibid. p. 540.

in which sailors proceed after having arrived from any distant part of the world; and I can easily conceive, that in the longest cruises, they may be exempted from the slightest symptoms of the scurvy, and even those too who labour under any of the predisposing causes; but immediately upon their arrival, that they shall be attacked with the most inveterate scurvy, though their diet be changed infinitely for the better, owing to the many and various excesses they are in general guilty of whenever an opportunity offers. And in confirmation of Dr Lind's opinion, Dr Milman has observed, that during a scarcity of corn, the indigent inhabitants of the colder districts of Italy near the Alps suffered considerably from the the scurvy. Their subsistence was frequently on the decoctions of a few roots, and often for a whole day they never tasted any food*; which I apprehend sufficiently controverts any opinion that has or may be formed of the changes the blood can undergo, and of the effect those medicines named antiseptics have upon the constitution in

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* Milman's Inquiry, p. 24.

overcoming a preternaturally saline or other condition of that fluid. For that these antiseptics have no very great influence in the cure of the scurvy, Dr Lind has informed us, that those patients to whom he gave salads of garden-stuffs with ripe fruits, &c. did not recover sooner than those who did not eat them; and it is a fact well known, and mentioned by Dr Milman, that the Turks, though they use considerable quantities of the vegetable acids in their sherbet, yet no people are oftener visited with the plague, or suffer more from it than they. I would not altogether, however, assert, that the administration of the juices of fruits, &c. in the scurvy and other putrid diseases never was serviceable: Yet I am well aware that the commendation, these medicines have received, is far beyond that which is their tribute; for are there many, or I may say any instances, where putrid diseases have been entirely got the better of by solely persisting in the use of these remedies? I am of opinion, not: yet practitioners generally commence the cure of these affections by their antiseptics, among which the juices
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of fruits are seldom omitted ; but, on account of bad symptoms supervening, they are under the necessity of having recourse to the Peruvian bark, wine, &c. By these means a cure is generally obtained ; yet so fond are they of the antiseptic doctrine, they conclude the happy effect is owing to the medicines being antiseptic. Was this however the case, every power according to this quality should be equally serviceable ; but in many instances we know the very reverse to be the fact. As this matter may receive some light by Dr Milman's inferences on those means employed by Dr Lind to try what effect they had in the cure of the scurvy, I shall insert what he delivers, in his own words. “ All those means of cure
 “ which have been enumerated may be re-
 “ duced to the following heads: To the
 “ nutrientia, sudorifica, stimulantia, and the
 “ tonica. The first of these act upon the
 “ simple solid. By repairing the necessary
 “ and daily waste of the machine, which
 “ could not be replaced by an indigestible
 “ diet or insufficient food, nourishment re-
 “ stores that state to the fibre, which is re-

“quisite to the due action of the vital power.
“By giving that degree of plethora which is
“necessary to promote the various secretions
“and excretions of the body, it prevents that
“principle from being weakened by the re-
“tention of impure matters, the evacuation
“of which leaves it in a condition capable of
“properly exercising its functions. This is
“the manner in which I conceive the fresh
“flesh-broths of the Haflar hospital and the
“boiled beef and greens, have rendered the
“benefit which has been observed to follow
“sometimes their separate, and at others their
“joint use. The acid juices of fruits, of
“the lemons and the oranges, are great pro-
“moters of perspiration and of urine. Their
“virtues in the cure of this disease were
“improved by dilution with water-gruel,
“which cannot be well conceived to operate
“otherwise than by giving that fullness to
“the vessels which might render the effect
“of the acids, as sudorifics and diuretics,
“more certain. But when the disease was
“a good deal advanced, and great weakness
“had taken place, a pint of rich Malaga
“wine, joined with four ounces of these
“acid

“ acid juices, by its stimulant cordial pro-
“ perties added great efficacy to these; which
“ was still further increased by the addition
“ of a quantity of sugar, so as to occasion a
“ sort of effervescence at the time of taking
“ this mixture. That the wine, and the
“ active spirit discharged by this effervef-
“ cence, are useful, by their cordial and sti-
“ mulant qualities, and by concurring with
“ the acids in gently promoting the proper
“ excretions of the skin and the kidneys,
“ seems evident from the happy presage
“ always drawn in this disease, when the
“ skin, from having been dry, becomes soft
“ and moist. Dr Lind moreover inform us,
“ that the effects of this cordial mixture ac-
“ tually were sudorific and diuretic. This
“ view of the subject affords us some means
“ of accounting for the beneficial effects
“ ascribed to the mineral waters in the scur-
“ vy. In consequence of the considerable
“ quantity of fixed air they contain, they
“ are stimulant and diuretic *.”

Lastly, to conclude my observations on
this head, I would advance, that the reme-
dies

* Milman's Inquiry, p. 197.

dies and means used to resist putrid diseases are those that tend to invigorate the body, such as a nourishing diet, the moderate use of wine, proper exercise, with the article of cleanliness. Dr Lind observes, that when wine was more freely drank in Holland, the scurvy was less frequent; and among the first cures recommended to the world, was wormwood infused in wine; and this was afterwards used in Saxony as a preventive, where this disease occurred very often*.

For any further arguments and facts in support of what I have now delivered, I must refer the reader to Drs Lind and Milman's excellent works on this subject, as they supersede any thing else I can possibly offer. Indeed, some circumstances taken notice of have been before remarked by those Gentlemen; but as these were so closely connected with the subject, the mention of them was perfectly unavoidable. My chief object was to point out some prevailing mistakes of the scurvy hardly ever appearing unless where salted meats had been employed,

* Lind on the Scurvy, p. 177.

ed, and where the long-continued use of them did not produce the disease: And having, it is presumed, shown the futility of these notions in an evident light, it follows, no particular disposition of the fluids could have taken place, and especially in that case where no salted provisions had been made use of; which at once must overthrow Dr Cullen's idea of the disease. I have only further to observe, that the causes inducing scurvy and other putrid diseases *, operate upon the solids in weakening the vital principle. Every circumstance corroborates this idea; it gives the most satisfactory explanation of the symptoms; and as the degree of debility increases, so the different phenomena characterising the different stages of the disease ensue; and, to use Dr Milman's expression

* I apprehend the only essential difference between the scurvy and putrid fevers, is this, that in the former, disease the vital power is gradually impaired; while in the latter, the causes, from their more debilitating effect, hasten the patient sooner to his grave; and the attack of the disease, after exposure to them, in many cases is so immediate, as to refute any idea of the fluids suffering any change.

pression, the very essence of scurvy is weakness.

I now proceed to offer some directions in the employing remedies for the cure of fever; and as my only indication consists in obviating the debilitated state of the system, the remedies to fulfil this purpose are those known by the name of Tonics and Stimulants: under the former of which denominations come the bark, the Columbo and Virginian snake-roots, &c.; under the latter, wine, opium, volatile alkali, musk, and camphire. There are many remedies of the above classes that have been used in fevers with advantage; but as their effect is generally the same, differing only in their degree of tonic and stimulant power, my remarks are therefore confined to the Peruvian bark, wine, opium, and musk. But before proceeding to make particular mention of these remedies, I shall inquire into the propriety of administering vomits* in the
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* Dr Lind has found a vomit to be attended with the best effects on the attack of fever when the patients had
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commencement of fever. It appears to me, that on the attack of fever, when for the most part a considerable quantity of bile is accumulated in the stomach; and perhaps at the same time a quantity of aliment remaining there in its crude state on account of the loss of tone in that viscus, that vomiting may be serviceable in removing these; and I have no doubt of its proving useful by the agitation occasioned in the system, which rouses the patient from that state of languor marking the attack of fever; and by removing the viscid matter in the stomach, the stimulating remedies to be afterwards exhibited may have greater effect by coming more intimately in contact with the sentient parts. On the contrary, it may be urged, that where an accumulation of matter takes place in the stomach, as for instance bile, that this is only a symptom, depending upon debility; therefore, without any previous vomiting, our attention should

had been exposed to contagion; and soon after its application, they have had a head-ach, with rigors, sickness, &c. which symptoms have been entirely removed by the vomit.

should be directed to remove the debilitated state of the system, without paying any regard to the aliment, bile, &c. that may be present in the stomach. That there is good ground for entertaining the idea, I readily admit; yet I have seen repeated instances, where, on account of the extreme debility prevalent in the stomach, the matter lodged proved a disagreeable irritation to it, inasmuch, that a nausea and the disposition to vomit constantly exist: therefore I am of opinion, a gentle emetic, as it evacuates this offending matter, and at the same time as the act of vomiting proves stimulant to the whole system, it may be proper to exhibit it; and that, if immediately after its operation some stimulating medicine is had recourse to, the patient, instead of being more weakened, will in reality be refreshed and invigorated by its administration †. But

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† I am only to be understood that vomiting is proper on the attack of a fever, and that it is never to be employed in the advanced stages of the disease. The collection of bile, particularly in the warm climates, is so quick, that the stomach is constantly disposed to throw it out, and our utmost endeavours prove sometimes ineffectual

a second vomit is perfectly inadmissible: And when a person is attacked with a violent vomiting in the beginning of a fever, no vomit is in this case to be given, as this generally depends on an exceedingly debilitated state of the stomach and of the whole system; while the whole stress of the cure is to be trusted to the tonic and stimulant remedies; and with regard to them, it is to be observed, the doses should be small and frequently repeated, that the virtue of the former dose may not be gone off before another is exhibited. This was Dr Sydenham's method of proceeding; and I am well convinced it is indispensably necessary; for if you give large doses of any stimulating medicine, particularly of a powerful one, and only repeat it at distinct intervals, you do more harm than good, by exciting the patient considerably, and afterwards suffering him to sink into a state probably

effectual to stop the violent retchings. I have been lately assured by an able physician, that blisters applied to the pit of the stomach have frequently been the means of checking this, and then our medicines will be retained; after which period we have it in our power to correct that state of the system, on which the accumulation of the bile depends.

bably lower than what you found him. And further, I would remark, that the tonics are preferable in general to the more powerful stimulants; their effect is much more permanent; and on this account there is little danger of the patient suffering any great degree of collapse from the previous excitement: but when the disease is considerably advanced, and death supervening, in this case the stimulants, and those possessing this power in a more remarkable degree, should be made choice of before others not so highly stimulant. At the same time, the most assiduous attention is necessary in the doses to be administered, so as to prevent the tendency to death by the debility which succeeds the excitement. If this is strictly adhered to, there are few fevers which may not be got the better of; and I have often seen and heard of many cases where the patients have been in very unfavourable circumstances: yet by persisting in the stimulant plan, paying at the same time proper regard to the directions here laid down, they have speedily recovered; and by continuing in this method, but diminishing the dose

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as it was found requisite, any relapse was most effectually prevented. I come now to make mention of the observations I proposed concerning the several remedies to be employed in the cure of fever; and first, of the

BARK. This excellent medicine has long been used with considerable success in the cure of many diseases. We are chiefly indebted to Dr Sydenham for our knowledge of its efficacy in the cure of intermittents. This intelligent physician informs us, it had nearly been altogether out of use from the improper method of giving it. The mode it was then given in, which with many still prevails, was just before the accession of the fit. In this case, it seldom failed in stopping, or at least mitigating, the paroxysm; yet it commonly returned in the course of a fortnight; and on this account it fell into disrepute, until Dr Sydenham, sensible of its virtues, was led not to condemn a medicine merely because it was believed to be prejudicial from a few instances having occurred where the patients died after having taken it.

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He therefore made further trial of it, administering it in a more proper manner by giving it in the intermediate days; and the opinion that he had formed of the bark was rendered perfectly just from the success which attended its exhibition. And here I cannot see any just ground why the bark should not constantly be given in the intermediate days as well as in the day the fit happens. The disease consists in a state of weakness; and the sooner, surely, that this is removed, the patient must suffer less than where it continues for a longer period: we ought therefore to persist in its use for some time before recourse is had to other remedies more highly invigorating.

With regard to the dose, I perfectly agree with Dr Cullen, who is of opinion that bark is only to be depended on when given in large doses. From the ignorance of many in this, it has often been given, but failed of success; when, if the proper quantity had been taken, its operation would probably have produced the desired effect. Dr Lettsom very justly condemns a practice no less common than destructive

to the patient, of giving two spoonfuls three times a-day of a weak decoction; he assures us, the least that can be depended on in the cure of a fever with urgent symptoms of putrefaction is two ounces a-day. This is a quantity, however, that few patients can be brought to take; and though they were disposed to take every dose, yet in many cases it induces a nausea, and in others the stomach totally rejects it. This is the less to be regretted, as Dr Saunders plainly demonstrates, that the red Peruvian bark possesses more powerful effects than the common bark now in use: “ He seldom found it necessary to give more of the red bark than half a drachm every two hours during the interval of the fit; and that six drachms between the paroxysms was the utmost quantity he ever found requisite to be taken; but where the stomach can retain it, a drachm may be given every hour*.” This I think, of the red bark, is a very sufficient dose; and with respect to the most agreeable way of administering it, it is best in the form of

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mix-

* Saunders on the Bark.

mixture, with the taste of it disguised by means of extract of liquorice. There is hardly any one formula, however, that will answer in every case: some persons can take a quantity of the powder in the mouth, and wash it over by means of a fluid; while others reject the bark decoction or infusion, and even the tincture proves disagreeable. The swallowing of the bark in a wafer is a very good way to those who are capable of taking it in this manner.

Having said thus much on the dose of the bark, and on the most proper as well as most agreeable form of administering it, I proceed to obviate an objection that some physicians entertain against its use, I mean the notion of its giving rise to abdominal obstructions, jaundice, and dropsy; or that these obstructions, when already existing, should be cured by evacuants, instead of persisting in the use of the bark and other stimulants. Dr Cullen, in the cure of intermittents, lays down two very judicious indications. The first is, in the time of intermission to prevent the recurrence of paroxysms: The other, in the time of the

the paroxysms to conduct these so as to obtain a final solution of the disease. These are indications which are undoubtedly worthy of our attention; but why should he at the same time oppose himself by a third, which defeats his intention in the fulfilling the two former? It is this, the taking off certain circumstances which might prevent the fulfilling of the two first indications. These circumstances are, a phlogistic diathesis prevailing in the system, and congestions fixed in the abdominal viscera. The first, we are told, must be removed by blood-letting; the second, by vomiting and purging. But Dr Cullen surely does not conceive, that when a person has been considerably exhausted by disease, and this disease too that primarily depended upon the application of debilitating powers, there should be present at the same time a phlogistic diathesis. I would alledge there was even no phlogistic diathesis before the person was exposed to the debilitating causes; for if the phlogistic diathesis was in a proportionable degree to counterbalance that of the sedative power applied, no dis-

ease would have taken place. But we find that the disease does actually happen; and when of course a state of debility is induced, which no doubt during the disease is considerably augmented, how is it then probable there should exist a phlogistic diathesis? Besides, I would observe, there is a strange inconsistency in the indications which Dr Cullen has laid down respecting one another. The remedies which he employs to fulfil his two first indications are those, which, though no phlogistic diathesis was present, yet are such as would tend to induce that state in the system; and of course he is attempting to produce a state of the body on the one hand, which immediately after he intends to obviate; for the phlogistic diathesis, and abdominal obstructions, are the causes which prevent his exhibiting opium.

Respecting the abdominal obstructions, or ague-cakes, as some term them, I would apply the same argument here as I made use of in speaking of the phlogistic diathesis, that, when the patient is reduced to extreme weakness, these obstructions should be of
such

such a nature as to indicate evacuation, or that the tonic plan should aggravate the complaint when already existing. I am, however, very confident, that though this disease does depend in a great measure on the weakened state of the system, yet, on account of the internal local affection, our endeavours to subdue it altogether will be for the most part fruitless: the utmost we can do is to palliate symptoms; and this, I contend, is to be effected by the use of the bark *, &c. Dr Donald Monro, who had frequent opportunities of observing this complaint, tells us, “that many practitioners of great repute have been prejudiced against the bark, and inform us, that the

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“ free

* Sir John Pringle affirms, that the visceral obstructions following intermitting and remitting fevers, are not to be attributed to the bark, but to the continuance of the disease, and to frequent relapses. Dr Saunders and Dr Millar are both of the same opinion. Besides, Dr Cullen himself, in his *Materia Medica*, asserts, that “Bitters may be useful in obstructions of the liver and spleen, as in those of the other abdominal viscera, and therefore may be employed in cachectical cases.” See *Lectures on the Materia Medica*, Dublin 1781, 2d edit. p. 237.

“ free use of it often lays the foundation of
“ obstructions in the abdominal viscera,
“ especially when it has been given when
“ there was an icteritious colour in the eyes
“ and countenance, and that in such cases
“ we ought not to give the bark until these
“ symptoms are gone. At first he was very
“ cautious of giving it under such circum-
“ stances, till meeting with some instances
“ where the paroxysms were severe, and be-
“ came more frequent, while the patient
“ was so low as to be in danger of sinking
“ under the disorder; he then gave the bark
“ freely, as the only remedy capable of pre-
“ serving life; which not only stopt the
“ ague, but carried off the icteritious symp-
“ toms, and restored the patient to perfect
“ health.” And he is persuaded, “ That
“ obstructions in the abdominal viscera
“ mostly arise from the obstinacy of the
“ disorder, and not from the use of the
“ bark; for he has oftener observed these
“ obstructions where little or no bark has
“ been used, than where it was given free-
“ ly*.

Mr

* Monro's Observat.

Mr Cleghorn, too, is of opinion, that although the use of the bark has been considered as being improper when there is an icteritious colour in the eyes, yet it ought to be given immediately after the appearance of this symptom, and that it is dangerous to delay it*. And Dr Lind acquaints us, “ That during the siege of the
 “ Havannah, while the English troops suffered so considerably by the diseases incidental to Europeans in hot climates, the
 “ administration of the bark was blamed; because, after the fever had been removed
 “ by its efficacy, the patients were apt to be afflicted either with the jaundice, a drop-
 “ sy, or a swelling and obstruction in the liver. But such diseases and obstructions
 “ of the liver were much less dangerous and fatal than the fever, and were not the effects of the bark, but of the patients continuing in so impure an air, and of the
 “ duration of the fever †.” “ And when the fever was stopt by the bark immediately
 “ after the first or second fit, as in my own
 “ case,

* Cleghorn's *Minorca Diseases*.

† Lind's *Hot Climates*, p. 185.

“ case, and of those of two hundred of my
“ patients, neither a jaundice or dropfy en-
“ sued ; whereas, when the bark could not
“ be administered on account of the imper-
“ fect remissions of the fever, or when the
“ patient had neglected to take it, either a
“ dropfy, jaundice, or constant headach,
“ were the certain consequences ; and the
“ degree of violence was in proportion to
“ the number of the preceding fits, or to
“ the continuance of the fever. By every
“ paroxysm the dropfical swellings were vi-
“ sibly increased, and the colour of the skin
“ rendered of a deeper yellow*.”

Another objection to the use of the bark is, that when fevers change their type from an intermitting form to that of a continued fever, many physicians, under such a circumstance, reject the bark, and have recourse to evacuants, with the view of procuring an intermission or remission. That such a practice is totally void of support, will appear from the authorities adduced, when speaking of the proximate cause. I would, however, ask one question of such
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* Lind on Hot Climates, p. 313.

physicians: What occasions the fever to change from an intermitting or remitting form, to that of a continued? I am much of opinion they will agree with me in saying, That it depends on a greater degree of weakness being induced: if so, then why employ evacuations? Does not the more frequent recurrence of paroxysms indicate a more violent disease? I apprehend this cannot be denied me: And if so, why not persist in the use of the bark? or if this medicine is not found, upon the trial, to subdue the complaint, why not then have recourse to other medicines more invigorating? And that there is not the smallest ground for the employing debilitating powers, appears from the practice of Dr Sydenham, who did not hesitate to prescribe the bark when intermitting fevers became continued: neither did he find that wine hurt the patient when administered with the bark; on the contrary, the heat, thirst, and other febrile symptoms, generally went off, after a sufficient quantity had been taken. The nearer the intermittent approached to a continued

tinued form, he found it the more necessary to give a larger quantity of the bark.

The next remedy that I speak of is

WINE. Wine may perhaps in a great measure supersede every other stimulant in the cure of fever; and it has this great advantage over all others, of being generally agreeable to the palate. A few instances have occurred to me where wine-and-water has been called for by the patient, who drank it with the greatest avidity. Of all the wines that are presented to us, I think the red Port answers the purpose as well as any other; and, with respect to the mode of giving it, I am of opinion it ought always to be conjoined with the bark: it prevents the bark from being rejected by the patient, and makes it sit more easy on the stomach. If the bark is, however, altogether rejected, we ought in this case not to persist in its use, but administer in its place a greater proportion of wine. By proceeding in this manner for a day or two, the disease may in some degree be subdued; when another trial of the bark should be made, which will now

probably be retained by the stomach ; if not in substance, a strong decoction or infusion should be substituted. When both the bark and wine are given, the dose of the one should alternate with the other, so as to keep up a proper degree of excitement. If the bark be given every hour, for instance, half a glass of the wine between each dose of the bark will be for the most part a sufficient quantity ; but fully double this should be taken where no bark is exhibited ; in such cases, a bottle in the day will be a tolerable dose. Our constant object, however, which I cannot too much inculcate, is to administer a small quantity at a time, and repeat it frequently, to prevent, if possible, any degree of collapse. As there is for the most part a considerable thirst during the fever, some weak wine and water, if agreeable to the patient, will answer very well for common drink.

The next medicine that comes in succession, is OPIUM.

This is a drug possessing a very powerful degree of stimulus, insomuch that its
use

use should be chiefly, if not altogether, confined to those diseases which require such a highly stimulant power. I conceive it is solely owing to this that writers on the *Materia Medica* have ranked opium under the class of Sedatives: and yet we find these very writers everywhere speaking of its stimulant effect; and particularly, on this account, they strictly forbid its use in inflammatory diseases. With equal propriety might brandy, wine, ether, volatile alkali, and the whole class of stimulants, be ranked under sedatives, as opium; for every one of these will prove sedative in their secondary operation. It is in this way that excess in drinking proves in many cases a remote cause of fever; and yet no one will assert, that as long as the stimulant effect of the liquor continues, it is a remote cause of fever depending upon debility. Many people contend, as a proof of opium being a sedative, that it can totally extinguish life, and that it has the power of inducing sleep, which likewise depends on its effect as such. Respecting the first of these arguments, I would allege, that brandy, ether, and many other
other

other highly stimulant powers, will produce the same effect if taken in sufficient quantity; for it is to be observed, that opium only destroys life by an indirect operation; when its primary operation is over, the collapse ensues: and as a proof of this we find, that patients labouring under any inflammatory complaint never die during the inflammatory stage; it is only after this state when the debility takes place in consequence of the excitement, that they run any risk; to prevent which, during the inflammatory diathesis, we must have recourse to evacuations.

And, with regard to the latter argument, I would assert, that opium, in many cases, instead of procuring sleep, prevents it by its stimulant effect. If a person has an inflammatory disease, and gets a dose of opium, I will affirm, that it will increase every symptom of the inflammation considerably; and, instead of inducing sleep, it will have the contrary effect, of making the patient watchful. A very remarkable case of this nature came within my own knowledge. A gentleman laboured under mania, with
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the most furious delirium and pervigilium. The physician that attended, a man of eminence in his profession, very properly prescribed venæsection, and likewise ordered a pretty smart purge. According to his direction he was bled, and on the same morning he got the cathartic. The consequence of the evacuations was, that from being delirious in the morning, towards the evening his senses were infinitely more perfect, and he was inclined to sleep. But most unfortunately the people of the house with whom he lodged, gave him about 40 drops of laudanum, thinking it would favour his inclination to sleep. It had however the most directly opposite effect. The pervigilium returned, with the redness and flushing of the face; and in short, by its administration, every symptom was grievously aggravated. I would even allege, that opium given to persons in perfect health at bedtime, will prevent, instead of occasioning sleep, until its stimulant effect is over, as I have frequently experienced. A tossing and tumbling were the certain consequences of my taking a draught composed
of

of forty drops of laudanum immediately on getting into bed. Therefore, when opium is prescribed to a patient, perhaps with only a slight indisposition, and when it is meant to induce sleep, it ought to be taken an hour or two before going to bed.

Another proof of opium being highly stimulant is the loquacity and cheerfulness which it produces even among those whose spirits are naturally depressed; and if taken in sufficient quantity, it can occasion intoxication. Dr Young informs us of a surgeon who always made it a point of taking a dose of opium before he performed any considerable operation, to support him under it*. There is a very striking example proving the stimulant effect of opium in the case of a hypochondrical patient, related in the 3d volume of the *Physical and Literary Essays*. Having read in some magazine or newspaper of the quantity of opium taken by the Turks, and of the invigorating effect it had upon them, he was led to procure some, and of which he took a very considerable quantity. The consequence was, that during its stimulant

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effect

* Young on Opium.

effect he experienced the utmost serenity and chearfulness, and he found himself in greater spirits and vigour than he had been sensible of for a considerable time before; but when its stimulating power had ceased to act, and a state of collapse ensued, he complained of a violent sickness, and fancied he saw frightful spectres, and in short he was in the utmost uneasiness. He however got better of these symptoms, and expressed his strong desire for another dose, having experienced the most inexpressible satisfaction for some time. The most ample proof, however, which we have of the effect of opium, is in the Turks, who are astonishingly addicted to it. They are sensible of its giving them a flow of spirits; and when they once begin to take it, a habit is soon acquired, and to produce the same effect the dose must always be augmented, and on their going to battle the quantity they take is almost incredible.

Taking it, therefore, for granted, that opium is possessed of a considerable stimulant effect, I proceed in the next place to observe, as in many cases the bark runs off by stool,

stool, though given in any form, that in order to obviate this a mixture with eighty or ninety drops of laudanum should be prescribed, of which the patient may take a spoonful or two every hour. There are also other cases which require the use of opium. Many intermittents prove so obstinate as not to be cured by the bark; and though we were to persist in its use we should not stop its progress, and the situation of the patient would become dangerous. Hence medicines of a more invigorating nature become necessary. Dr Donald Monro observes, that the bark was found in general the best remedy; yet it did not answer in every case, and when the patient sunk under its administration, other medicines were given which had the desired effect.

Dr Odier of Geneva remarks in a letter to Dr Duncan, “ of his having seen a case
 “ in St Thomas’s hospital in London, in
 “ which Dr Fordyce gave a dose of Dover’s
 “ powder, with a sweating draught of salt of
 “ hartshorn, two hours before the fit, in order
 “ to stop it. This succeeded very well. A
 “ profuse sweat came on just before the
 N 2 “ hour

“ hour the cold fit used to return, and it
“ was altogether prevented. Some doses of
“ Peruvian bark were afterwards admini-
“ stered; and this fever, which had before
“ resisted many remedies, even the bark it-
“ self, was then in a few days effectually
“ cured. This observation we have had
“ many opportunities of repeating here;
“ not only in the quartan, but also in ter-
“ tian agues; and we find, that wherever
“ the patient can be brought to sweat, the
“ fit is altogether stopt. If he does not
“ sweat, the fit is nevertheless much dimi-
“ nished thereby*.” In speaking of the
proximate cause of fever, I observed the
effect of opium and bark in exciting sweat,
with the diminished heat of the body that
took place; and likewise attempted the ex-
planation of this circumstance. I shall not
here make any comment on what I before
delivered, further than that in almost every
case of fever where I have read of stimulating
remedies being given, I constantly have
found that a profuse sweat was the never-
failing effect which followed their exhibition.

And

* Sixth Vol. of the Medical Comment.

And I would remark, that the sweating which follows the use of antimonials never becomes near so universal as that procured by opium, &c. though to favour the operation of the antimonial, the patient is generally loaded with a heap of bedcloaths, and probably is desired to put on a flannel shirt; and notwithstanding this, the sweating is for the most part extremely partial, appearing only in those places nearest the heart, the extremities being nearly void of it; while as Dr Lind observes, there is an agreeable softness of the skin in those who are sweating from the use of opium, instead of the burning sensation which affects persons sweating in the hot fit, and who are not under its influence.

Under the article of MUSK, I have very little to offer, as opium in every case may supersede its use. There can be little doubt, however, of its having proved to be of great efficacy in removing hiccup and subsultus tendinum, the consequence of advanced stages of weakness. Dr Clark observes, that

“ musk given to the quantity of a scruple

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“ every

“ every four hours, seldom failed to abate
“ the hiccup and other convulsive symp-
“ toms attendant on fever, and to act as a
“ powerful cordial and diaphoretic*.” The
great objection to musk is, we so seldom
get it genuine, that we cannot depend
much on the effect which the dose may
have, and at the same time the extravagant
price it bears, renders it a medicine not
adapted to the circumstances of every pa-
tient. When musk is notwithstanding pre-
ferred to opium, it is still to be observed,
that it only becomes necessary when the
disease is far advanced, and the patient
stands in need of such a cordial: therefore
the first dose should be pretty large, about
the quantity of half a drachm made into
a bolus; and after this, smaller doses should
be substituted; carefully preventing any
collapse, by giving a second dose before the
effect of the first be perfectly worn off.

Having now finished the remarks I pro-
posed concerning the remedies to be em-
ployed in fevers, I have only to observe,
that blisters may occasionally be had re-
course

* Clark on Hot Climates, p. 142.

course to. When a fever is considerably advanced, attended with a quick fluttering pulse and coma, I am of opinion the application of a blister will have a good effect. In two cases I remember to have treated, the patients were in a very bad situation, having frequent pickings at the bedcloaths and subfultus tendinum, I ordered a blister to be applied to their heads in the evening; and next morning I was not a little surpris'd to find them considerably better, when I expected every hour would have been their last. The pulse in both, which had been so feeble and quick in the evening as hardly to be counted, was now much diminished in quickness, and had acquired a greater firmness. This effect I was pretty certain was owing to the blister; for they had not taken any thing during the night. Nor do I think it so astonishing as I then did, as Dr Lind has found the application of them followed with an excellent effect. He has ordered patients in the evening to be blistered, and has left them with pain and confusion of the head, with a quick pulse, great heat, immoderate thirst, and the
eye

eye has exhibited such a dejected look as to portend imminent danger. But next morning he has found them infinitely better, their eyes have assumed a more natural cast, the pulse has been diminished in quickness, and their general appearance has bespoke a nearer return to health *. Blisters are, however, only to be employed in the advanced stages of fever; and then their application is to be considered merely as adjuvants, as we at the same time administer our stimulants internally. Air of a moderate temperature may also be employed: it for the most part proves very refreshing to the patient; prevents the accumulation of the effluvia from his body, which as a cause of many fevers will no doubt aggravate the disease when already existing, and therefore should be studiously avoided: and if the patient's strength will permit, he might be taken out of bed; but it is requisite he should be placed in a horizontal posture, to prevent the risk of his falling into a faint.

The PRACTICE here recommended is applicable

* Lind on Fevers and Infection.

plicable to the cure of intermittents as well as to fevers of the continued type; and from a thorough conviction of its propriety and necessity, I trust that the arguments and facts brought in support of it will be maturely considered before they are deemed unavailing.

N. B. The Author flatters himself, any inaccuracies in the style, with which he is certain this performance abounds, the candid reader will readily overlook.

F I N I S.

E R R A T A.

- Page 8. line 5. for *has*, read *have*.
P. 38. line 6. for *evinces*, read *evince*.
P. 62. line 6. instead of *involuntary excretions*, read *involuntary passing of the excretions*.
P. 64. line 21. for *depend* read *depends*.
P. 95. line 20. for *are*, read *is*.
P. 120. line 14. for *are*, read *is*.
P. 164. line 2. for *tonic*, read *tonics*.

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