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AN

ESSAY

ON THE

NATURE OF FEVER,

BEING

AN ATTEMPT TO ASCERTAIN

THE PRINCIPLES

OF ITS

TREATMENT.

BY

A. PHILIPS WILSON, M. D. F. R. S. Ed.

Fellow of the Royal College of Physicians of Edinburgh, &c.

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CONTENTS.

II. *Of*

Two of the Notes have accidentally been numbered 32, and two of them 42.

Page 179, l. 22, after *head*, add, *which occasions compression of the brain.*

ADVERTISEMENT.

As the term *proximate cauſe* will frequently occur in the following Eſſay, it is neceſſary to define the ſenſe in which I uſe it; for it will appear, I think, on the moſt curſory view, that different writers do not always affix the ſame meaning to it. Thoſe who ſay that there can be no rational indications of cure, but ſuch as are founded on a knowledge of the proximate cauſe of diſeaſes, surely do not make the ſame application of this term, as thoſe who maintain that an attempt to inveſtigate the proximate cauſe, is abſurd, until we are poſſeſſed of

of organs capable of detecting the minuteſt motions of the animal ſyſtem. The former cannot mean that our whole attention ſhould be directed towards acquiring a knowledge, which every one muſt know it is impoſſible in the nature of things for us to acquire; nor is it the intention of the latter to aſſert that it is impoſſible for us to acquire ſuch a knowledge of the morbid ſtates of the body, as we poſſeſs of many of its functions in health, or that ſuch a knowledge would not be of the moſt eſſential importance in the treatment of diſeaſes.

The proximate cauſe has been defined, that ſtate of the body which, when preſent, cauſes, when removed, removes, and when changed, changes, the diſeaſe. When we have aſcertained any cauſe to which this definition applies, and pointed out the means

means of removing it, we have done all that is neceſſary. Theſe, therefore, ought to be our objects in treating of the nature of diſeaſe. It ſignifies not what minute motions of the ſyſtem produced this cauſe, or by what minute motions the means which remove it, operate.

The word proximate cauſe is objectionable; it by no means expreſſes the idea which the beſt writers affix to it, and has led to fanciful theories reſpecting theſe minute motions of the ſyſtem, which are doubtleſs the more immediate, and therefore, ſtrictly ſpeaking, the proximate cauſe of all diſeaſes, but which we cannot trace, and which, if we could trace, it is probable we could not regulate.

INTRODUCTION.

FROM the ſilence of Phyſicians on the proximate cauſe of fever, ſince the appearance of Dr. Cullen's *Firſt Lines*, and Dr. Brown's *Elementa Medicinæ*, they appear to have regarded its inveſtigation as a hopeleſs labour; and it had, before this period, baffled the attempts of ſo many, that the following Eſſay would require ſome apology, were it not that in the advancing ſtate of ſcience, every new inquirer has better data than thoſe who preceded him.

Beſides,

Beſides, certain pre-conceived opinions ſeem to have influenced all who have written on the nature of fever; and it appears to me, that the difficulty which has attended the inquiry, has proceeded as much from the manner in which it has been conducted, as from the nature of the ſubject. Many have formed their hypotheſes either from a partial view of fever, as in the *Firſt Lines*, or from other phenomena of the animal ſyſtem, as in the *Elementa Medicinæ*; and the chief aim of the theoriſt has been to ſhew in what way his hypotheſis may be applied to account for the phenomena of fever.

This mode of reaſoning would not be admitted in any other ſcience. It is not only neceſſary for the theoriſt to point out the ſteps by which he has arrived at his concluſion, but to ſhew that the phenomena

mena admit of no other, and not till then is he entitled to deſcend from his hypotheſis to the phenomena, and to ſhew in what manner it accounts for thoſe, which were not previouſly noticed, becauſe they were not neceſſary to eſtabliſh it.

Another cauſe which has retarded the inquiry into the proximate cauſe of fever, is a frequent reference to the *vis medicatrix naturæ*. Phyſicians ſeem to have thought, that to explain the nature of any diſeaſe, it is only neceſſary to ſhew that it is a ſalutary effort of the ſyſtem, forgetting that the nature of the diſeaſe muſt firſt be underſtood before it is poſſible to aſcertain whether its tendency is ſalutary or not.

No writer refers more frequently to the *vis medicatrix naturæ* than Dr. Cullen, yet in the Preface to his *Firſt Lines*, after ſome

ſome obſervations on the doctrine of Stahl, he remarks; "I might go farther, and "ſhew how much the attention to the "*Autocrateia*, allowed of, in one ſhape or "other, by every ſect, has corrupted the "practice among all Phyſicians, from Hip-"pocrates to Stahl. It muſt, however, be "ſufficiently obvious, and I ſhall con-"clude the ſubject with obſerving, that "although the *vis medicatrix naturæ* muſt "unavoidably be received as a fact, yet "wherever it is admitted, it throws an "obſcurity upon our ſyſtem; and it is "only where the impotence of our art is "very manifeſt and conſiderable, that we "ought to admit of it in practice." I ſhall have occaſion, in the beginning of the Appendix, to make ſome obſervations on this ſubject.

In the firſt volume of my Treatiſe on Febrile Diſeaſes, publiſhed in 1799, the reader

reader will ſee the imperfect outline of the preſent Eſſay. I there only pointed out, that in fever the laws of excitability differ from thoſe of health, and this change I was contented to regard as the proximate cauſe of fever, without attempting to aſcertain on what it depends.

In the ſecond volume, which appeared in 1800, I compriſed in the following lines the reſult of my obſervations on this ſubject.

" Towards aſcertaining the proximate cauſe of fever I did not attempt to do much. It appears, as far as I can judge from the obſervations alluded to, that fever is not owing to any change induced on the fluids, their becoming too acrimonious, too viscid, &c. nor to any change in the ſtate of the ſimple ſolid, nor to a partial change in that of the living ſolid, ſuch as relaxation,

or

or ſpaſm of particular parts; nor to any exhauſtion or accumulation of the excitability; (theſe, as far as I know, are the only rational opinions which have been maintained on the ſubject); but to the laws of excitability being changed, not in any one, but in every part of the living ſolid, and equally changed in every part of it, in conſequence of which, the natural agents no longer produce moderate excitement, followed by exhauſtion, but atony, or that degree of excitement which is followed by atony."

In arriving at theſe concluſions, I was led to conſider minutely the hypotheſis of Dr. Brown, which profeſſed, and by many was believed, to unfold the true nature of fever; whereas Dr. Cullen's, as will appear from what I ſhall have occaſion to ſay of it in the firſt part of this Eſſay,* was confeſſedly

* See the 58th and two following Pages.

feſſedly hypothetical. I endeavoured to point out, that the laws of excitability maintained by Dr. Brown, apply only to thoſe powers on which the animal functions depend; that there is a ſtate of debility, which I termed atony, wholly different from Dr. Brown's exhaustion, or accumulated excitability; and a ſtate of excitement which differs from Dr. Brown's excitement in not being ſucceeded by exhauſtion, but by atony, which, to diſtinguiſh it, I called exceſſive excitement; that Dr. Brown's excitement and exhauſtion are always healthy ſtates, and conſequently that he erred in ſuppoſing the ſyſtem in diſeaſe regulated by the ſame laws which it obeys in theſe ſtates, and therefore failed in applying his doctrine to the cure of diſeaſes; that exceſſive excitement and atony are the only general diſeaſes properly ſo called, all other diſeaſes of the ſyſtem ariſing from ſome local diſeaſe, and that

theſe

theſe ſtates are the ſame which we term ſynocha and typhus.

After devoting, in vain, much time to a review of the phenomena of fever, in order to acquire a more explicit knowledge of its proximate cauſe, I republiſhed my obſervations on this ſubject in 1803, in the ſame words, with very few exceptions, in which they firſt appeared. It ſeems to me, that ſince that time my endeavours have been more ſucceſsful, and that the cauſe of the change in the laws of excitability which we obſerve in fever, may be diſtinctly traced, and its phenomena explained, in as ſimple and ſatisfactory a way as thoſe of any other diſeaſe.

I ſhall divide the following Eſſay into two parts. In the firſt, I ſhall conſider the chief opinions which have prevailed reſpecting the proximate cauſe of fever, and point

point out what appears to me to be their defects. In the other, I ſhall lay before the reader the ſteps by which I arrived at my opinion on this ſubject, and the manner in which it explains the various phenomena of fever.

AN ESSAY, &c.

CHAP. I.

Of the Opinions which have prevailed reſpecting the proximate Cauſe of Fever.

IT required but a very imperfect knowledge of the animal œconomy to ſuggeſt, that ſome of the phenomena of fever may be explained by ſuppoſing the blood to have acquired morbid properties; and, till the time of Stahl, Hoffman and Boerhaave, Phyſicians endeavoured in vain to ſhew that the blood is ſo changed in fevers as to account for all their phenomena, on the ſuppoſition of the ſolids remaining in the ſame

ſame ſtate as in health. To give an account of the more ancient opinions reſpecting the proximate cauſe of fever, or even of thoſe which prevailed at the time juſt mentioned, when all Phyſicians were either followers of Galen, or had adopted the bolder, but equally unfounded hypotheſis of Paracelſus, would be a tedious and uninſtructive taſk. The error of the principles on which theſe hypotheſes reſt, will ſufficiently appear from what it will be neceſſary to ſay of the opinions of later writers.

About the time I am ſpeaking of, the diſcovery of the circulation of the blood, the moſt important ſtep ever made towards a knowledge of the animal œconomy, and the true principles of reaſoning, unfolded in the works of Lord Bacon, began gradually to open the eyes of Phyſicians to the errors of their ſyſtems. When they found that the fluids perform but a ſubordinate

dinate part in the animal œconomy, and were taught to diſtinguiſh between the workings of fancy and the inductions of reaſon, they began to feel leſs reverence for the ſyſtem of Galen, and the opinions of Paracelſus. But theſe cauſes operated ſlowly; and, although Phyſicians were enabled to detect the errors of their predeceſſors, they were not prevented from falling into ſimilar errors themſelves, or even from, in part, adopting the errors they expoſed.

It is true that Stahl, without heſitation or reſerve, rejected both the hypotheſes and mode of reaſoning adopted by thoſe who went before him; but to compenſate for the ſervice he thus rendered to medicine, his own hypotheſis more eminently ſets at defiance all juſt rules of reaſoning than that of any of his predeceſſors.* He, however, muſt be regarded as the firſt who diverted the attention of Phyſicians from what

* Appendix, Note 1.

what Dr. Cullen calls the humoral pathology.

Hoffman and Boerhaave retained many of the moſt exceptionable parts of this pathology; but their writings occaſioned a change in medical reaſonings, of far greater conſequence than any that could be effected by the chimerical hypotheſis of Stahl. They pointed out the ſolids as that part of the animal ſyſtem to which we are chiefly to look for the cauſes of the various changes we obſerve in it.

The ſyſtems of theſe writers prevailed till the days of Cullen and Brown, who admiring the effects of this change in medical reaſoning, have, if I may be allowed to give my opinion, carried it much too far, by endeavouring to account for all the ſymptons of diſeaſes by the changes induced on the ſolids alone.

I ſhall make a few obſervations on the theories of Hoffman and Boerhaave,

and

and then conſider more at length thoſe of Cullen and Brown.

SECT. I.

Of the Hypotheſes of Hoffman and Boerhaave.

While Hoffman admitted many parts of the hypotheſes of his predeceſſors, he ſaw the neceſſity of a principle eſſentially different from any hitherto aſſumed. As this principle is the only original, and indeed the only valuable part of his hypotheſis, we ſhall confine our attention to it, and we cannot have a better account of it than in the author's words, which I ſhall quote: "Ex hiſce autem omnibus uberius "hactenus excuſſis, perquam dilucide ap- "parere arbitror, quod ſolus ſpaſmus et "ſimplex atonia, æquabilem, liberum, ac "proportionatum ſanguinis, omniſque ge- "neris fluidorum motum, quibus excreti- "onum ſucceſſus et integritas functionum "animi et corporis proxime nititur, tur-

"bando

" bando ac pervertendo, univerſam vitalem
" æconomiam ſubruant ac deſtruant; atque
" hinc univerſa pathologia longe rectius
" atque facilius ex vitio motuum microcoſ-
" micorum in ſolidis, quam ex variis af-
" fectionibus vitioſorum humorum, deduci
" atque explicari poſſit; adeoque omnis
" generis ægritudines internæ ad preterna-
" turales generis nervoſi affectiones ſint
" referendæ. Etenim læſis quocunque
" modo, vel nervis per corpus diſcurren-
" tibus, vel membranoſis quibuſvis nervoſis
" partibus, illico motuum anomaliæ, modo
" leviores modo graviores ſubſequuntur.
" Deinde attenta obſervatio docet, motus
" quoſvis morboſos ſedem figere et tyrani-
" dem exercere in nervoſis corporis parti-
" bus, cujus generis (præter omnes cana-
" les, qui ſyſtaltico et diaſtaltico motu
" pollentes, contentos ſuccos tradunt, uni-
" verſum nimirum inteſtinorum et ven-
" triculi ab æſophago ad anum canalem,

" totum

" totum ſyſtema vaſorum arterioſorum,
" ductuum biliariorum, ſalivalium, urina-
" riorum et ſubcutaneorum, ſunt quoque
" membranæ nerveo-muſculares cerebri
" et medullæ ſpinalis, præſertim hæc quæ
" dura mater vocatur, organis ſenſoriis ob-
" ductæ, nec non tunicæ illæ ac ligamenta
" quæ oſſa cingunt artuſque firmant. Nam
" nullus dolor, nulla inflammatio, nullus
" ſpaſmus, nulla motus et ſenſus impo-
" tentia, nulla febris aut humoris illius
" excretio accidit, in qua non hæ partes
" patiuntur.

" Porro etiam omnes quæ morbos
" gignunt cauſæ, operationem ſuam potiſſi-
" mum perficiunt in partes motu et ſenſu
" præditas, et canales ex his coagmentatos,
" eorum motum, et cum hoc, fluidorum
" curſum, pervertendo; ita tamen, ut ſicuti
" variæ indolis ſunt, ſic etiam varie in
" nerveas partes agunt, iiſdemque noxam
" afferunt. Demum omnia quoque eximiæ

" virtutis medicamenta non tam in partes
" fluidas, earum craſin ac intemperiem
" corrigendo, quam potius in ſolidas et
" nervoſas, earundem motus alterando ac
" moderando ſuam edunt operationem: de
" quibus tamen omnibus, in vulgari uſque
" eo recepta morborum doctrina, altum
" eſt ſilentium."*

On the foregoing paſſage Dr. Cullen obſerves, " There can be no ſort of doubt " that the phenomena of the animal œco- " nomy in health and in ſickneſs, can only " be explained by conſidering the ſtate and " affections of the primary moving powers " in it. It is to me ſurpriſing that Phyſi- " cians were ſo long in perceiving this, " and I think we are therefore particularly " indebted to Dr. Hoffman for putting us " into the proper train of inveſtigation; " and it every day appears that Phyſicians " perceive the neceſſity of entering more

" and

* See Dr. Hoffman's Med. Rat. Syst. Tom. III. § 1. Chap. 4.

" and more into this inquiry. It was this, " I think, which engaged Dr. Kaaw Boer- " haave to publiſh his work entitled *Impetum* " *faciens*; as well as Gaubius to give the " Pathology of the *Solidum vivum*. Even " the Baron Van Swieten has upon the " ſame view thought it neceſſary, in at " leaſt one particular, to make a very con- " ſiderable change in the doctrine of his " maſter, as he has done in his commentary " upon the 755th Aphoriſm. Dr. Haller " has advanced this part of ſcience very " much by his experiments on irritability " and ſenſibility. In theſe and in many " other inſtances, particularly in the writ- " ings of Mr. Barthez, of Montpelier, of " ſome progreſs in the ſtudy of the affections " of the nervous ſyſtem, we muſt perceive " how much we are indebted to Dr. Hoff- " man for his ſo properly beginning it."

The principal circumſtance in which the ſyſtem of Boerhaave differs from that of

of Hoffman, is, in the former's reſting rather on the ſtate of the ſimple than of the living ſolid; for Boerhaave believed that moſt of the changes of the animal ſyſtem, which do not depend on the ſtate of the fluids, ariſe chiefly from the rigidity or laxity of the ſimple ſolid.

Of the ſtate of the fluids, being a better Chemiſt, he ſpoke more accurately than his predeceſſors; but both in this and the other parts of his ſyſtem his data are in general hypothetical, in ſome inſtances evidently erroneous, and his reaſonings lead to no concluſions which can now be admitted.

The conſiſtency and ſimplicity of Boerhaave's ſyſtem, together with the great knowledge diſplayed both by himſelf and his commentator Van Swieten, in ſupport of it, occaſioned it to be very generally received, and for many years it wholly ſuperceded the ſyſtem of Hoffman.

Later

Later Phyſiologiſts, however, have pointed out, in a ſatisfactory manner, that in the changes of the animal ſyſtem the living, not the ſimple, ſolid is chiefly concerned; and in medical reaſoning the ſtate of the latter is now almoſt neglected. However much the original conſtitution of an animal body may depend on the ſtate of the ſimple ſolid, and however much a change in this ſolid may affect its ſtate at any period of life, the changes induced on the ſimple ſolid muſt in general themſelves depend on the ſtate of the moving powers. Beſides, no writer has pointed out to us of what nature theſe changes are, or in what manner they affect the ſyſtem; while, on the other hand, we readily obſerve many changes which take place in the living ſolid, and the conſequences which attend them.

Theſe circumſtances at length led Phyſicians to reject the ſyſtem of Boerhaave,

haave, and to aim at ſuch a modification of that of Hoffman, as ſhould adapt it to the improved ſtate of phyſiological knowledge. This has been attempted by three writers of great name, Dr. Cullen, Dr. Brown, and Dr. Darwin. The opinions of the two latter are ſo ſimilar, that it will be unneceſſary to examine both, and as Dr. Brown's work was firſt given to the world, although it would appear that Dr. Darwin's was written about the ſame time, I ſhall confine my obſervations to the former.

SECT. II.

Of the Hypothesis of Dr. Cullen.

In the preſent ſection, I mean in the firſt place to give a view of Dr. Cullen's hypotheſis, which cannot be better done than in his own words, and then to enquire how far it explains the nature of fever.

"The

"The proximate cauſe of fever," he obſerves, "ſeems hitherto to have eluded "the reſearch of Phyſicians; and I ſhall "not pretend to aſcertain it in a manner "that may remove every difficulty; but I "ſhall endeavour to make an approach "towards it, *and ſuch as I hope may be of* "*uſe in conducting the practice in this diſeaſe;* "while at the ſame time I hope to avoid "ſeveral errors which have formerly pre-"vailed on this ſubject.

"As the hot ſtage of fever is ſo con-"ſtantly 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. See Boerh. app. 756.

"To diſcover the cauſe of the cold "ſtage of fevers, we may obſerve, that it "is always preceded by ſtrong marks of a "general debility prevailing in the ſyſtem.

"The

" The ſmallneſs and weakneſs of the pulſe,
" the paleneſs and coldneſs of the extreme
" parts, with the ſhrinking of the whole
" body, ſufficiently ſhew that the action
" of the heart and larger arteries is for
" the time extremely weakened. Together
" with this, the languor, inactivity and
" debility of the animal motions, the im-
" perfect ſenſations, the feeling of cold
" while the body is truly warm, and ſome
" other ſymptoms, all ſhew that the energy
" of the brain is on this occaſion greatly
" weakened; *and I preſume, 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 diminiſh-
" ed energy of the brain.

" I ſhall hereafter endeavour to ſhew,
" that the moſt noted of the remote cauſes
" of fever, as contagion, miaſmata, cold
" and fear, are of a ſedative nature, and
" *therefore render it probable that a debility*

" *is*

" *is induced.* Likewiſe, when the parox-
" yſms of a fever have ceaſed to be repeat-
" ed, they may again be renewed, and are
" moſt commonly renewed, by the appli-
" cation of debilitating powers; and fur-
" ther, the debility which ſubſiſts in the
" animal motions, and other functions,
" through the whole of the fever, renders
" it *pretty certain* that ſedative or debili-
" tating powers have been applied to the
" body.

" It is therefore evident, that there
" are three ſtates which always take place
" in fever—a ſtate of debility, a ſtate of
" cold, and a ſtate of heat; and as theſe
" three ſtates regularly and conſtantly ſuc-
" ceed each other, in the order we have
" mentioned them, *it is preſumed that they*
" *are in the ſeries of cauſe and effect with*
" *reſpect to one another.* This we hold as
" a matter of fact, even although we
" ſhould not be able to explain in what

" manner, or by what mechanical means
" theſe ſtates ſeverally produce each other.

" How the ſtate of debility produces
" ſome of the ſymptoms of the cold ſtage,
" may perhaps be readily explained; *but*
" *how it produces all of them, I cannot ex-*
" *plain otherwiſe than by referring the mat-*
" *ter to a general law of the animal œconomy,*
" *whereby it happens, that powers which have a*
" *tendency to hurt and deſtroy the ſyſtem, often*
" *excite ſuch motions as are ſuited to obviate*
" *the effects of the noxious power. This is*
" *the vis medicatrix naturæ, ſo famous in*
" *the ſchools of phyſic; and it ſeems proba-*
" *ble, that many of the motions excited in*
" *fever are the effects of this power.*

" *That the encreaſed action of the heart*
" *and arteries, which takes place in the hot*
" *ſtage of fevers, is to be conſidered as an*
" *effort of the vis medicatrix naturæ, has*
" *been long a common opinion among Phyſi-*
" *cians, and I am diſpoſed to aſſert, that*
" *ſome*

" *some part of the cold ſtage may be im-*
" *puted to the ſame power.* I judge ſo,
" becauſe the cold ſtage appears to be
" univerſally a means of producing the
" hot: becauſe cold externally applied has
" very often ſimiliar effects; and more
" certainly ſtill, becauſe it ſeems to be in
" proportion to the degree of tremor in the
" cold ſtage, that the hot ſtage proceeds
" more or leſs quickly to a termination of
" the paroxyſm, and to a more complete
" ſolution and longer intermiſſion.

" It is to be particularly obſerved,
" that during the cold ſtage of fever there
" ſeems to be a ſpaſm induced every where
" on the extremities of the arteries, and
" more eſpecially of thoſe on the ſurface
" of the body. This appears from the
" ſuppreſſion of all excretions, and from
" the ſhrinking of the extreme parts: and
" although this may perhaps be imputed,
" in part, to the weaker action of the
" heart,

" heart, in propelling the blood into the " extreme veſſels, yet as theſe ſymptoms " often continue after the action of the " heart is reſtored, *there is reaſon to believe* " that a ſpaſmodic conſtriction has taken " place, that it ſubſiſts for ſome time, and " ſupports the hot ſtage; for this ſtage " ceaſes with the flowing of the ſweat, and " the return of other excretions, which " are marks of the relaxation of veſſels " formerly conſtricted.—Hoffman. Med. " Rat. Syſt. Tom. 4th, P. I. § 1. Cap. 1. " Art. 4.

" The idea of fever then *may be*, that " a ſpaſm of the extreme veſſels, however " induced, proves an irritation to the " heart and arteries, and that this con- " tinues till the ſpaſm is relaxed or over- " come. There are many appearances " which ſupport this opinion, and there is " little doubt that a ſpaſm does take place, " which proves an irritation to the heart,

" and

" and therefore may be conſidered as a " principal part in the proximate cauſe of " fever. *It will ſtill, however, remain a " queſtion, what is the cauſe of this ſpasm, " whether it be directly produced by the " remote cauſes of fever, or if it be only a " part of the operation of the vis medicatrix " naturæ?*

" *I am diſpoſed to be of the latter " opinion,* becauſe, in the firſt place, while " it remains ſtill certain that a debility lays " the foundation of fever, it is not obvious " in what manner the debility produces " the ſpaſm, and what ſeems to be its effect, " the encreaſed action of the heart and " arteries: and ſecondly, becauſe, in almoſt " all the caſes in which an effort is made " by the vis medicatrix naturæ, a cold fit " and a ſpaſm of the remote veſſels are " almoſt always the beginning of ſuch an " effort. See Gaub. Pathol. Medicin. Art. " 750.

" *It*

" *It is therefore preſumed*, that ſuch a " cold fit and ſpaſm at the beginning of " fever, is a part of the operation of the " vis medicatrix; but, at the ſame time, " *it ſeems to me probable*, that during the " whole courſe of the fever, there is an atony " ſubſiſting in the extreme veſſels, and that " the relaxation of the ſpaſm requires the " reſtoring of the tone and action of theſe.

" *This it may be difficult to explain*; but " I think it may be aſcertained as a fact, by " the conſideration of the ſymptons which " take place with reſpect to the functions " of the ſtomach in fevers, ſuch as the " anorexia, nauſea, and vomiting. From " many circumſtances it is ſufficiently cer- " tain, that there is a conſent between the " ſtomach and ſurface of the body; and, " in all caſes of the conſent of diſtant parts, " it is preſumed to be by the connection " of the nervous ſyſtem, and that the con- " ſent, which appears is between the ſentient

" and

" and moving fibres of the one part with
" thoſe of the other; is ſuch, that a certain
" condition prevailing in the one part oc-
" caſions a ſimilar condition in the other.
" In the caſe of the ſtomach and ſurface of
" the body, the conſent particularly ap-
" pears by the connection which is obſerved
" between the ſtate of the perſpiration and
" the ſtate of the appetite in healthy
" perſons; and if it may be preſumed
" that the appetite depends upon the ſtate
" of tone in the muſcular fibres of the
" ſtomach, it will follow that the connec-
" tion of appetite and perſpiration depends
" upon a conſent between the muſcular
" fibres of the ſtomach, and the muſcular
" fibres of the extreme veſſels, or of the
" organ of perſpiration, on the ſurface of
" the body. It is further in proof of the
" connection between the appetite and per-
" ſpiration, and at the ſame time of the
" circumſtances on which it depends, that
" cold

" cold applied to the ſurface of the body,
" when it does not ſtop perſpiration, but
" proves a ſtimulus to it, is always a pow-
" erful means of exciting appetite. Having
" thus eſtabliſhed the connection or conſent
" mentioned, we argue, that as the ſpmptoms
" of anorexia, nauſea, and vomiting, in
" many caſes manifeſtly depend upon a
" ſtate of debility or loſs of tone in the
" muſcular fibres of the ſtomach, *ſo it may*
" *be preſumed,* that theſe ſymptoms in the
" beginning of fever depend upon an
" atony communicated to the muſcular
" fibres of the ſtomach from the muſcular
" fibres of the extreme veſſels on the
" ſurface of the body. That the debility
" of the ſtomach which produces vomiting
" in the beginning of fever, actually depends
" upon an atony of the extreme veſſels on
" the ſurface of the body, appears par-
" ticularly from a fact obſerved by Dr.
" Sydenham. In the attack of the plague

" a vo-

" a vomiting happens which prevents any
" medicine from remaining on the ſtomach;
" and Dr. Sydenham tells us that in ſuch
" caſes he could not overcome this vomit-
" ing but by external means applied to
" produce a ſweat, that is, to excite the
" action of the veſſels on the ſurface of
" the body. The ſame connection be-
" tween the ſtate of the ſtomach and that
" of the extreme veſſels on the ſurface of
" the body, appears from this alſo: that
" the vomiting which ſo frequently hap-
" pens in the cold ſtage of fevers com-
" monly ceaſes upon the coming on of the
" hot, and very certainly upon any ſweats
" coming out. It is indeed probable that
" the vomiting, in the cold ſtage of fevers,
" is one of the means employed by nature
" for reſtoring the determination to the
" ſurface of the body; and it is a circum-
" ſtance affording proof both of this and
" of the general connection between the

"ſtomach and the ſurface of the body,
"that emetics thrown into the ſtomach,
"and operating there in the time of the
"cold ſtage, commonly put an end to it
"and bring on the hot ſtage. It alſo af-
"fords a proof of the ſame connection,
"that cold water taken into the ſtomach
"produces an encreaſe of heat on the
"ſurface of the body, and is very often
"a convenient and effectual means of pro-
"ducing ſweat. From the whole we have
"now ſaid on this ſubject, *I think it is*
"*ſufficiently probable* that the ſymptoms of
"anorexia, nauſea, and vomiting depend
"upon, and are a proof of, an atony
"ſubſiſting in the extreme veſſels on the
"ſurface of the body, and that this atony,
"therefore, now aſcertained as a matter
"of fact, may be conſidered as a principal
"circumſtance in the proximate cauſe of
"fever.

"This atony we ſuppoſe to depend
"upon

" upon a diminution of the energy of the
" brain; and that this diminution takes
" place in fevers we conclude not only
" from the debility prevailing in ſo many
" of the functions of the body mentioned
" above, but particularly from ſymptoms
" which are peculiar to the brain itſelf.
" Delirium is a frequent ſymptom of fever;
" and as from the phyſiology and patho-
" logy we learn that this ſymptom com-
" monly depends upon ſome inequality in
" the excitement of the brain, or intellectual
" organ, we hence conclude that in fever
" it denotes ſome diminution in the energy
" of the brain. Delirium, indeed, ſeems
" often to depend upon an encreaſed im-
" petus of the blood in the veſſels of the
" brain, and therefore attends phrenitis.
" It frequently appears alſo in the hot
" ſtage of fevers, accompanied with a head
" ach and throbbing of the temples. But
" as the impetus of the blood in the veſſels

" of

" of the head is often conſiderably encreaſed
" by exerciſe, external heat, paſſions, and
" other cauſes, without occaſioning any
" delirium; ſo, ſuppoſing that the ſame im-
" petus in the caſe of fever produces deli-
" rium, the reaſon muſt be that at the ſame
" time there is ſome cauſe which diminiſhes
" the energy of the brain, and prevents a
" free communication between the parts
" concerned in the intellectual functions.
" Upon the ſame principles alſo I ſuppoſe
" there is another ſpecies of delirium de-
" pending more entirely on the diminiſhed
" energy of the brain, and which may
" therefore ariſe when there is no unuſual
" encreaſe of the impetus of the blood in
" the veſſels of the brain. Such ſeems to
" be the delirium occurring at the begin-
" ning of the cold ſtage of fevers, or in
" the hot ſtage of ſuch fevers as ſhew
" ſtrong marks of debility in the whole
" ſyſtem.

" Upon

"Upon the whole our doctrine of "fever is explicitly this: The remote "cauſes 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 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 "the cold ſtage and ſpaſm connected with "it, the action of the heart and larger "arteries is encreaſed, 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 eſpe- "cially overcoming the ſpaſm affecting "them; upon the removing of which the "excretion of ſweat and other marks

"of

"of the relaxation of excretories take "place."

From the parts of the preceding quotation which are printed in Italic letters, it is evident that Dr. Cullen regarded his doctrine of fever as little more than an hypothesis, calculated to give arrangement to detached facts, which, without some system, readily slip from the memory.

He believed, indeed, and it must be admitted, that it was founded on a better principle than that of any of his predecessors, except Hoffman; and he perceived that it was more simple and consistent than the doctrines of this writer; but he looked forward to a more advanced state of science, in which his system would suffer a change similar to that which he had effected in Hoffman's. All this he expresses fully in the Preface to his *First Lines*.

"Upon this general plan," he observes,

ſerves, "I have endeavoured to form a "ſyſtem of phyſic that ſhould comprehend "the whole of the facts relating to the "ſcience, and that will, I hope, collect and "arrange them in better order than has "been done before, as well as point out, "in particular, thoſe which are ſtill want-"ing to eſtabliſh general principles. This "which I have attempted may, like other "ſyſtems, hereafter ſuffer a change; but "I am confident that we are at preſent in a "better train of inveſtigation than phy-"ſicians were in before the time of Dr. "Hoffman."

But ſetting aſide what Dr. Cullen ſays of his doctrine, if we examine the doctrine itſelf we ſhall find that it is wholly conſtructed on a hypothetical baſis, on the ſuppoſed operations of the vis medicatrix naturæ, reſpecting which we have in the Introduction to this Eſſay given Dr. Cullen's opinion.

How

How the debility of the nervous, proves an indirect ſtimulus to the ſanguiferous, ſyſtem; how this ſtimulus acts in exciting the cold ſtage and ſpaſm; how, through the intervention of theſe, the action of the heart and larger arteries is encreaſed; is only explained by reference to the operation of the vis medicatrix, that is, is not explained at all; and yet it appears even at firſt view that on theſe the whole ſyſtem reſts.

Shall we ſuppoſe Dr. Cullen, after declaring "that wherever the vis medica-"trix is admitted into medical ſyſtems it "throws an obſcurity on them," ſo inconſiſtent as to offer as a true ſyſtem one wholly and avowedly founded on the ſuppoſed operations of this agent!

On entering on a more particular conſideration of Dr. Cullen's hypotheſis, we meet with other fundamental objections to it. From the nervous debility which attends

tends the commencement of fever, he infers, that the firſt morbid change induced is a diminiſhed energy of the brain. But this debility is never obſerved at the commencement of fever, without a correſponding debility of the circulating ſyſtem. It was incumbent on Dr. Cullen, therefore, to enquire which was the primary affection. We know that every change in the ſtate of the circulating, is immediately felt in the nervous, ſyſtem. But on the other hand, that the latter is often moſt eſſentially deranged (as I ſhall ſoon have occaſion to point out more particularly) without materially, or even at all, deranging the functions of the former. The probability therefore is, that the cauſes of fever, which uniformly affect both ſyſtems, make their firſt impreſſion on the ſanguiferous ſyſtem. Dr. Cullen adduces no arguments to counterbalance this probability.

We have every reaſon to believe that a ſpaſm of the extreme veſſels is not eſſential to fever, as Dr. Cullen ſuppoſes it to be. That atony of theſe veſſels is eſſential to fever we can hardly doubt, becauſe we ſee no inſtance of fever without evident marks of it; but whether the extreme veſſels fall into a ſtate of conſtriction, or relaxation, in conſequence of this atony, the nature of the ſymptoms which enſue, is the ſame. In ſome fevers the ſecreting ſurfaces are relaxed from the commencement.* Theſe have been found the moſt fatal, which will not ſurpriſe us when we reflect, that the debility which gives riſe to ſpaſm is much leſs than that which occaſions relaxation, in which the parts may be ſaid wholly to have loſt their tone. Hence conſtriction of the extreme veſſels is moſt common in the early, and relaxation in the latter ſtages of fever; and hence, alſo, it

* Appendix, Note 2.

it is, that although a ſweat, induced by a return of vigor to the extreme veſſels, is the moſt favourable of all the ſymptoms of fever, that kind of ſweat which ariſes from mere relaxation of theſe veſſels, is often among the moſt fatal.*

As Dr. Cullen conſiders a weakened energy of the brain as the cauſe of all the ſymptoms of fever, he believes that a reſtoration of the due energy of this organ is the only means of removing them. This he ſuppoſes to be effected by the encreaſed action of the heart and larger arteries. But as we never obſerve the energy of the brain reſtored, without a correſponding change taking place in the extreme veſſels; here again it was neceſſary for Dr. Cullen to enquire which was the primary change; and in this inſtance, alſo, the general laws of the animal œconomy oppoſe his opinion. Is it more conſonant with theſe

* Appendix, Note 3.

theſe laws to ſuppoſe, that the encreaſed action of the heart and larger arteries reſtores energy to the brain, and that this energy gives tone to the extreme veſſels; or, that the encreaſed action of the heart and larger arteries is the means of exciting the extreme veſſels, and that the tone of the brain is reſtored in the ſame way with that of the ſkin, kidnies, &c. by the renewed vigor of the circulation? Is it poſſible that there ſhould be a renewal of the energy of the brain until the functions of the extreme veſſels are reſtored?

Dr. Cullen ſuppoſes that the removal of the ſpaſm of the extreme veſſels, and the conſequent relaxation of the excretories neceſſarily remove the fever. But we find from obſervation that theſe may take place while the fever increaſes, while the pulſe becomes more frequent and feeble, the breathing more oppreſſed,

preſſed, and the various other functions, both natural and animal, more deranged.

Dr. Cullen's hypotheſis then appears to be doubly objectionable, as reſting on the ſuppoſed operations of the vis medicatrix naturæ, and being inconſiſtent with obſervation and the known laws of the animal œconomy.

As an hypotheſis to ſerve the temporary uſe of connecting together a long catalogue of facts, till we ſhould arrive at a more explicit knowledge of the nature of fever, it is elegant and ingenious; and admitting the data, accounts for all the phenomena of this diſeaſe: but Dr. Cullen is every where ready to confeſs the inſufficiency of theſe data.

Dr. Brown, on the other hand, with data no leſs, though not ſo apparently ill-founded, has endeavoured to conceal the defects of his hypotheſis, and give it the appearance of what he wiſhed it to be thought,

thought, a true ſyſtem of medicine; and it is ſo blended with certain poſitions, of the accuracy of which there can be no doubt, and which were univerſally admitted long before Dr. Brown wrote, that, to detect its errors, it will be neceſſary to examine it in detail.

SECT. III.

Of the Hypotheſis of Dr. Brown, and the Laws of Excitability.

We cannot ſtate Dr. Brown's hypotheſis* in the way in which Dr. Cullen's is ſtated in the preceding Section, for he never choſe to give any connected view of it, but left the reader to collect it from various paſſages of his *Elementa Medicinæ;* and ſuch are the contradictions that prevail in this work, that, in extracting from it a conſiſtent doctrine, it will be neceſſary, we ſhall find, to reject many paſſages which cannot

* Appendix, Note 4.

cannot be reconciled to the opinions in general maintained in it.

I ſhall give a conciſe view of this doctrine, and then point out what appears to me to be its defects, at the ſame time conſidering ſuch topics as it is neceſſary to diſcuſs before we enter on the ſubject of the next Chapter; ſo that the preſent Section may be regarded as an introduction to that Chapter, as well as a view and examination of the Brunonian hypotheſis. To this arrangement there appears to be no objection; and, from the intimate connection of the ſubjects, it will ſave repetition.

That power by which the phenomena peculiar to the living ſtate are produced, is termed by Dr. Brown, excitability; under this term he includes both the nervous and muſcular power,* and ſeems in ſome places

* Appendix, Note 5.

places to include a power different from either.*

He conſiders every agent, capable of producing any change in the living body, an exciting power,† and conſequently terms all agents ſtimuli,‡ and their effect on the living ſolid he terms excitement.§

In proportion as they produce excitement, they exhauſt the excitability; that is, render the living body leſs capable of being excited.‖

As Dr. Brown does not admit that there is any agent capable of producing a ſedative effect, he maintains that excitement can only be diminiſhed by an exceſs or abſtraction of ſtimuli.**

The excitability, according to his hypotheſis, has no exiſtence except when acted upon by ſtimuli.†† By the total abſtraction of ſtimuli, it is as effectually deſtroyed as by their exceſſive application;

* Appendix, Note 6. † Note 7. ‡ Note 8. § Note 9.
‖ Note 10. ** Note 11. †† Note 12.

tion; it is on this account that Dr. Brown terms life a forced ſtate.

He does not conſider the excitability as a property reſiding in, and depending on the mechaniſm of, particular parts; but an uniform, indiviſible property, pervading the whole ſyſtem, which cannot be affected in any one, without being affected in a ſimilar way in every other part. *

Upon the whole, according to the Brunonian ſyſtem, the excitability (the power on which the phenomena of life depend) is an uniform, indiviſible property reſiding in every living body, whether animal or vegetable, to whoſe exiſtence the conſtant application of ſtimuli is neceſſary, which excitement tends conſtantly to exhauſt, which may be deſtroyed by the exceſſive application of ſtimuli, and which accumulates in conſequence of their partial abſtraction. †

With

* Appendix, Note 13. † Note 14.

With reſpect to the powers to which we are to attribute the accumulation of excitability, Dr. Brown leaves us in the dark. The powers in queſtion, it may be ſaid, are thoſe of digeſtion and aſſimilation. But how is this to be reconciled with the favorite hypotheſis of Dr. Brown, that a certain quantity of excitability, to laſt through life, is beſtowed on every living body at the commencement of its exiſtence?*

Between the healthy ſtate, in which the excitability and ſtimuli applied are in due proportion, and death, in which the excitability is extinguiſhed, either by exceſs, or too great an abſtraction, of ſtimuli, the Brunonian ſyſtem ſuppoſes all poſſible gradations. Theſe are evidently to be divided into two claſſes, thoſe in which the excitability is to a certain degree exhauſted by too great an application of ſtimuli,

* Appendix, Note 15.

ſtimuli, and thoſe in which a morbid accumulation is ſuppoſed to take place in conſequence of too great an abſtraction of ſtimuli. In the latter of theſe the body is ſaid to be in a ſtate of direct,* and in the former of indirect,† debility. Theſe are ſuppoſed to be its morbid ſtates.

It is evident, however, that there is a ſtate of body different from either of theſe, and different alſo from the healthy ſtate. When ſtimuli are too much abſtracted, debility is ſuppoſed immediately to enſue; but the immediate conſequence of too great an application of ſtimuli is not debility, but increaſed excitement. Dr. Brown, therefore, although he maintains that in the greater number of diſeaſes the ſyſtem is in a ſtate of debility,‡ yet admits that there are many in which it is in a ſtate of encreaſed excitement.§

The diſeaſes of excitement he terms ſthenic,

* Appendix, Note 16. † Note 17. ‡ Note 18. § Note 19.

ſthenic, thoſe of debility aſthenic. Theſe two claſſes include all general diſeaſes.

The foregoing principles Dr. Brown regarded as fully demonſtrated, and with a want of caution, altogether inexcuſable, founded on them his modes of practice.

As he allows exceſs of excitement to be a morbid condition of the body, he admits that an abſtraction of ſtimuli is in ſome caſes requiſite;* but he chiefly depends for the cure of diſeaſes on the addition of ſtimuli. On this, his plan of treatment in the diſeaſes both of direct and indirect debility is founded. In the former, we are taught that the morbid accumulation of excitability is to be reduced by a cautious application of more ſtimuli;† in the other, in which the excitability has been morbidly diminiſhed by too great an application of ſtimuli, that the cure is to be begun by a ſtimulus, but a little weaker

* Appendix, Note 20. † Note 21.

weaker than that which produced the diſeaſe, and the ſyſtem brought to the healthy ſtate, by gradually diminiſhing the quantity of ſtimulus employed; time being thus given for the excitability to be ſufficiently reſtored for the functions of the ſyſtem to go on, in conſequence of the application of the natural ſtimuli alone.*

Such is the ſyſtem of Dr. Brown, or rather that which may be collected from his *Elementa Medicinæ*, for, in order to give the reader a diſtinct view of it, I have been obliged to ſeparate the ſyſtem always aimed at, and often clearly expreſſed in the writings of Dr. Brown, from many of his opinions which have introduced much confuſion into this ſyſtem and often directly contradict its fundamental principles.

Although the opinions here alluded to are not to be regarded as any part of the

* Appendix, Note 22.

the Brunonian hypotheſis, ſince they are incompatible with it, as they have generally been thought to make part of this hypotheſis, it is neceſſary to ſay ſomething of them that my account of it may not appear defective.

One of the moſt ſtriking inconſiſtencies in the writings of Dr. Brown, I have juſt had occaſion to allude to—that every living body at the commencement of its exiſtence receives a certain quantity of excitability, which, if not extinguiſhed by violent ſtimuli, or by too great an abſtraction of ſtimuli, will laſt for a certain length of time. The quantity received, he ſuppoſes, determines the natural duration of life, it being impoſſible to protract it after that quantity is exhauſted.

"We know not (he obſerves) what "excitability is, or in what manner it is "affected by the exciting powers. But "whatever it be, whether a quality or a "ſubſtance,

" ſubſtance, a certain portion is aſſigned
" to every being upon the commencement
" of its living ſtate. The quantity or
" energy is different in different animals,
" and in the ſame animal at different times.
" It is partly owing to the uncertain na-
" ture of the ſubject, partly to the poverty
" of language, and partly to the novelty
" of this doctrine, that the phraſes of the
" excitability, being abundant, increaſed,
" accumulated, ſuperfluous, weak, not well
" enough ſuſtained, not well enough ex-
" erciſed, or deficient in energy, when
" enough of ſtimulus has not been em-
" ployed; tired, fatigued, worn out, lan-
" guid, exhauſted, or conſumed, when the
" ſtimulus has operated in a violent degree;
" at other times in vigour, or reduced to
" one half, when the ſtimulus has neither
" been applied in exceſs or defect; will be
" employed in different parts of this work."

It is almoſt unneceſſary to obſerve, that

that the different parts of the foregoing quotation are perfectly irreconcilable. The confusion which Dr. Brown here attributes to the poverty of language, and the novelty of the subject, arises from the most evident contradictions in his hypothesis. Dr. Beddoes justly observes, that "he who assumes "that a certain portion of excitability is "originally assigned to every living system, "by his very assumption denies its con-"tinual production, subsequent diffusion "and expenditure, at a rate equal to the "supply, or greater or less."

An inconsistency, if possible, more remarkable than this, is the supposition that both the above species of debility may exist in the same body at the same time. In paragraph CCXL. he observes, "As "debility, therefore, whether direct or in-"direct, or both conjoined, &c." and after pointing out the doses of medicines suited to his two species of debility, he observes, in

in DCXCI. " when the affection is more a " mixture of both ſorts of debility, theſe " proportions of the doſes muſt be blended " together." If direct debility be that ſtate in which the excitability is morbidly accumulated, and indirect debility that in which a morbid exhauſtion of it has taken place, in what ſtate ſhall we ſuppoſe the ſyſtem to be when both ſpecies of debility are conjoined! for, when the excitability and ſtimuli applied, are in due proportion, according to the hypotheſis of Dr. Brown, it is in a ſtate of health.

Nay, in ſome places, he ſeems to maintain that the ſame agent may, at the ſame time, induce both ſpecies of debility. In CXXXVII. he obſerves, " the ſame thing " is to be ſaid of exceſs of venery, which " is partly an indirect, partly a direct, " always a great, debilitating power."

Is it poſſible to ſuppoſe that ſleep ſhould ever be the conſequence of what

Dr. Brown calls direct debility, of that ſtate of the ſyſtem in which every agent produces a greater degree of excitement than in health! yet, in a variety of paſſages, direct debility is regarded as capable of producing ſleep.*

Although Dr. Brown maintains that the mode of action of every agent on the living body is the ſame (CCCXV. CCCXVI. CCCXVII. &c.), yet he is conſtantly obliged to admit that there is a ſpecific difference in the effects of different agents, (CCCI. CCCIX. &c.) Towards the end of the latter of theſe paragraphs, he obſerves, "when the excitability is worn out by "any one ſtimulus, any new ſtimulus finds "excitability, and draws it forth, and "thereby produces a further variation of "the effect." If the operation of all ſtimuli is the ſame, a new ſtimulus can produce no effect which may not be produced by

* Appendix, Note 23.

by changing the quantity of that firſt applied.

Are not the ſuppoſed ſtates of direct and indirect debility oppoſite conditions of body? Can we ſuppoſe them to produce preciſely the ſame train of ſymptoms? Yet Dr. Brown is conſtantly forced into this inconſiſtency, (CLXI. CC. CCXXXIV. &c.) In CC. he obſerves, "epilepſy depends likewiſe on debility, and the ſame "ſcantineſs of fluids, only here the debility "is *commonly* of the direct kind. Fevers "may ariſe from indirect debility, as in "the confluent ſmall pox, or where drunkenneſs has been the principal exciting "noxious power applied, but at the ſame "time the *moſt frequent* cauſe of fever is "direct debility."

Various ſimilar inconſiſtencies might be pointed out. In ſtating the hypotheſis of Dr. Brown, we muſt regard the ſpirit of his writings, and overlook the paſſages which

which cannot be reconciled to the doctrines in general maintained in them.

When we conſider the comprehenſive nature and ſimplicity of this hypotheſis, and that many of the facts on which it appears to reſt are ſuch as we every day experience in our own bodies, we may account for its having laid hold of the minds of all who were not wedded to former ſyſtems. On a cloſer view, however, it will be found, if we except that part of it which was univerſally admitted by Phyſicians before Dr. Brown's *Elementa Medicinæ* appeared, that, ſo far from being a legitimate deduction from facts, it is as unfounded as the hypotheſis of any other writer on this ſubject.

I ſhall follow the ſame mode of arrangement, in the following obſervations on Dr. Brown's ſyſtem, which has been adopted in giving an account of it.

All

All agents, according to this ſyſtem, are ſtimuli. Every thing capable of producing any change in the living body excites the action of the muſcular or nervous ſyſtem, or both; and, in proportion as it excites them, exhauſts their excitability. There is no agent, therefore, capable of diminiſhing the excitability, without occaſioning previous excitement. Let us conſider if this be a fact.

It ſeems, indeed, to be a law of the living ſolid, that every agent applied in a certain degree acts as a ſtimulus to it; and the ſubſequent exhauſtion is proportioned to the excitement it occaſions. Applied in other degrees, however, the ſame agent acts no longer as a ſtimulus. Diſtilled ſpirits received into the ſtomach, for example, occaſion excitement, and, to a certain extent, the greater the quantity the greater is the excitement; but the immediate effect of a large quantity of diſtilled

diſtilled ſpirits, ſuddenly received into the ſtomach, has often been inſtant death.

A ſmall quantity of opium applied to the heart occaſions increaſed excitement. A large quantity does not excite in it violent contractions, followed by the loſs of its excitability, but inſtant paralyſis, without any previous excitement.*

The firſt inſtance, then, in which this hypotheſis departs from truth, is, in ſuppoſing that every thing capable of acting on the living ſolid is a ſtimulus, and conſequently, that there is no agent which directly deſtroys the excitability.

The excitability, we have ſeen, is, according to Dr. Brown's ſyſtem, an uniform, indiviſible property of the living body, which cannot be increaſed or diminiſhed in any one part, without being affected in the ſame way, although in a leſs degree, in every other.

This

* Appendix, Note 24.

This is true of the excitability, when the term is confined to that of the nervous ſyſtem. As ſenſation and voluntary motion in every part of the body depend on the ſenſorium commune, and the excitability of this organ is affected by ſtimuli applied to any part of the ſyſtem, every part is affected in conſequence of the excitement of any one.

If we fatigue one limb, we find that we have, though in a leſs degree, diminiſhed the power of every other. The exerciſed limb is more debilitated than other parts of the body, becauſe it not only ſuffers in common with theſe, from the debilitated ſtate induced on the ſenſorium, but the excitability of its muſcles is alſo impaired in conſequence of the contractions excited in them; for contractions excited in the muſcles of voluntary motion, through the medium of the nervous ſyſtem, impair the excitability of the

the muſcles affected, without influencing that of any other.* The excitability of the nervous ſyſtem, then, may be ſaid to be indiviſible, the nervous excitability every where depending on the ſtate of one particular organ. But the muſcular excitability exiſts in every muſcle, independently of its exiſtence in any other.

If, for example, we apply a ſtrong ſolution of opium to the denuded muſcles of a limb, they are inſtantly deprived of their excitability, but that of every other muſcle of the body remains as entire as before the application of the opium. If an animal be killed by deſtroying the excitability of the heart, the excitability of no other muſcle is deſtroyed along with it. In every other part of the body the muſcular excitability is found as entire as after any other death equally ſudden.†

The difference in the laws of excitability

* Appendix, Note 25. † Note 26.

bility of the muſcular and nervous ſyſtems, Dr. Brown either overlooked, or could not prevail on himſelf ſo far to encroach on the ſimplicity of his ſyſtem, which he conſidered its chief excellence, as to acknowledge.

It was obſerved above, that between the healthy ſtate, in which the excitability and ſtimuli applied are in due proportion, and death, in which the excitability is extinguiſhed either by an exceſs or too great an abſtraction of ſtimuli, Dr. Brown ſuppoſes all poſſible gradations conſtituting the diſeaſes of direct and indirect debility.

According to Dr. Brown's hypotheſis, a ſtate of indirect debility is that in which the excitability is more or leſs exhauſted, and, conſequently, the ſame ſtimuli produce a leſs degree of excitement than in health.

We need only reflect on what we

obſerve in our own bodies to know, that, in proportion as we are ſubjected to the action of ſtimuli, we become leſs capable of being excited by them; and if their application is continued, the ſtrongeſt fail to rouſe to any further exertion till a ſtate of ſleep (during which, if it be found, there is the greateſt abſtraction of ſtimuli which is conſiſtent with health) has, to a certain degree, renewed the excitability. The application of the ordinary ſtimuli of life renders this ſtate, during a certain portion of the day, neceſſary to our exiſtence. We may delay ſleep by conſtantly applying a ſtronger ſtimulus than that which preceded it; but in proportion as it is thus delayed, it becomes the more irreſiſtible, and the longer its continuance muſt be, in order to reſtore the due degree of excitability.

On theſe facts, which it is unneceſſary to ſay were as well known before the publication

publication of the *Elementa Medicinæ* as at preſent, Dr. Brown has founded the whole of his hypotheſis, and has thus given to them an appearance of ſyſtem which they have not in the works of any other writer. They form, as far as I can judge, the only part of that hypotheſis which can be admitted; but he has ſo interwoven them with its other parts, that it requires ſome attention to make the ſeparation.

Sleep is the only ſtate, either healthy, or morbid, to which Dr. Brown's definition of indirect debility will at all apply; and here it applies only to thoſe organs on which the animal functions depend; for thoſe on which the vital and natural functions depend are never in a ſtate of exhauſtion. During ſleep, theſe organs ſuffer no diminution of vigor but what is the conſequence of the ſuſpenſion of the animal functions.

The

The ſtate of the heart and blood veſſels, after each ſyſtole, is not that of exhauſtion, as exhauſtion is defined by Dr. Brown; nor does he, indeed, ſuppoſe it to be ſo. That it is not a ſtate of exhauſtion might be demonſtrated in various ways. It is ſufficient, however, to obſerve, that the exhauſted excitability can never be reſtored while the ſtimulus which exhauſted it continues to act. A man will never recover from the fatigue of a long walk while he continues walking; nor will the retina recover its ſenſibility while expoſed to the ſame degree of light which impaired it. But the contractions of the heart continue to recur, although it is expoſed to the uninterrupted action of the agent which excites them; for, as I have frequently obſerved in frogs, if a ligature be thrown round the aorta, ſo that the heart continues uniformly gorged with blood, its fibres are ſtill alternately contracted

tracted and relaxed, and, for the ſpace of four or five minutes, with the ſame frequency as before the ligature was applied. Thus, if we ſprinkle ſalt upon a muſcle, we do not produce permanent contraction, followed by exhauſtion, but alternate contractions and relaxations, followed by exhauſtion. But the ſtate of the muſcle, in the relaxation which intervenes between the contractions, is eſſentially different from its condition in that relaxation which ſucceeds them; becauſe, in the former caſe, the ſame agent, although its application has not been interrupted, is ſtill capable of exciting the muſcle to action.

The vital and natural functions are the powers of aſſimilation, by which the excitability of thoſe parts of the ſyſtem which ſuffer exhauſtion is renewed. If theſe powers alſo ſuffer exhauſtion, to what powers in the animal ſyſtem ſhall we attribute the renewal of their excitability!

From

From the nature of exhauſtion, it is impoſſible that any exertion of theſe powers themſelves can renew it. Every thing which calls them into action muſt further diminiſh their excitability.

It is this objection to the hypotheſis of Dr. Brown which forced him into the ſuppoſition, that a quantity of excitability, which is to laſt through life, is beſtowed on every animal at the commencement of its exiſtence. In forming his hypotheſis he could not but perceive that, admitting every part of the ſyſtem to be in a ſtate of exhauſtion, there is no power inherent in it capable of reſtoring its excitability. This formidable objection he found it neceſſary to get rid of, although at the expence of introducing into his ſyſtem the moſt evident inconſiſtency.

Beſides, it is impoſſible that the excitability of the organs on which the vital and natural functions depend, can be renewed,

renewed, while the ſame agents which occaſioned its exhauſtion continue to be applied. If the organs on which the vital and natural functions depend, were ſubject to the ſame exhauſtion from the ſtimuli that excite them, which takes place in thoſe of the animal functions from their uſual ſtimuli, no animal could exiſt above a few hours.

The excitability of the organs on which the animal functions depend is renewed during ſleep, becauſe the ſtimuli which occaſion the exhauſtion are then withdrawn, and the powers of life remain unimpaired. But if the natural ſtimuli occaſioned exhauſtion in the organs on which theſe powers depend, as the ſtimuli are never withdrawn, would not their uninterrupted application, a fortiori, prevent any renewal of the excitability.*

In diſeaſe, indeed, the organs of the vital

* Appendix, Note 27.

vital and natural functions are often debilitated; but this debility we ſhall find of a nature ſo different from exhauſtion, that excitement is a means of removing it.

What Dr. Brown ſays of the action of ſtimuli in removing exhauſtion, appears to be altogether unfounded; the excitability of thoſe organs on which the animal functions depend, the only part of the ſyſtem ſubject to exhauſtion, in the ſenſe in which he uſes this term, is moſt ſpeedily and effectually reſtored during the abſence of every ſtimulus which excites them.

Direct debility, according to Dr. Brown, is a ſtate in which the excitability is accumulated, and conſequently every ſtimulus produces a greater degree of excitement than in that condition of the ſyſtem which, alone, he allows to be a ſtate of health.

The

The firſt queſtion which here preſents itſelf is, what is that condition of body which he admits to be a ſtate of health? How ſhall we define that ſtate in which he ſuppoſes the excitability and ſtimuli applied to be in due proportion?

In the morning the quantity of excitability is proportionably great; in the evening it is ſmall; and between theſe there are infinite gradations. Which of theſe ſtates does Dr. Brown conſider that of perfect health?

From a review of his ſyſtem, it appears, that wherever there is any addition made to the ſtimuli neceſſary for preſerving a ſtate of health, a morbid degree of excitement takes place, and a condition of body, differing only in degree from the moſt morbid, follows. However trifling the degree of morbidity may be, according to the hypotheſis of Dr. Brown it is a morbid ſtate; and, conſequently, we are

only in perfect health on awaking from found ſleep.

But what ſhall we ſay of the doctrine of direct debility, if it can be ſhewn, which it is ſurely very eaſy to do, that this is the ſtate of the ſyſtem in which ſtimuli of every kind produce the greateſt degree of excitement, that in which the excitability is accumulated in a greater degree than in any other? If it can be ſhewn, that Dr. Brown's definition of direct debility applies only to that ſtate which, according to his ſyſtem, is the only ſtate of perfect health?

We come now to the application of the hypotheſis of Dr. Brown, to explain the phenomena and treatment of general diſeaſes; which, we have ſeen, turns wholly on the ſuppoſition that, in all diſeaſes of the whole ſyſtem, except thoſe of encreaſed excitement, concerning the general nature of

of which there is no diſpute, the body is in one of thoſe ſtates which he terms direct and indirect debility.

On reviewing all that he ſays on the diſeaſes of direct debility, I can find but one fact on which his opinions are founded; and this, although at firſt view favorable to theſe opinions, if fairly examined, will not be found to afford any argument in ſupport of them; namely, that the animal body is ſtrongly affected by certain agents, food and heat, when they have not, for a conſiderable time, been applied in the uſual degree.

The mildeſt food will often deſtroy life in animals who have faſted for ſeveral days; and a moderate temperature will produce violent effects on thoſe who have been expoſed to a great degree of cold.

Of theſe ſtates of body, ſuppoſing them in the extreme, it may be obſerved,

in

in the firſt place, that neither heat in the one caſe, nor food in the other, occaſion excitement, which, were the excitability accumulated, as Dr. Brown ſuppoſes, they ought to do.

But, admitting that the effects of the agents are what they ought to be according to the hypotheſis of direct debility, they will go but a ſhort way towards eſtabliſhing it. A ſtate of direct debility is that in which all agents occaſion a greater degree of excitement than in health. Do all agents produce a greater than ordinary excitement in an animal that has been expoſed to hunger or cold? Inſtead of applying heat* to the animal that has been expoſed to cold, and giving food to that which has faſted, let food be given to the former, and let the latter be expoſed to heat. According to Dr. Brown, violent excitement ſhould ſtill, in both caſes, be the

* Appendix, Note 28.

the conſequence. In both, the excitability is ſuppoſed to be accumulated; and to both powerful ſtimuli are applied.

But ſo much the reverſe is the fact, that heat, in thoſe who have faſted and are ſtill deprived of food, and food, in thoſe who have been expoſed to cold, and are ſtill deprived of a due degree of heat, occaſion leſs excitement than in health. The truth is, that in animals under the operation of hunger or cold, every agent, except that whoſe application in the uſual degree has been interrupted, produces leſs powerful effects than in ordinary ſtates of the ſyſtem.

The effects of every agent are, of courſe, proportioned to the change it induces on the body. It is not difficult, therefore, to account for the effects of a temperature of 100° being more remarkable in an animal which has been expoſed to that of 10°, than in one that has been expoſed

expoſed to a temperature of 60°; or for a hearty meal producing more powerful effects on one that has faſted for ſeveral days, than on one that has faſted for a few hours, although expoſure both to hunger and cold tends to exhauſt the excitability, which we know to be the caſe, from the effects of other agents. It is well known, that a quantity of fermented liquor, which proves a ſtrong ſtimulus in a temperature of 50°, may be taken in a temperature of 15° without producing any marks of excitement; yet, in the latter caſe, according to Dr. Brown's hypotheſis, an accumulation of excitability has taken place.

According to this hypotheſis, ſenſation is one of the effects of ſtimuli. Are there any ſenſations more acute than the pains of hunger and cold? The ſenſation of cold, it is true, is occaſioned by the abſtraction of heat; that of hunger by the want

want of food; but theſe ſenſations are not the leſs real, and are followed by the ſame exhauſtion which ſucceeds powerful ſenſations from any other cauſe.

The few facts on which the hypotheſis of direct debility is founded, are referable to a law of the animal œconomy, by which the body is rendered more ſenſible to the action of agents, in proportion as it has been previouſly leſs expoſed to them. A perſon who has long indulged in indolence is overcome by the ſlighteſt exertion; but a high temperature, or a full meal, occaſions no greater excitement in him than in thoſe accuſtomed to exerciſe.

We have inſtances of the ſame kind in the effects of the various natural agents which excite the different organs of ſenſe, light, noiſe, ſapid and odoriferous bodies. An animal which has lived long in the dark is ſtrongly affected by light: One which has been long accuſtomed to quiet-

neſs

neſs is diſturbed by the leaſt noiſe: The palate that has been habituated to inſipid articles of diet is ſtrongly affected by animal food: And the inhabitants of a pure atmoſphere are ſenſible of odours which are unperceived by thoſe of a large city. But in none of theſe inſtances is the body rendered more ſenſible to any other agent, than that whoſe application, in the uſual degree, has been interupted.

The hypotheſis of direct debility ſuppoſes, that the abſtraction of any one of theſe agents renders the ſyſtem more ſenſible to every agent, and that the effect of all is, at all times, excitement.

The facts are, that the abſtraction of any one of theſe agents only renders the body more ſenſible to the action of that agent; and the effect of that agent is not always excitement, but either excitement or atony, according to the degree in which it is applied, and the ſtate of the body at the

the time of its application, that is, according to the change it induces.

If the change is moderate, it proves a ſtimulus; and, within a certain range, the greater the change the greater is the excitement. Beyond this, as we have ſeen in the inſtances of opium and diſtilled ſpirits, it occaſions debility; and, when exceſſive, death.

When the change induced is conſiſtent with the health of the parts on which the agent acts, excitement is the conſequence; but when the change is ſufficient to derange the mechaniſm of the living ſolid, if I may uſe the expreſſion, its immediate effects are debility or death. Nor is this more remarkable of the agents which are directly applied to the living ſolid, than of thoſe whoſe firſt impreſſion is on the mind. The paſſions, within a certain degree of intenſity, act as ſtimuli; beyond this they debilitate, and even extinguiſh life, without previous excitement.

The degree of exhauſtion which follows the operation of any agent is always proportioned to the excitement it occaſions; but the degree of atony which a greater quantity of the ſame agent produces bears no proportion to its exciting power. Thus tobacco will not occaſion the ſame degree of excitement which opium or diſtilled ſpirits do, but it is better fitted to produce atony.

Of thoſe agents whoſe firſt impreſſion is on the mind, ſome, grief, fear, diſguſt, are ill calculated to excite, although, when preſent only in a ſmall degree, they act as ſtimuli; but they are chiefly calculated to produce atony; others, love and joy, on the contrary, produce much excitement, and only occaſion atony when in exceſs.

With reſpect to what Dr. Brown ſays of the depreſſing paſſions, as it makes a part of his hypotheſis of direct debility, it

it muſt fall with that hypotheſis: unleſs we allow that grief, fear, &c. occaſion an accumulation of excitability, there is nothing which Dr. Brown ſays on this ſubject that can be admitted. According to a law of the animal œconomy, I have juſt had occaſion to mention, thoſe under the operation of grief are rendered more ſenſible to joy, and thoſe under the operation of fear to confidence; but they are rendered leſs ſenſible to the operation of every other agent. His aſſertion that grief is only a leſs degree of joy, and fear nothing more than a diminution of confidence, is quite gratuitous. He might with equal reaſon aſſert, that confidence is a diminution of fear, and joy a leſs degree of grief. The one ſet of paſſions are as poſitive agents as the other; and if the one tend more to excite, and the other to depreſs, it is only what is true of agents of every other ſpecies.

The

The morbid ſtates above conſidered, namely, thoſe ariſing from the effects of cold and hunger, are the ſuppoſed ſtates of direct debility, of which Dr. Brown ſpeaks with moſt preciſion. It is unneceſſary particularly to conſider any of the others, becauſe one obſervation applies to all of them; there is none in which many agents do not produce as little or leſs excitement than in health, therefore there is none in which the excitability is accumulated.

With regard to the diſeaſes in which Dr. Brown ſuppoſes the ſyſtem to be in a ſtate of diminiſhed excitability, a ſimilar obſervation applies to them; there is no general diſeaſe in which the ſyſtem is rendered leſs ſenſible to the action of all agents. In typhus, it is leſs ſenſible to that of opium and wine; but a degree of exerciſe or heat, which would not incommode

mode us in health, is, in this diſeaſe, often capable of deſtroying life.

The diſeaſes which Dr. Brown terms ſthenic, and which conſiſt, in a morbid degree, of excitement, he falls into the error of regarding as only a greater degree of the ſame excitement which prevails in health, in the ſame way as he ſuppoſes the diſeaſes of indirect debility only a greater degree of the ſame exhauſtion which produces ſleep. But as the latter does not bear the character of exhauſtion; neither does the former, which is always followed by atony, bear the character of healthy excitement.

That excitement only is healthy which occaſions a tendency to ſleep. The debility it produces, we have ſeen, affects only the organs on which the animal functions depend. Whether theſe organs are in a ſtate of excitement or exhauſtion, the

powers

powers by which the body is preſerved are equally unimpaired. The latter cannot partake of the alternation of vigilance and ſleep.

Morbid excitement, on the other hand, debilitates the vital as well as the animal functions. Every part of the ſyſtem is enfeebled, and, inſtead of the organs on which the animal functions depend becoming uniformly leſs ſenſible to the various ſtimuli which excite them, they become preternaturally ſenſible to thoſe ſtimuli, while the vital organs become proportionably leſs ſenſible to the action of their ſtimuli; and the conſequence, as might have been foretold, inſtead of ſleep, is a painful and reſtleſs watchfulneſs. Theſe circumſtances are here ſimply ſtated as facts; the manner in which they are to be explained will be conſidered in the next Chapter.

Upon

Upon the whole, the following, as far as I am capable of judging, are the facts which Dr. Brown overlooked in forming the great outlines of his hypothesis.

There is no accumulation of excitability beyond that which constitutes a state of the most perfect vigour. There is no exhaustion of excitability, in the sense in which Dr. Brown uses the term, beyond that which constitutes the most perfect sleep, and both are equally states of health.

Every agent is capable of producing either excitement or atony, according to the degree in which it is applied.

In health, the natural agents applied in the usual degree, viz. a certain temperature, a certain quantity of exercise, &c. always occasion that kind of excitement which is followed by exhaustion.

In general disease, that is, in fever, which is the only general disease properly so

ſo called,* the ſtate of the excitability is ſo changed, that the ſame agents do not produce a greater or leſs degree of the ſame effects they produce in health, as Dr. Brown ſuppoſes; but either atony, or that kind of excitement which is followed by atony.

It muſt appear, I think, to every one who attentively conſiders the hypotheſis of Dr. Brown, that its author, in ſpeaking of diſeaſes, has conſtantly in view the healthy ſtate of the animal body; and attempts, in vain, to apply the laws which regulate the excitability of certain parts of the ſyſtem in health, to explain the phenomena of diſeaſe.

CHAP.

* Appendix, Note 29.

CHAP. II.

Of the Proximate Cauſe of Fever.

THE functions of the animal body have been divided into three claſſes; the vital, the natural, and the animal.

There is an evident impropriety in this diviſion. While the nature of the two firſt ſets of functions differs ſo eſſentially from that of the third ſet, that nothing can be more evident than the line of diſtinction; the vital and natural functions ſo run into each other, that it is impoſſible to draw any preciſe line between them.

The objection is ſtrengthened, when we conſider the offices of the different ſets of functions. By the animal functions, the individual is connected with the external world; by the vital and natural, his own exiſtence is preſerved.

We again ſee the impropriety of the diviſion, if we turn our attention to the ſources from which the organs of theſe functions immediately derive their power. The animal functions depend immediately on the brain and its appendages. The vital and natural functions, though influenced by the ſtate of the nervous, depend immediately on the ſanguiferous ſyſtem, and are perfectly performed where the brain has never exiſted, as in the caſe of fœtus born without the head.

It may alſo be obſerved, that of the vital and natural functions the animal is unconſcious; of the animal functions he is not only conſcious, but many of them are ſubjected to his will.

The organs of theſe different ſets of functions, we have ſeen in the preceding Section, poſſeſs different kinds of excitability ſuited to the purpoſes they ſerve. Thoſe of the vital and natural functions

on

on which life depends, poſſeſs an excitability which never ſuffers any diminution except in diſeaſe, nor complete exhauſtion except in death. Thoſe of the animal functions which ſuffer intermiſſions that they may relieve the vital organs on which their action depends, poſſeſs an excitability which ſuffers a conſtant alternation of exhauſtion and renewal; for it is only for a certain length of time that the vital can, without interruption, ſupport the action of the animal organs.

The vital and natural functions, then, are ſimilar in their nature; have for their object the preſervation of life; are performed independently of the will or conſciouſneſs of the animal; and immediately depend on organs which poſſeſs an excitability that continues unimpaired through life.

The animal functions are not concerned in the preſervation of life. They have

have for their object to connect the animal with the external world; are subjected all to the consciousness, many to the will, of the animal; immediately depend on organs of which the other functions are independent, and which possess an excitability subject to a constant alternation of exhaustion and renewal.

The division of vital and natural functions, therefore, if adopted at all, can be regarded only as a subdivision. But it will be found more convenient, I believe, wholly to set aside this division, which seems to be of little use, and divide the functions into those which are necessary for the preservation of life, which we may call *vital*, and those by which the animal is connected with the external world, which, for want of a more appropriate term, may be called *animal*.

The one set of these functions is evidently designed for the support of the other.

other. The end of animal life is to feel and communicate enjoyment: The animal functions, or, to ſpeak more accurately, their immediate organs (which, to ſave repetition, I ſhall call *animal organs*, in contradiſtinction to the organs properly called *vital*), are the immediate inſtruments of both; and they are maintained by the vital organs, whoſe office is to form and nouriſh them, and, when their vigor is exhauſted by the impreſſions which excite them, ſo that they ceaſe to act, to refit them for receiving and anſwering thoſe impreſſions.*

It is evident, therefore, that when the vital organs are debilitated, the animal organs neceſſarily partake of the debility. It is equally evident why the converſe of this is not true, the nervous ſyſtem may be debilitated without affecting the powers of circulation. Thus when any cauſe impedes

* Appendix, Note 30.

pedes the action of the heart, the brain immediately partakes of the diſorder; but even in apoplexy the pulſe is generally ſtrong and good, and only becomes ſlower and more languid in proportion as the reſpiration, which is performed by muſcles that depend for their vigour on the influence of the brain, becomes ſo.*

Thus it is that in diſeaſes properly termed nervous, the pulſe is generally good, however great the languor and depreſſion of ſtrength may be; nay I have even found, from experiments made for the purpoſe of aſcertaining this point, that the total deſtruction of the nervous ſyſtem produces no immediate effects on the action of the heart, nor in any other way affects its motion than neceſſarily happens in conſequence of the interruption of reſpiration.†

The heart is poſſeſſed of little ſenſibility,

* Appendix, Note 31. † Note 32.

bility, ſo little indeed that abſceſſes and ulcers are often formed in it without giving any pain;* and is, conſidering the ſize and importance of the organ, ſcantily ſupplied with nerves, the uſe of which ſeems to be to convey from the ſenſorium to the heart certain impreſſions, not to give vigor to an organ which we find in a ſtate of perfect vigor, where no ſenſorium has exiſted.†

It appears from what has been ſaid in the preceding part of this Eſſay, that the terms excitement and exhauſtion, in the ſenſe in which Dr. Brown uſes theſe terms, apply only to the animal functions, and are merely expreſſions for the healthy ſtates of vigilance and ſleep. There is no debility which can be induced on the animal organs while the vital retain their vigor, which is to be regarded as morbid, becauſe, under ſuch circumſtances, the vigor

* Appendix, Note 32. † Note 33.

vigor of the former is always reſtored by ſleep.

But if the excitement of the animal organs be continued till it occaſions debility in the vital organs, which ſupport it, a ſtate very different from exhauſtion is the conſequence. The ſteps by which exhauſtion is changed into this ſtate, which may properly be termed atony, are eaſily traced.

A certain excitement of the animal functions continued for a certain length of time, occaſions a tendency to ſleep. This tendency increaſes as the excitement is protracted, as long as the ſanguiferous ſyſtem properly performs its functions. But if the excitement is continued beyond this point, a degree of reſtleſsneſs comes on. Inſtead of a healthy action of the heart and arteries, their pulſations gradually become more feeble and frequent, and the various functions, all of which depend more

more or leſs directly on the ſanguiferous ſyſtem, ſhew a correſponding debility.

The tendency to ſleep at length ceaſes, ſleep being no longer capable of reſtoring vigour. It is in vain to abſtract light from the eyes and give reſt to the limbs, the powers which ſhould renew the excitability of the animal organs are themſelves in want of refreſhment.

The only means of reſtoring theſe powers to a due action, is applying to them a ſtimulus ſtronger than the natural ſtimulus, in proportion as their excitability is leſs than it ought to be; for the ſyſtem, it is evident, poſſeſſes no organs by which the vigour of thoſe of aſſimilation, that is, of the vital organs, can be reſtored.

The application of a preternatural ſtimulus to them, is the unavoidable conſequence of their debility: for the ſecreting organs no longer performing their functions, the more irritating and noxious

parts of the blood, which ought to be expelled from the body, are retained, and ſoon excite the heart and blood veſſels to an action as powerful as the healthy action, often more ſo.

It is to be recollected, that all debilitating cauſes applied to the vital organs are felt moſt in thoſe parts which are leaſt vigorous, that is, at the greateſt diſtance from the heart.*

Theſe, to avoid circumlocution, I ſhall call the circumference of the vital ſyſtem; the heart and larger veſſels I ſhall call the centre. Of the former are compoſed the different ſecreting organs. Hence debility of the vital ſyſtem is firſt indicated by a failure of the various ſecretions.

If the increaſed action of the heart and larger veſſels, as frequently happens, by applying an increaſed ſtimulus, reſtores vigour to the circumference of the vital ſyſtem,

* Appendix, Note 34.

ſyſtem, the action of the ſecreting organs will be renewed, the offending matter expelled, and health reſtored. If not, the preternatural excitement of the heart and larger veſſels muſt neceſſarily go on till the debility extends to them; and there being now no longer any means of excitement in the ſyſtem itſelf, if the debility induced be conſiderable, the animal dies. Death may frequently be prevented by exciting the heart and arteries by artificial means, until they have reſtored power to the capillaries.*

I have been conſidering the effects of too violent or long continued excitement of the animal organs. It is evident, however, that ſimilar effects may be produced in two other ways. If we ſuppoſe a noxious power received into the ſanguiferous ſyſtem capable of debilitating the heart and

* Appendix, Note 35.

and blood veſſels, whether previous excitement of the animal organs has taken place or not, the ſame want of vigour in the excretories, and encreaſed action of the heart and blood veſſels, will enſue. Theſe ſymptoms, and their conſequences, may alſo take place without any debilitating power applied to the heart and blood veſſels; for theſe remaining in their natural ſtate, if any cauſe impede the proper action of the excretories, the ſame encreaſed action of the ſanguiferous ſyſtem will take place, but it will not be preceded by equal marks of debility, but only by thoſe which attend the failure of the excretions.*

It appears, then, that when a debilitating cauſe is applied to the vital ſyſtem, the extreme parts of this ſyſtem looſe their tone; that in conſequence of this, ſecretion being impeded, a preternatural ſtimulus

* Appendix, Note 36.

lus is applied to the heart and larger veſſels, which, by exciting them, tends to reſtore tone to the capillaries, in the ſame way that an encreaſed action of the larger veſſels of an inflamed part tends to reſtore tone to the capillaries of that part.* On this principle, I believe, the whole phenomena of fever may be explained; to eſtabliſh this poſition, it will be neceſſary to review theſe phenomena, which I ſhall do under the three heads of the Symptoms, Cauſes, and Cure of Fever.

Of the Symptoms of Fever.

The ſymptoms of fever may be divided into thoſe of increaſed excitement and thoſe of debility; and each of theſe ſets of ſymptoms may be ſubdivided according as they indicate derangement in the vital or animal organs.

As

* Appendix, Note 37.

As the ſtate of the animal organs depends on that of the vital, we find, provided there is no diſeaſe which immediately influences the former, that the excitement of the one ſet of organs always correſponds with that of the other. If the vital organs are debilitated, the animal organs partake of the debility; if the vital organs are preternaturally excited, the animal organs are affected in a correſponding manner. When this coincidence is obſerved then, and there is no other evident cauſe affecting the animal organs, we infer that the vital organs are primarily affected, becauſe their affection is capable of producing the whole phenomena.

At the commencement of fever, when the pulſe becomes weak, ſmall, frequent, perhaps irregular; when the breathing is feeble, frequent, and interrupted with ſighing; when the features and other extreme parts ſhrink; when the various

ſecretions

ſecretions begin to fail, the tongue becoming clammy, the throat dry and rough, the appetite being impaired* with encreaſed thirſt and conſtipated bowels, the urine limpid and in ſmall quantity, and the ſkin cold, pale, dry and ſhrivelled; the patient, at the ſame time, complains of a general ſenſe of debility; the limbs totter, the tongue trembles, the mind becomes feeble, unſteady, and anxious, the ſtrength and acuteneſs of the different organs of ſenſe are impaired, the ſight wavers, the hearing is confuſed, the ſmell and taſte often loſt, and the feeling indiſtinct, a degree of numbneſs frequently affecting the limbs, and a ſenſe of cold and creeping being referred to various parts of the body. Such are the ſymptoms with which fever makes its attack—thoſe of an enfeebled circulation with the various conſequences, which we could have foretold would attend it.

By

* Appendix, Note 38.

By degrees we ſee this ſtate of general debility changing to one of a different nature. After the various excrementitious matter, which ſhould have been thrown out of the body, has for ſome time been retained, the pulſe begins gradually to increaſe in ſtrength, and the breathing becomes fuller and leſs feeble; the blood is now impelled with vigour into the extreme veſſels; the paleneſs, ſhrinking, and coldneſs of the ſkin, is ſucceeded by turgescence, redneſs, and heat; inſtead of the feebleneſs which attends the firſt ſtage, the muſcles acquire a preternatural vigour; the ſenſibility, inſtead of being impaired, is now morbidly acute;* and various other ſymptoms, denoting an increaſed force of circulation, gradually ſhew themſelves.

This general effort of the ſyſtem reſtores the vigour of the ſecreting organs, and all that ſhould have been gradually ſeparated

* Appendix, Note 39.

ſeparated from the blood is now forced off at once by the alimentary canal, kidneys and ſkin. Looſe ſtools frequently occur; the urine becomes copious, and is loaded with the peculiar ſalts which the kidneys ſecrete from the blood, while the ſkin is bathed in ſweat, highly charged with the ſaline ſubſtances thrown off by this organ. Thus the ſtimulus which rouſed to preternatural action the central parts of the ſanguiferous ſyſtem, by which vigour was reſtored to its circumference, is removed, and health reſtored.

A degree of general debility, it is true, prevails, but the action of the extreme veſſels is ſo far reſtored, that it is in due proportion to the remaining vigour of the heart and larger veſſels. In ſhort, what now takes place throughout the ſyſtem is analogous to what happens during reſolution in an inflamed part. The extreme veſſels are capable of effecting the

neceſſary changes in the fluids ſupplied to them by the larger veſſels. Thus even the general debility which ſucceeds fever is a wiſe proviſion of nature; and thus it is that the debility of the capillaries, and along with it the fever, is often renewed for ſome time after it has ceaſed, by too full a diet, exerciſe, or any other cauſe that increaſes too much the force of the heart and larger veſſels, and thus throws on the capillaries a larger quantity of the fluids than on the firſt return of their vigour they can eaſily bear. A full meal, under theſe circumſtances, is often ſucceeded by a ſtrong pulſe, and a dry ſkin.

Such is the courſe of a ſimple attack of fever; and ſurely no train of ſymptoms appears more ſimple, or admits of a more eaſy explanation on the beſt eſtabliſhed laws of the animal œconomy. Let a debilitating power (ſuch as we know many of the cauſes of fever to be) be applied to

to the vital organs, all the foregoing train of ſymptoms neceſſarily follow, unleſs the increaſed action of the heart and larger veſſels fails to rouſe to their natural action the extreme parts of the circulating ſyſtem, that is, to reſtore the due vigour of the ſecreting organs.

Various ſymptoms, then, gradually unfold themſelves, all of which, we ſhall find, are evident conſequences of want of vigour in thoſe organs. Theſe, like the ſymptoms of the more ſimple form of fever, may be divided into thoſe of encreaſed excitement and thoſe of debility.

When the debility of the extreme veſſels is ſuch, that the encreaſed force of circulation excited by the retention of what the excreting organs ſhould have thrown out of the body, is unable to reſtore their action, the preternatural excitement of the heart and larger veſſels muſt go on till they themſelves fall into a ſtate of debility.

The

The period at which this debility ſupervenes, and the violence of the excitement which precedes it, depend on the vigour of the habit, and, as we ſhall preſently ſee more particularly, on the nature of the cauſe which produces the fever.

It hardly ever happens, however, that the encreaſed action of the heart and larger veſſels is wholly ineffectual. In the ſpace of a few hours they rouſe to a more or leſs perfect action the various excretories, in conſequence of which part of the irritating cauſe is thrown off, and a mitigation of the ſymptoms follows. But on the abatement of the action of the central parts of the ſanguiferous ſyſtem, the capillaries again falling into a ſtate of debility, the accumulation of what ſhould be excreted again takes place, and the various ſymptoms of excitement are renewed. Thus fevers, which laſt more than a day, conſiſt of a ſeries of remiſſions and exacerbations.

As

As the debility of the extreme veſſels is overcome with more difficulty the more frequently it returns, the excitement, during the exacerbations for the firſt days, gradually encreaſes in violence, till by degrees the excitability of the heart and larger veſſels being impaired, the ſymptoms of excitement gradually give place to thoſe of debility.

The ſymptoms of excitement, in continued fevers, differ only in degree from thoſe of a ſimple paroxyſm. The ſymptoms of debility differ in kind as well as in degree.

As the paroxyſms recur, the pulſe becomes ſtronger and more rapid, the reſpiration more hurried and frequent, the heat greater, the face fuller and more florid, the eyes more turgid with blood, and incapable of bearing the light, and the headach

headach, throbbing of the temples, and tinnitus aurium more troubleſome,* and from the greater heat, and the increaſing debility of the ſecreting organs, the ſkin, mouth and throat more arid.

Delirium much leſs frequently ariſes from exceſs of excitement than from debility, and in the firſt paroxyſm of fever the excitement is ſeldom ſuch as to produce it. As the excitement encreaſes, however, in the ſucceeding paroxyſms, it ſometimes ſupervenes, and then riſes to a degree beyond what we uſually meet with in debilitated ſtates of the ſyſtem, the patient becomes frantic, and is with difficulty retained in bed.

In violent excitement it is not unuſual for ſome of the ſmaller veſſels to be ruptured by the exceſſive action of the heart and larger arteries; hemorrhagies thus occur from the noſe, ears, lungs, rectum or uterus,

* Appendix, Note 40.

uterus, and leſſen the excitement. It may be remarked, that when the excitement is conſiderable, we rarely meet with hemorrhagy from the eyes, ſkin, bowels, or urinary paſſages, from all of which they are ſo common in ſtates of extreme debility.

The ſymptoms of excitement are few and ſimple; thoſe of debility numerous and complicated. One cauſe of which is that exceſſive excitement never continues long. However powerful the exciting cauſes may be, the excitement ſoon arrives at that point beyond which the powers of the ſyſtem cannot raiſe it. Debility, on the contrary, is of long continuance; during its influence there is time for various changes to take place.

The pulſe, from the leſſened tenſion and weaker pulſation of the heart and arteries, becomes ſoft, ſmall, weak, and more frequent. It is not difficult to perceive

ceive the final cauſe of the beats of the heart generally becoming more frequent when they are enfeebled, for the feeble ſyſtole not propelling the ſame quantity of blood, or not propelling it with the ſame velocity, a more frequent ſyſtole becomes neceſſary in order to ſupport a due vigour of circulation.

As the debility encreaſes the pulſe becomes irregular, as well as weak. The enfeebled heart now and then refuſes to be rouſed to any action, ſo that an intermiſſion of the pulſe takes place, or it is rouſed only to a tremulous and irregular action, occaſioning a fluttering pulſe, or its action is languid and unequal, and the pulſe becomes undulating.

As the debility encreaſes, the beats of the heart at length fail to rouſe to action the more diſtant arteries, themſelves in a ſtate ill calculated to obey the ſtimulus. The pulſe in the extremities then ceaſes, and

and they become cold, and the coldneſs gradually extends, till the heart ceaſing to beat, it ſoon becomes general.

The ſtate of the breathing correſponds with that of the circulation. It is weak, frequent, and interrupted with ſighing; but when, from the encreaſing debility of the heart and blood veſſels, the rapidity of the circulation is much reduced, it often becomes ſlower than natural, being now and then interrupted for a conſiderable time.* It becomes difficult from the feebleneſs of the muſcular powers which ſupport it, and often rattling, from relaxation of the exhaling and inactivity of the abſorbing ſyſtem.

Under theſe circumſtances we are not ſurpriſed to find the voice low, weak, wheezing, and often ſhriller or hoarſer than uſual.

The ſecreting and abſorbing powers in every part of the body neceſſarily partake of the ſtate of the circulating ſyſtem.

* Appendix, Note 41.

In proportion as theſe powers are diminiſhed, the various ſecreted fluids become ſubject to the ſpontaneous changes which they undergo when uninfluenced by the contact of a living ſurface, by which the noxious parts being continually abſorbed, and new parts ſecreted in their ſtead, they are preſerved in a ſtate of health.

Hence, in fevers of ſome continuance, the mouth is clammy, and the thinner parts being carried off by the air, its mucus becomes tough, brown, fœtid, ſometimes black, and adheres firmly to the teeth, lips, and other parts of the mouth. In the advanced ſtage, if the tongue is not covered with brown or black mucus, it often appears of a ſhining dark red or purpliſh colour, from the debility and conſequent relaxation of its extreme veſſels; in either caſe it frequently becomes dry, and deep chaps are formed in it, the ſpeech being rendered inarticulate.

The

The mucus of the trachea and bronchiæ is ſubject to the ſame changes, rendering the breath dry, hot, and offenſive.

A conſiderable change happens in that of the œſophagus, deglutition being often impeded by the dryneſs of the paſſage, as well as the debility of its muſcles; and a failure of ſecretion is indicated in the ſtomach and bowels by thirſt and coſtiveneſs.

In all theſe appearances, however, there is conſiderable variety; for when the extreme veſſels are greatly relaxed, the thinner fluids flow from them copiouſly. Thus the mouth is often moiſt to the fatal termination; and an ichorous, or even bloody diarrhœa frequently ſupervenes, the relaxation being ſo great as to permit the paſſage of the red particles, in the diſſolved ſtate in which they now exiſt in the blood, the conſequence of there being no renewal of the component parts of this fluid, the

powers

powers of abſorption and ſecretion being ſuſpended.

The ſame ſtates of the veſſels are indicated by correſponding appearances of the urine. It is high-coloured and ſcanty, or more copious, ichorous, or bloody. The ſkin, in like manner, is dry and contracted, or relaxed and moiſt; and blood is ſometimes mixed with the ſweat.

All the hemorrhagies which occur in the advanced ſtage of typhus are of the ſame nature with thoſe juſt mentioned, and are, for the moſt part, ſymptoms of great danger, both as indicating great debility of the circulation, and as the means of encreaſing that debility. In the worſt caſes of typhus, indeed, blood, or rather a thin ſerum in which more or fewer of the red globules are broken down and ſuſpended, ſometimes runs from almoſt every ſurface of the body, whether internal or external.

When blood thus extravaſated is retained

tained beneath the cuticle, it forms the ſpots and ſtains called petechiæ, vibices, &c.

A greater degree of the debility which occaſions theſe ſymptoms, gives riſe to mortification, which appears in the worſt kinds of fever, and which is to be regarded in no other light than a partial extinction of life; and accordingly we find it moſt common in the extremities, where the circulation is moſt feeble, or on the parts on which the patient reſts, where it is apt to be interrupted by the preſſure.

To a perſon acquainted with the ſymptoms of this diſeaſe, it is almoſt unneceſſary to point out how exactly the various functions of the nervous ſyſtem correſpond with the ſtate of the vital organs. Hence the uneaſineſs and confuſion of mind which ſo conſtantly attend the foregoing ſymptoms. In proportion as the pulſe loſes its ſtrength and becomes irregular, the mind becomes

becomes feeble and unſteady, till at length it is incapable of any exertion, and is carried along with whatever idea preſents itſelf.*

Correſponding changes take place in the various organs of ſenſe; depraved taſte, ſmell, hearing, and ſight, particularly the laſt, are among the moſt frequent ſymptoms of the advanced ſtage of fever. The taſte and ſmell, indeed, are often depraved, independently of any affection of their nerves, from their peculiar ſecretions being vitiated.

The powers of voluntary motion muſt partake of the ſtate of the nervous ſyſtem. They become weak and irregular. Sometimes in particular parts of the body are wholly loſt, or the muſcles are affected with twitching and ſtarting, and often with more violent convulſions. It is to be recollected that great debility of the brain diſpoſes to convulſions. They ſupervene on ſyncope from a fatal loſs of blood, and frequently

* Appendix, Note 42.

frequently on other ſtates of great nervous debility.

As the debility encreaſes, a retenſion of urine, from a paralyſis of the bladder, involuntary diſcharges of urine and fæces from that of the ſphinters, loſs of deglutition, &c. ſupervene. The cauſe of ſuch ſymptoms is too evident to require any comment.

On the nature of thoſe ſymptoms, which have given riſe to the opinion of a putreſcency of the fluids, it will be neceſſary to make ſome farther obſervations. Dr. Cullen, in the 86th paragraph of his Firſt Lines, remarks, "that the production "of human and marſh effluvia is favored, "and their power encreaſed, by circum- "ſtances which favour putrefaction, and "they often prove putrefactive ferments "with reſpect to the animal fluids. As

"putrid

" putrid matter, therefore, is always, with " reſpect to animal bodies, a powerful " ſedative, ſo it can hardly be doubted " that human and marſh effluvia are of " the ſame quality; and it is confirmed " by this that the debility which is always " induced ſeems to be in proportion to the " other marks that appear of the power " of thoſe cauſes;" and, in the 105th paragraph, he obſerves, " the ſymptoms denoting a putreſcent ſtate of the fluids are, " 1ſt, With reſpect to the ſtomach, the loathing of animal food, nauſea and vomiting, " great thirſt, and a deſire of acids.

" 2d, With reſpect to the fluids: 1ſt, " The blood drawn out of the veins not coagulating as uſual: 2d, Hemorrhagy " from different parts, without marks of " encreaſed impetus: 3d, Effuſions under " the ſkin, or cuticle, petechiæ, maculæ, " and vibices: 4th, Effuſions of yellow ſerum under the cuticle.

" 3d, With

"3d, With reſpect to the ſtate of the "excretions, fœtid breath, frequent looſe "and fœtid ſtools, high-coloured turbid "urine, fœtid ſweats, and the fetor and "livid colour of bliſtered parts.

"4th, The cadaverous ſmell of the whole "body."

From what has already been ſaid the reader will perceive that the foregoing ſymptoms are readily accounted for without ſuppoſing any other putreſcent ſtate of the fluids than that which neceſſarily ariſes from the debility of the vital powers.

The firſt ſet of ſymptoms, the loathing of animal food, nauſea, vomiting, thirſt, and a deſire for acids, ariſe from a failure or vitiated ſtate of the ſecretions, are common to all kinds of fever, and as ſtrongly marked in ſynocha, in which no putreſcency of the fluids can be ſuſpected, as in typhus.

Of the ſecond ſet of ſymptoms, I have

already had occaſion to obſerve, that the hemorrhagies and effuſions ſo common in malignant fevers are readily accounted for by the relaxed ſtate of the ſolids, and the blood being thinner than in health, a change that naturally ariſes from the powers of abſorption and ſecretion being impaired, and the blood conſequently partaking of the change it undergoes when theſe ceaſe altogether. While the animal lives, however, this change never goes ſo far in the circulating fluids, as to occaſion fetor in them.

With reſpect to the remaining ſymptoms, when the various functions are much impaired, the contents of the ſtomach and alimentary canal ſtagnate. Theſe form no part of the living body. They are as apt to become putrid as the ſame matter out of the body expoſed to the ſame degree of heat and moiſture, unleſs the antiſeptic fluids are ſupplied to check this tendency. They therefore become putrid in fevers of long

long continuance, and the more readily if there be preſent a conſiderable portion of bile which is a ſeptic.

A ſimilar change takes place on the ſurface of the body. The failure of the due ſecretion and abſorption there, cauſes a ſtagnation and putrefaction of the natural moiſture. Hence the putrid ſmell of the ſweat. This alſo happens in parts which have been bliſtered, in ulcers, &c. ſo that it is not difficult to account for the cadaverous ſmell of the body, without ſuppoſing any putrefactive ferment in the circulating fluids. And if thoſe labouring under typhus are more ſubject to gangrene than people in health, this only proves that in them the vital powers are more languid, and conſequently apt to fail. *

II. *Of the Remote Cauſes of Fever.*

Reſpecting the cauſes of fever it will only

* Appendix, Note 42.

only be neceſſary to ſhew that they are all ſuch as occaſion debility of the vital organs, and that all cauſes which debilitate theſe organs produce fever.

All cauſes which excite violent and long-continued exertions of the animal organs are found to produce this diſeaſe. Any exertion of the muſcles of voluntary motion too long continued, exceſſive venery, long watching, intenſe ſtudy, violent paſſions, pain, or any other ſpecies of fatigue, are among the moſt powerful cauſes of fever; and we ſhall find, in ſpeaking of the treatment of this diſeaſe, that from whatever cauſe it may have ariſen, every thing which excites the animal organs tends to increaſe and prolong it, becauſe they cannot anſwer the ſtimulus without farther exhauſting the vital organs. Theſe cauſes I would term indirect, becauſe they act through the medium of the animal organs.

The other cauſes of fever act immediately

ately on the vital organs. The moſt ſimple of theſe are ſuch as tend to deprive theſe organs of their uſual ſtimulus. This may be done either by abſtracting part of the blood, or by preventing the formation of a due quantity. Thus it is that profuſe evacuations, long faſting, and all diſeaſes which prevent for a certain length of time the neceſſary ſupply of chyle, occaſion fever.*

Other cauſes of fever operate by applying to the vital organs a preternatural ſtimulus, the immediate effects of which are followed by debility. Thus the exceſſive uſe of opium,† or fermented liquors, occaſions fever. If the quantity of theſe is only ſuch as to ſtimulate, without ſubſequent debility, fever does not take place. The pulſe indeed becomes ſtronger and fuller, and the heat is increaſed, but the ſuſpenſion of the ſecreting powers, and the other ſymptoms of fever, do not follow, and

* Appendix, Note 43. Note 44.

and the diſeaſed ſtate of the pulſe and increaſed heat are only temporary. The noxious matter is ſoon expelled by the excretories, and the heart and blood veſſels regain their healthy action. But if the preternatural excitement goes on till it occaſions debility in theſe organs, the ſecretions fail, and all the ſymptoms of fever gradually ſhew themſelves.

To the ſame head, the cauſes of fever acting directly on the vital organs, belong the moſt frequent of all theſe cauſes, cold and contagion. To be convinced that theſe act by debilitating the vital organs, we have only to take a view of their immediate effects, and of the circumſtances in which they are moſt apt to excite fever.

It would be ſuperfluous to adduce any proofs of cold below a certain degree being a debilitating power. * This degree is different at different times, according to the ſtate

* Appendix, Note 45.

ſtate of the body, and the circumſtances under which the cold is applied.

A degree which is eaſily reſiſted by a ſtrong and vigorous habit, will debilitate a feeble one, and an uniform application of cold continued only for a ſhort time, and applied at a time when the body has not previouſly been expoſed, at leaſt for a conſiderable time, to a much higher temperature, will be reſiſted; when a more partial, or longer application of cold, eſpecially after expoſure to a high temperature, will occaſion debility. Damp clothes are the moſt pernicious mode of applying cold, probably from its being confined to the ſurface where the circulation is moſt apt to fail; * while a cold air is applied equally to the ſurface and to the lungs; † and from its being of long continuance. Such are the circumſtances under which cold is moſt apt to debilitate the vital powers, and preciſely under

* Appendix, Note 46. † Note 47.

under theſe circumſtances it is found moſt apt to occaſion fever.

Dr. Cullen obſerves, in the 94th paragraph: "The circumſtances of the cold "applied, which ſeem to give it effect," that is, in producing diſeaſe, "are, 1ſt, "The intenſity, or degree of the cold: 2d, "The length of time during which it is "applied: 3d, The degree of moiſture at "the ſame time accompanying it: 4th, Its "being applied by a wind or current of "air;" this circumſtance ſeems only to operate by occaſioning a more ſudden and partial abſtraction of heat: "5th, Its being "a viciſſitude, or ſudden and conſiderable "change of temperature from heat to cold.

"The circumſtances of perſons ren-"dering them more liable to be affected "by cold, ſeem to be, 1ſt, The weakneſs "of the ſyſtem, and particularly the leſ-"ſened vigour of circulation, occaſioned "by faſting, by evacuations, by fatigue,

"by

"by a laſt night's debauch, by exceſs in "venery, by long watching, by much "ſtudy, by reſt immediately after great "exerciſe, by ſleep, and by preceding diſ-"eaſe: 2d, The body, or its parts, being "deprived of their accuſtomed coverings: "3d, One part of the body being expoſed "to cold while the reſt is kept in its uſual, "or a greater, warmth.

"The power of theſe circumſtances "is demonſtrated by the circumſtances "enabling perſons to reſiſt cold. Theſe "are a certain vigour of conſtitution, exer-"ciſe of the body, the preſence of ac-"tive paſſions, and the uſe of cordials." To the powers enabling the body to reſiſt cold, Dr. Cullen adds, the preſence of other impreſſions and habit. Theſe of courſe tend to obviate the effects of all agents.

With reſpect to contagion, little acquainted as we are with its nature, we know that it is a debilitating power. This appears

from the ſources from which it originates, from its immediate effects, and from the ſtates of body moſt favourable to its operation.

Many diſeaſes, the ſmall pox, meaſles, &c. we never ſee produced by any other cauſe but contagion; and it was a queſtion once much agitated among phyſicians, whence theſe diſeaſes originated, for the contagion ſeems to ariſe from the diſeaſe itſelf. The queſtion ſeems only capable of being anſwered by the ſuppoſition that ſuch diſeaſes, though afterwards propagated by their peculiar contagions, are at firſt produced, independently of contagion, by a concurrence of cauſes which do not often take place. Hence we ſee in the hiſtory of medicine accounts of contagious diſeaſes diſappearing, and at ſome future period again ſhewing themſelves, or of others appearing in their ſtead.*

What-

* Appendix, Note 48.

Whatever may be the difficulty of tracing the ſource of many contagious diſeaſes, that of typhus is ſufficiently apparent. A very ſimple concurrence of cauſes is capable of producing this diſeaſe. It may ariſe in any crowded and ill-ventilated place, (even brutes are ſubject to it under theſe circumſtances *), or from putrid effluvia applied in certain quantity, and for a certain length of time, from whatever cauſe.

The firſt effects of the contagion of fever, as we might have expected from the nature of theſe ſources, are ſymptoms of debility. At the commencement of a contagious fever there is a general proſtration of ſtrength, the pulſe is ſmall and feeble, the extreme parts of the circulating ſyſtem performing their functions very imperfectly. The mind is unſteady, and the vigor of the muſcles of voluntary motion much impaired. †

We

* Appendix, Note 49. † Note 50.

We have ſeen that general debility, however induced, may occaſion fever; it will not, therefore, ſeem ſurpriſing that all cauſes of debility are favourable to the operation of contagion; fear, and other depreſſing paſſions, or the debilitating effects of the more powerful exciting paſſions, a ſcanty diet or intemperance, indolence or fatigue, a very cloſe and warm, or a cold damp atmoſphere, render the body more liable to infection. Even the temporary debility from the ſtomach being empty, and ſleep, in which the circulation is more languid than when we are awake, nay, the ſlight debility occaſioned by a diſguſting odour, have been obſerved to produce the ſame effect. On the ſame principle convaleſcents are found more liable to infection than people in health.

Theſe remarks are well illuſtrated by the obſervations of a variety of writers, and particularly by Dr. Lind, in his Treatiſe on

on Fevers and Infections. He obſerved, that thoſe who had been ſlightly expoſed to contagion often eſcaped the fever, if not ſoon after ſubjected to the action of debilitating cauſes; and that many who were recovering from contagious fevers, their bodies not yet free from the contagion, had the fever renewed by ſuch cauſes. *

When, on the other hand, we take a view of the means of preventing infection, we find that all thoſe which have proved at all efficacious are ſuch as tend to ſupport the vigor of the vital organs, equanimity and confidence, a generous but moderate diet, in which is included a proper quantity of wine, regular exerciſe, an intereſting but not fatiguing application of mind, tonic medicines judiciouſly adminiſtered, that is, in ſuch a way as ſhall not oppreſs the ſtomach nor diſorder the bowels, and

* Appendix, Note 51.

and the cold bath ſo employed as not to overpower the ſtrength.

From the foregoing curſory view of the cauſes of fever, it appears that they are all ſuch as more or leſs directly debilitate the vital ſyſtem. The immediate operation of ſome appears to be on the centre of this ſyſtem, the heart and larger blood veſſels; of others on the circumference, the capillary veſſels; and of ſome equally on all parts of the ſyſtem.

When the debilitating cauſe acts on the extreme parts of the vital organs, we find the ſymptoms of debility ſlight, and the excitement which ſucceeds comparatively great, the cauſe of which is ſufficiently evident. The debility of the extreme parts of the circulation is never ſo immediately felt throughout the ſyſtem as that of organs more immediately eſſential to life,

life, and the vigor of the heart and larger veſſels remaining unimpaired, they are readily excited to increaſed action by the retention of what ſhould have been thrown off by the capillaries. Thus it is that increaſed excitement is the characteriſtic feature of fever from cold, whoſe operation is on the extreme parts of the ſanguiferous ſyſtem.

When, on the other hand, the debilitating power is applied to the heart and large blood veſſels, the ſymptoms of debility muſt be great, compared with the degree of excitement; for the debilitating cauſe being applied to the organs moſt immediately neceſſary to life, its effects are quickly felt in every part of the ſyſtem;* and the debility of the extreme parts of the ſanguiferous ſyſtem being ſecondary, the retention of the matter which ſhould be excreted by them is neither ſo ſudden nor ſo complete as

* Appendix, Note 52.

as in the former caſe, nor are the heart and larger veſſels, with which the debility originated, in a ſtate ſo well fitted for excitement, as when the debilitating caúſe operates only on the capillaries. Hence it ſeems to be that in fevers from contagion, debility is the leading feature.

It may be difficult to prove that the firſt operation of contagion is on the heart and larger veſſels, yet the arguments afforded, both by the ſymptoms which characteriſe the commencement of typhus, and by analogy from the phœnomena of other contagious fevers, leave little room to doubt it. The ſmall pox, for example, when the contagion is very active, commences with a train of ſymptoms ſo ſimilar to the commencement of typhus, that they can hardly be diſtinguiſhed, but it commences with the ſame train of ſymptoms, whether it is received in the natural way or by inoculation.

Now

Now in the inoculated ſmall pox we are aſſured that the firſt impreſſion of the contagion is on the heart and large veſſels, for the fever does not commence till matter is formed in the inoculated part and received into the ſanguiferous ſyſtem by means of the abſorbents; and it would ſeem that in typhus and the natural ſmall pox a certain time is required, as in the inoculated ſmall pox, before the contagion, to whatever part applied, occaſions the ſecretion of ſuch a matter as, when abſorbed, ſhall produce the diſeaſe.*

In fever from the third ſet of cauſes, thoſe which act equally on every part of the vital organs, we do not perceive either the excitement or debility ſo predominant at the commencement, as the one or the other is in the preceding caſes. Thus in fever from a poor and ſcanty diet, for example, or from chronic diſeaſes, which

* Appendix, Note 53.

impede the due formation of chyle, the ſymptoms gradually ſhew themſelves without any very marked debility or excitement; for here all the vital organs being equally and gradually debilitated, there is no ſudden retention of the excreta to excite to powerful action the heart and larger veſſels, nor are they, from partaking of the debility, in a ſtate to be greatly excited; and, on the other hand, as no debilitating power is ſuddenly applied to the heart and larger veſſels, the ſudden debility which attends the commencement of typhus is not obſerved.

III. *Of the Treatment of Fever.*

If we admit the accuracy of the preceding obſervations, the principles which ſhould conduct the treatment of fever are ſufficiently obvious, and the practice at which we thus arrive, we ſhall find experience has pointed out to be the beſt. It has, however,

however, been unſteady, complicated, and ill-defined, owing, as far as I can judge, to theſe principles having been miſtaken, or but imperfectly underſtood.

We have ſeen that the debility of the vital ſyſtem may be induced not only by debilitating powers immediately applied to it, but alſo by too great and long-continued excitement of the animal organs. We hence infer that this debility having taken place, any excitement of theſe organs muſt increaſe it.

We have alſo ſeen, that debilitating powers immediately applied to the vital ſyſtem, produce fever, whether they make their firſt impreſſion on its circumference or central parts; and in endeavouring to reſtore its vigor, we ſhall find, that the means of cure admit of the ſame diviſion.

It appears, from what has been ſaid, that although the circumference of the vital

vital ſyſtem is always in a ſtate of debility in fever, the centre is often in that of increaſed excitement, namely, while ſufficient excitability remains in the heart and larger veſſels to occaſion a preternatural excitement of them by the ſtimulus of the retained excreta. We ſhall, therefore, find that while at all periods of fever the means of exciting the circumference of the vital ſyſtem are indicated, thoſe of exciting the centre are only proper in the advanced ſtages.

It farther appeared, as in the caſe of inflammation, that although the increaſed excitement of the heart and larger veſſels is evidently favorable to the reſtoration of vigor in the capillaries, yet it will often, when there is great difficulty in exciting the capillaries, and thus expelling the preternatural ſtimulus, become ſo violent and be ſo long continued as to occaſion debility of the central parts of the vital ſyſtem, inſtead

ſtead of reſtoring circulation to its circumference; or, if this be in ſome degree effected, it is more than compenſated by the ſubſequent debility of the centre, which ſoon extends to the circumference. Under theſe circumſtances, ſo far from employing means further to excite the heart and larger veſſels, our practice muſt be directed to reſtrain their action.

In the treatment of fever, then, there are two indications which may be termed general, becauſe they apply to all fevers and to all their periods, namely, to avoid the excitement of the animal organs, and to excite the circumference of the vital ſyſtem; and two, which may be termed partial, becauſe they apply only to certain ſtates of fever, namely, to diminiſh the excitement of the heart and larger veſſels when it is above the due degree, and to increaſe it when it falls below that degree.

The

The firſt indication in fever, then, is to remove every cauſe which may excite the animal organs. Of this indication little need be ſaid. Every one knows of what importance it is in all kinds of fever to keep the patient quiet and ſtill, and, when the ſymptoms are ſevere, whether thoſe of ſynocha or typhus, to avoid as much as poſſible impreſſions of every kind. Not merely the excitement of the different organs of the ſenſes, as they are termed, but much heat, the irritation occaſioned by food in the ſtomach and bowels, when the powers of digeſtion are ſuſpended, or too long a retention of the natural excretions of the latter, the continuance of thirſt, in ſhort, every thing that excites ſenſation, is injurious.*

To the ſecond general indication belong many of the moſt important parts of the treatment of fever. It has from the infancy

* Appendix, Note 54.

infancy of medicine been obſerved, that increaſing the action of the ſkin often removes fever; but it is only lately that we have been taught how this ought to be done. When the patient was confined to a hot room, and loaded with bed cloaths, if a ſweat were induced it was the conſequence of relaxation from the exhauſting effect of the temperature, not of a return of vigor, and ſo far from being accompanied with the good effects of ſpontaneous ſweating, it almoſt always did harm, by leaving the ſkin in a more debilitated ſtate than before its appearance, not to mention the injury done by the painful impreſſions occaſioned by this mode of treatment.

We now excite the action of the ſurface by forcibly propelling the blood towards it by emetics, or by diaphoretic medicines, particularly the preparations of antimony and ſome neutral ſalts; for it

it is to be obſerved, that the means which excite the natural action of the ſkin are much more powerful in relieving fever than thoſe which produce ſweat, during which this organ ſeems always to be rather in a ſtate of relaxation than of vigor,* we ſucceed better with tartrat of antimony than with the compound powder of ipecacuanha; and, for the ſame reaſon, we ſucceed better by the application of cold than by the heating regimen. I need not here point out the great advantage of cool air in fever, and the excellent effects of the application of cold water, employed in the manner recommended by Dr. Currie.†

In warm weather the ſkin is more relaxed; in cold weather it is more vigorous. Even when the relaxation occaſioned by warm weather goes ſo far as to produce ſweat, the ſecreting power of the ſkin is

* Appendix, Note 55. † Note 56.

is not ſo great as under a due degree of exerciſe in a cold atmoſphere. This appears both from the ſtate of the urine, from which, as I have explained in a note juſt referred to, the degree of activity of the ſkin may be aſcertained, and from the greater appetite in the latter caſe. Now a ſtate of fever comes in place of exerciſe, it ſupports the proper warmth of the body under the application of cold to its ſurface, and ſecures the good effects of the latter; for the application of cold to the ſurface is never proper in fever when the temperature falls below the healthy degree, and is moſt ſucceſsful when it is conſiderably above it, provided the ſkin is dry. In ſhort, it is moſt ſucceſsful in fevers in the ſame circumſtances in which it is found in health moſt to excite the action of this organ, only in the one caſe the increaſed temperature is ſupported by exerciſe, in the other by a

preternatural ſtimulus applied to the heart and blood veſſels.

Analogous to the office of the ſkin, the external ſurface of the animal ſyſtem is the office of its internal ſurface, if I may uſe the expreſſion, the alimentary canal; and ſuch is the ſympathy between theſe ſurfaces, that if one is languid the other is affected in the ſame way; and if we excite either, we at the ſame time, in a greater or leſs degree, increaſe the action of the other. If the bowels are conſtipated, we find the ſkin dry and ſhrunk; as ſoon as the bowels are reſtored to action, the ſkin becomes ſoft and moiſt, and vice verſa. except when, the ſecretion by the ſkin being ſuddenly ſtopped, the fluid which ſhould have paſſed by it is thrown on the bowels, applying to their veſſels a ſtimulus which prevents their inactivity.

We might therefore, a priori, have expected

expected conſiderable advantage in fever from exciting the bowels, not to mention that their veſſels form ſo large a portion of the circumference of the vital ſyſtem. Every phyſician muſt have obſerved the excellent effects of ſupporting the due action of the bowels in fever, particularly in its early ſtages; but no other writer has placed this ſubject in ſo clear a point of view as Dr. James Hamilton,* of Edinburgh. I have found the moſt decided advantage from exciting catharſis to the extent which he recommends.

Such are the chief means of exciting the capillaries in fever. Others have been propoſed, particularly rubbing the ſurface with warm oil, which, in certain fevers, is ſaid to be very ſucceſsful. Of this, which appears a doubtful practice, from the heat and irritation which muſt attend it, we have had no experience in this country.

In

* Appendix, Note 57.

In the early ſtages of almoſt all fevers which laſt above a few hours, it is neceſſary, with the preceding means, to have recourſe to ſuch as are calculated more directly to leſſen the increaſed action of the heart and larger veſſels, and conſequently the debility which ſucceeds this action.

As the means which excite the capillaries tend to throw off the cauſe of preternatural excitement in the heart and larger veſſels, and as by the evacuation which they occaſion they tend to reduce the volume of the blood, the remedies we have juſt been conſidering, it is evident, with their other good effects, muſt reduce the excitement, and are ſometimes ſufficient for this purpoſe.

We more directly diminiſh the ſtimulating quality of the blood, by diluting it by a plentiful ſupply of any mild fluid, and by the uſe of acids and certain neutral ſalts, which ſeem to act rather in this way than

as

as diaphoretics, particularly acetat of kali, and nitrat of ſoda.

When theſe fail ſufficiently to diminiſh excitement, we are obliged to have recourſe to veneſection, the moſt powerful means of diminiſhing both the volume and ſtimulating quality of the circulating fluids; for it is to be recollected, that as the red blood is abſtracted by this operation, the whole maſs is rendered thinner and leſs ſtimulating. The precautions to be kept in view in the employment of veneſection, and the circumſtances which render them indiſpenſable, I have detailed at length in the firſt volume of my Treatiſe on Fevers.

Our view in the commencement of fever, is not to reduce the action of the heart and larger veſſels to the natural ſtandard. This would be obviating the means which we have ſeen the nature of the circulation affords for removing the cauſe of the diſeaſe. It is only our object

to

to leſſen excitement when it goes beyond the degree moſt favorable to this end; for we obſerve in fever, that when the excitement, in conſequence of the difficulty of reſtoring action to the capillaries, runs very high, the heart and larger blood veſſels themſelves are ſoon debilitated, and the diſeaſe aſſumes a more formidable appearance, the various ſymptoms which indicate a general debility of the vital ſyſtem ſupervening; but that, when their action is reſtrained, ſo that a longer continued though more gentle ſtimulus is applied to the extreme veſſels, it more frequently proves ſucceſsful.

If however the means employed for this purpoſe be ſo injudiciouſly directed as to reduce the excitement of the heart and larger blood veſſels more than is neceſſary to inſure the requiſite continuance of that excitement, a double injury is done. Not only the ſtimulus neceſſary to the excitement

ment of the capillaries is removed, but the debility of the heart and larger veſſels is hurried on, and the fever ſoon begins to aſſume its worſt form. Hence the incalculable injury done by the indiſcriminate uſe of the lancet, till lately ſo common in the commencement of fevers. *

Phyſicians ſeem inſenſibly to be ſubſtituting a free diſcharge by the bowels, for veneſection, at this period of the diſeaſe; and this change has not ariſen from any reaſoning on the ſubject, but from experience having taught them, that it is the more ſucceſsful practice. The nature of fever itſelf, however, might have pointed it out. It muſt appear, even at firſt view, of how great importance it is to ſave the ſtrength in this diſeaſe, and particularly to preſerve the maſs of blood as entire as poſſible, at a time when the ſyſtem is incapable of forming more.

Now

* Appendix, Note 58.

Now by catharſis, only the thinner and leſs important part of the blood is abſtracted; by veneſection, the moſt important, both becauſe it is the moſt eſſential to vigour, and the moſt difficult to be renewed. Every motive diſſuades us from having recourſe to the latter, while it is in our power, as in almoſt all the fevers, properly ſo called, of this country, to produce the deſired effect by catharſis.

Theſe arguments in favor of catharſis are ſufficient, not to mention, that by this evacuation, as I have juſt had occaſion to obſerve, we directly rouſe to action a large portion of the debilitated capillaries, and by ſympathy tend to excite thoſe of the ſkin.

It is not to be denied that veneſection is ſometimes neceſſary at the commencement of ſimple fever. In tropical climates, in particular, the excitement is often ſo exceſſive, that if allowed to go on it will even

even in a few hours reduce the ſyſtem to a ſtate of extreme debility. Here catharſis is too feeble a remedy; and, precarious as the alternative is, we muſt abſtract the more ſtimulating and vital parts of the blood.

To this peculiarity of the fevers of ſuch climates we may chiefly, perhaps, attribute their great fatality; and ſuch is the perplexity which it has occaſioned to practitioners, that we find the beſt and moſt experienced in doubt whether the effects of the excitement, or of the means neceſſary to relieve it, are moſt to be dreaded.* If we might venture to give our opinion from what we obſerve in the fevers of this country, we ſhould ſay, that moſt of thoſe who have written on the fevers of tropical climates have been too ſparing in the uſe of cathartics, which are indicated, not merely for the reaſons juſt pointed out, but by the

* Appendix, Note 59.

peculiar ſtate of the biliary ſyſtem, which almoſt always attends the fevers of warm climates. *

To the ſtate of increaſed excitement, whatever be the mode of treatment, or nature of the fever, if it laſt above a day or two, always ſucceeds that of debility. The action of the heart and larger veſſels now falls below, as in the firſt ſtage of fever it riſes above, the healthy degree. If we cannot ſufficiently excite them, the debility of the circumference increaſing with that of the central parts of the vital ſyſtem, the powers of life are at length loſt in the former, and death gradually extends to the centre.

This part of the treatment of fever ſeems at preſent the moſt defective; no writer, as far as I know, having laid down any rules by which the treatment of what may

* Appendix, Note 60.

may be called the ſecond ſtage of fever, can be confidently regulated.

The feeble ſtate of the circulation and the temporary good effects of powerful ſtimuli have led moſt phyſicians, and particularly thoſe of later times, to employ them with great freedom. Many, however, confeſs that they have been diſappointed in their effects;* of this number, I can feel no heſitation in declaring myſelf to be one. I have found the ſecond ſtage of fever moſt tractable when all powerful ſtimuli were avoided.

It is true, indeed, that large quantities of opium, or wine, will often give a degree of vigour, increaſing the ſtrength and leſſening the frequency of the pulſe. But theſe effects are tranſitory. It is ſoon neceſſary to repeat the remedy, and at length to increaſe its power, in order to procure the ſame effects; and this tranſitory vigour ſeems

* Appendix, Note 61.

ſeems frequently obtained at the expence of exhauſting the ſtrength, which, had it been more carefully huſbanded, might have carried the patient through his diſeaſe.

Were I to ſtate the reſult of my own experience in the ſecond ſtage of fever, it would be, that opium is only uſeful when ſmall doſes allay irritation, and procure compoſure, if not ſleep; and that wine is rarely beneficial if given in larger quantity than might be taken in health without ſubſequent debility, and can ſeldom, perhaps, be given without injury even to this extent.

Whether there are ſtates of fever in which large doſes of theſe ſtimuli may be of advantage, it is difficult to ſay. In extreme debility, when the patient is almoſt in *articulo mortis*, a ſtrong ſtimulus may ſometimes, perhaps, by rouſing the languid ſyſtem, be the means of preſerving life. I have frequently ſeen the experiment made with

with temporary, never with permanent good effects.

Is there reaſon to expect permanent good effects from increaſing the quantity of oxygen in the air which the patient breathes? *

There is another claſs of ſtimuli employed in the ſecond ſtage of fever which have obtained the name of tonics, which occaſion leſs, but more permanent excitement, and on theſe accounts ſeem better adapted to this diſeaſe.

Some of theſe, particularly the cold infuſion of bark, or, if this occaſion heat and irritation, † that of colomba, with a little wine, if of the ſtronger kinds diluted, and, when there is any appetite, light food, taken in ſmall quantities at a time; ‡ a careful attention to cleanlineſs, ventilation, and the ſtate of the bowels, a cool atmoſphere, and when the temperature of the body

* Appendix, Note 62. † Note 63. ‡ Note 64.

body is decidedly above the healthy degree, the application of cold water to the ſurface, form, according to my experience, the moſt ſucceſsful treatment of the ſecond ſtage of fever.

It is hardly neceſſary to obſerve, that I ſpeak here of the general plan of treatment. Various remedies are occaſionally indicated by the appearance of certain ſymptoms ariſing from local affections, the treatment of which, as they are not eſſential to fever, do not fall under the general plan of cure.

Univerſal experience has aſcertained, that the more powerful means of exciting the capillaries, if we except the application of cold to the ſurface, ſhould form no part of the treatment of the ſecond ſtage of fever, the cauſe of which is ſufficiently evident. With this exception, they all neceſſarily occaſion more or leſs evacuation, which

which ought, now, as much as poſſible to be avoided.

In the firſt ſtage of fever, our view is to reſtore tone to the capillaries; in the ſecond, to the whole vital ſyſtem.

APPENDIX.

NOTE 1.—IT was soon observed by physiologists, that the animal body, by its peculiar mechanism, tends to correct any injury it may sustain, and to expel any noxious matter introduced into it. If, instead of food, indigestible substances be received into the stomach, they are generally rejected by vomiting, or expelled by an increased peristaltic motion of the bowels. If these are injured by the passage of such substances, other motions are excited to repair this injury. If a thorn is introduced under the skin, inflammation and suppuration ensue, and the discharge of matter continues till the whole of the offending substance is washed out. This property of the animal body has long been known in the schools by the name of the *vis medicatrix naturæ*; and in medical reasonings, very unwarrantable uses have been made of it.

It would appear, at first view, that the attempt to ascertain whether a disease be an effect of the *vis medicatrix*, must be an important and very necessary inquiry, that we may not counteract its salutary efforts: but the difficulty of ascertaining what symptoms are to be referred to its agency, and

and still more the uncertain tendency of these symptoms, which are sometimes too feeble to overcome the cause of the disease, and sometimes so violent as to endanger life, has rendered this inquiry of but little use in practice. Were we, for instance, to trust to the salutary efforts of nature when poison is received into the alimentary canal, we should find, in many instances, that the motions excited would not be sufficient to expel it. In others, these motions would become so violent and long continued, that they would exhaust the strength, after the cause which excited them was removed. In the one case, we must, by artificial means, increase the efforts of nature, in the other, restrain, and at length allay them. In short, we must use the means which our knowledge of the animal œconomy teaches us will speedily, and without injury to the system, expel the offending cause; and we trust no further to the operations of nature than we find them conducive to these ends.

It appears, indeed, from the works of those times, when the operations of the *vis medicatrix* were most studied, that this study has rather injured than improved the practice of medicine. It not only induced Physicians to rely too much on the efforts of the system, and rendered them averse to the use of active medicines; but it threatened to put a stop to all rational inquiry respecting the nature of diseases. Every symptom, which could not otherwise be accounted for, was referred to the operations of this power; and Physicians supposed they had sufficiently explained the nature of a disease when they said it was an effort of the *vis medicatrix*.

But supposing them right in this conjecture, it would have gone but a short way towards ascertaining the nature of the disease. To return to the illustration of poison, it would be of little use for us to know that it is expelled by an effort of nature, if we are not at the same time informed of the steps by which nature expels it. Without this knowledge could we at all understand the disease, or regulate the efforts, excited?

Besides, those who wish to abridge their labour by a reference to the *vis medicatrix naturæ*, forget that it is only after we understand the nature of a disease that we can say whether it is a salutary effort of the system or not. They assume principles for the purpose of explaining the nature of a disease, at a knowledge of which they cannot arrive until its nature is understood. The result of our inquiry may be, that it is an effort of the *vis medicatrix;* but it is impossible that any reference to this power should assist us in the inquiry.

One would naturally suppose that when Physicians had convinced themselves that all diseases were only efforts of nature, to expel some noxious agent, or restore vigour to some debilitated part, they would have thought it worth their while to inquire by what steps she effected these ends. But this was too severe a task. Accustomed to expatiate in the regions of fancy, they could not stoop to the painful attention which an unbiassed observation of nature demands, nor be satisfied with the slow advances which an inquiry of this kind admits of.

But although they were confined on one side, on another

there

there was still sufficient room for conjecture; although they could not explain the manner in which the *vis medicatrix* operates, they could speculate on the nature of this power. Hence arose the celebrated doctrine of Stahl and his followers, who maintained that the *vis medicatrix* is the immediate operation of the mind, which, independently of any influence necessarily communicated to it by the state of the body, perceives, by its own intelligence, the injuries done to the body, and excites such motions as are calculated to correct these injuries and obviate their consequences. In the present day it would be very superfluous to enter on any refutation of an hypothesis so unfounded and inconsistent. Even to spend time in stating it, would require some apology, did we not find among its advocates such men as Mead, Gaubius, and Porterfield.

Note 2.—In the yellow fever of America, Dr. Linning observes, that the urine often shews the critical sediment on the very first day, which he uniformly found a bad symptom; and the more copious the sediment was, the worse, he remarks, was the prognosis. See Dr. Linning's Letter to Dr. Whytt on the Yellow Fever of South America, in the 2d Volume of the Essays and Observations Physical and Literary.

One of the most fatal fevers, of which we have any account, is the Ephemera Britannica described by Caius; the leading symptom of which was a profuse flow of sweat, from which it received the name of Sudor Anglicus. See Caius, De Ephemera Britannica.

Note 3.

NOTE 3.—Sweats of this kind, Dr. Jackson informs us, are often the forerunner of death in the yellow fever of Jamaica. Other tropical writers make similar observations, and even in this country no Physician can practice long without seeing them confirmed.

NOTE 4.—If the reader has seen the account of Dr. Brown's hypothesis, in the first volume of my Treatise on Fevers, he may probably remark, that its merits are there estimated more highly than in the work before him. The truth is, from its having been the favorite system among the students of the University of Edinburgh, at the time I studied, I had conceived a strong prejudice in favor of it before I was capable of estimating its merits, and it was long before I could persuade myself that it had in fact made no real addition to our knowledge.

The passages I shall have occasion to quote from Dr. Brown's works, I shall give from his own translation of the *Elementa Medicinæ*, corrected by Dr. Beddoes.

NOTE 5.—In the following notes the numbers refer to the paragraphs of Dr. Brown's Elements of Medicine, and the Greek letters to the sections of the paragraphs.

For the sense in which Dr. Brown uses the term excitability, see x, xi, xii, xiv, &c.

NOTE 6.—See Dr. Brown's Elements of Medicine, paragraph CLXVII.

NOTE 7.

NOTE 7.—See Dr. Brown's Elements of Medicine, paragraph XIX.

NOTE 8.—Dr. Brown supposes the action of all stimuli to be the same, differing only in degree; he, however, divides them, in different parts of his work, in three different ways, into universal and local, diffusible and natural, or durable, and direct and indirect. "The effects common to all the "exciting powers, are sense, motion, mental exertion, and "passion. Now these effects being the same, it must be "granted that the operation of all the powers is the same." See xv. "Stimuli are either universal or local. The uni- "versal stimuli are exciting powers, so acting upon the ex- "citability as always to produce some excitement over the "whole system. The appellation of universal is convenient "to distinguish them from the local. The local stimuli act "only on the part to which they are applied, and do not, "without previously occasioning some change in it, affect "the rest of the body." See XVII, α, ζ, γ.

Dr. Brown uses the terms natural, or durable stimuli, in contradistinction to diffusible, by which he means the stronger stimuli, such as distilled spirits, musk, volatile alkali, æther, opium, &c. See CIV, CV, CVI, CXXVI, CXXX, φ, and CCXC. "The stimulus of the articles of diet, not exclusive of the "diffusible stimuli, should be denominated direct, because "it acts directly, and immediately on the excitability of the "part to which it is applied. Direct stimulus, at least, in so "far as it regards the food, is assisted by another stimulus "depending

" depending upon distension of the muscular fibres, on which " account, for the sake of distinction, the latter should be " called indirect. The latter is owing to the bulk of animal " and vegetable food, the former is produced by a relation or " affinity of the stimulus to the excitability. The indirect " acts upon the living solids in so far as they are to be consi- " dered as simple. The direct act upon them as living only." See CXXVII, CCLXVIII, &c.

NOTE 9.—I have here altered, a little, the language of Dr. Brown, by substituting living solid for excitability, as the exciting powers cannot be said to act on what he has defined to be a quality. His words are, " The effect of the ex- " citing powers acting upon the excitability may be denomi- " nated excitement."

NOTE 10.—See Dr. Brown's Elements of Medicine, XXIV, and various other passages.

NOTE 11.—" The sedative affections, as they are called," Dr. Brown observes, " are only a less degree of the exciting " ones; thus fear and grief are only diminutions or lower " degrees of confidence and joy, not passions different in " kind. The subject of the passions admits of the same rea- " soning in every respect as that of heat; and in the same " manner, all the bodies in nature that seem to be sedative, " are debilitating, that is, weakly stimulating, inducing de- " bility by a degree of stimulus inferior to the proper one." Dr. Brown's Elements of Medicine, XXI. *n*.

NOTE 12.

NOTE 12.—"If the property which distinguishes living "from dead matter, or the operation of either of the two sets "of powers" (that is, either the external agents, or those which exist in the body itself) "be withdrawn, life ceases; nothing "else than the presence of these is necessary to life." Dr. Brown's El. of Med. XIII.

NOTE 13.—"Whether the excitement has been in- "creased or diminished in a particular part, and whether its "diminution has been owing to direct or indirect debility, "and in either way the sthenic diathesis has been produced, "all the rest of the body soon follows the kind of change "which has taken place, because the excitability is an uni- "form, undivided, universal property of the system." CLXVII. "And must we, giving up our fundamental princi- "ple, after so compleat an establishment of it, allow that the "excitability is not the same uniform, undivided property "over all the system," &c. CCXXXII. See also a note be- longing to this paragraph. Dr. Brown, however, admits, that although the excitability is always affected in the same way in all parts of the system, yet it is affected in a greater degree in that part on which the stimulus acts than in any other.

NOTE 14.—See Dr. Brown's El. of Med. XVIII, XXIV, XXXIX. In one of these paragraphs he observes, "This mu- "tual relation obtains between the excitability and excite- "ment, that the more weakly the powers have acted, or the "less the stimulus has been applied, the more abundant the "excita-

"excitability becomes. The more powerful the stimulus, the "excitability becomes the more exhausted." In xxxix, he observes, "in this case the excitability becomes abundant; "because in consequence of the stimuli being withheld, it is "not exhausted," &c. See what is said of sleep in ccxxxix.

NOTE 15.—See Dr. Beddoes's Observations on this part of the Brunonian System, in the Introduction to his edition of Dr. Brown's Elements of Medicine.

NOTE 16.—"The debility arising from defect of stimu- "lus may be called *direct*, because it is not produced by any "positive noxious power, but by a subduction of the things "necessary to support life." XLV. For an account of this species of debility, see various parts of the Elem. Med. particularly the xxxviiith and eight following paragraphs.

NOTE 17.—"The excitability thus exhausted by stimu- "lus constitutes debility, which may be denominated *indi-* "*rect*, because it does not arise from defect, but excess of "stimulus." XXXV. For an account of this species of debility, see a variety of passages in the Elem. Med. particularly the xxviith and ten following paragraphs.

NOTE 18.—See an account of the diseases of debility, Elem. Med. from DIII. to DCXCV.

NOTE 19.—See Elem. Med. CCLI. In CCCXXVIII. Dr. Brown

Brown observes, "to every sthenia, to all sthenic diseases, "increased excitement over the whole system is a common "circumstance; it appears during the predisposition, in an "increase of the functions of body and mind; and after the "arrival of disease, in an increase of some of the functions, "a disturbance of others, and a diminution of others, in such "sort that the two latter phenomena are easily perceived to "arise from the noxious powers that produce the former, and "to depend upon their cause." See an account of diseases of excitement from CCCXXVIII to CCCCLIII.

NOTE 20.—See Elem. Med. CVI, &c. and the mode of treatment in sthenic diseases from CCCCLIII to DIII.

NOTE 21.—In DCLXXXVIII, Dr. Brown observes, "In di- "rect debility, where the redundancy of excitability does "not admit of much stimulus at a time, ten or twelve drops "of laudanum every quarter of an hour, till the patient, if, as "is usually the case in such a high degree of debility, has "wanted sleep long, falls asleep. Afterwards, when some "vigour is produced both by that and the medicine, and some "of the excessive excitability worn off, a double quantity of "the diffusible stimulus should be added, and in that way gra- "dually increased, till the healthy state can be supported by "stimuli, less in degree, more in number, and more na- "tural."

NOTE 22.—In DCLXXXVII, Dr. Brown observes, "when

" indirect debility has had more concern in the case, as in " agues, or more continued fevers, occasioned by drunkenness; " and in the confluent small-pox; the same remedies are to " be employed, but in an inverted proportion of dose. We " should consequently set out here in the cure with the largest " doses, such as are next in effect to that degree of stimulus " which produced the disease; then recourse should be had " to less stimuli, and a greater number of them, till, as was " said just now, the strength can be supported by the accus- " tomary and natural stimuli."

NOTE 23.—See Elem. Med. CCL, &c. If it be said, that the Brunonian system does not suppose that stimuli necessarily occasion a greater degree of excitement when the system is in a state of direct debility than in health, what shall we understand by the excitability being accumulated in the former case? Is there any other test of the excitability's being accumulated but the greater degree of excitement produced by the same stimuli? It is true, indeed, Dr. Brown often loses sight of this, one of the fundamental principles of his system.

NOTE 24.—In various experiments, related in the Appendix to the fourth volume of my Treatise on Febrile Diseases, I had occasion to throw a strong solution of opium, or tobacco, into the heart. In all of them this organ immediately became paralitic.

NOTE 25.

Note 25.—I have found, by repeated experiments, that the convulsions excited by opium and tobacco only exhaust the excitability of those muscles which are affected by them; and that the exhaustion is not in proportion to the quantity of opium which the animal takes, but to the force and frequency of the convulsions it excites. The convulsions excited by opium are of the same kind as those in tetanus. During the intermissions the slightest touch will renew them; and although the animal dies in nearly the same time, when the quantity of opium taken is the same, whether he is subjected to such an irritation as constantly renews the convulsions, or is allowed to remain at rest, yet in the former case the excitability of the muscles of voluntary motion is found more impaired in proportion as the convulsions have been more frequent.

Note 26.—I have found on applying a solution of opium to the muscles of the leg of a frog, that they have immediately lost their excitability, while that of every other muscle of the body remained unimpaired, and this, whether the animal was dead or alive.

If a solution of opium is thrown into the heart of a frog, the excitability of the muscles of the limbs will be found much impaired after death. But this I have found is neither owing to any action of the opium on these muscles, nor to any sympathy between their excitability and that of the heart, but, analogous to the result of an experiment above alluded to, (Note 25.) to the convulsions excited in them.

When

When the aorta is either divided or secured by ligature before injecting the solution into the heart, no convulsions ensue, and the excitability of the muscles of the limbs remains as entire as after any other death equally lingering; for the convulsions which take place in this experiment, when the aorta is neither divided nor secured, do not arise, as has been supposed, from any sympathy existing between the heart and the other muscles of the body, but from the solution being conveyed by that vessel, and immediately applied to the brain. See the experiments related in the Appendix to the fourth volume of my Treatise on Febrile Diseases.

Note 27.—One set of the organs, indeed, on which the vital functions depend, are subject to exhaustion, the intercostal muscles and diaphragm. These, as I shall presently have occasion to observe more particularly, (Note 31.) are as completely muscles of voluntary motion as those of the limbs. The slight degree of exhaustion which takes place after each of the moderate contractions of these muscles in ordinary respiration, is sufficiently restored by the interval of rest which intervenes between these contractions, during which the stimulus which produced them is removed. The above muscles form a medium of connection of great importance between the vital and animal functions. They are the only muscles employed in what has usually been termed the vital functions, whose stimulus acts through the medium of the brain. In all other instances the stimulus is directly applied to the muscle itself;

itself; and in all, the muscle is one of involuntary motion. On this peculiarity of the muscles of respiration many phenomena of disease seem to depend. We shall see, in a succeeding note, in what way it influences the symptoms of apoplexy. It seems to be the cause of death in this, and in most other diseases in which the injury is confined to the brain; for it would appear from experiment, (Note 32.) that no injury of the brain can directly destroy the action of the heart.

It is a question which has been much agitated among physiologists, why some muscles are subject to the will, while others are independent of it; and it has been supposed, that some of the nerves passing through ganglia is the cause of this difference. But would not a more evident explanation of it occur, were we to find on enquiry, that all the muscles of voluntary motion are excited through the brain, all the muscles of involuntary motion by stimuli, immediately applied to the muscle itself? See Note 31.

Note 28.—It would be more accurate here, and in other places, if custom permitted it, to follow chemical writers in the use of the term caloric. Expressing, by the same term, both the sensation, and that which causes it, is a frequent source of inaccuracy. The same objection applies to the use of the word cold: we want a term expressive of the abstraction of caloric.

Note 29.—Fever seems to be the only general disease properly so called. All other diseases are either simple local affections,

affections, or local affections complicated with a general affection. The only diseases which can be mistaken for simple general diseases, are affections of the heart and brain.

A local affection of the brain is known by a greater degree of dilirium than can arise from the fever which attends it, or by coma, or convulsions. A local affection of the heart is known by palpitation, or syncope. When these symptoms occur in fever, the case is to be regarded as one of general and local disease combined.

Note 30.—It would seem, at first view, that animal enjoyment would have been much increased had nature enabled the vital to afford the animal organs so copious and constant a supply of excitability, that they should never have suffered exhaustion. What organs would have been necessary for this purpose we know not; but a little reflection will teach us, that animal enjoyment would no otherwise have been increased by these means than by prolonging the life of the animal. Temporary insensibility gives no interruption to enjoyment; for it is to be recollected, that as far as relates to the animal itself, perfect sleep has no duration.

Note 31.—I have had occasion to observe in a preceding note (27.) that if it should be found that all the muscles of voluntary motion are excited through the medium of the brain, of involuntary motion by stimuli immediately applied to the muscles themselves, we should hence have an easy solution of

of the question, why the former are independent of the will, and the latter subjected to it. But what is of much more importance, we should be enabled to explain many phenomena of disease, the nature of which seems obscure, and, in in some degree, inconsistent with the general laws of the animal economy.

As far as respects the principal muscles of either class, there can be no difference of opinion: every one admits that the muscles of the limbs are muscles of voluntary motion, and that they are stimulated through the medium of the brain; and that the heart and blood-vessels are muscles of involuntary motion, stimulated by the blood which is immediately applied to them. But besides these two classes of muscles, physiologists suppose that there is a third class, which, partaking of the nature of the other two, are partly muscles of voluntary, and partly of involuntary, motion.

Dr. Gregory, in the 11th chapter of his *Conspectus Medicinæ Theoreticæ*, after enumerating what he supposes to be the muscles of voluntary, and those of involuntary motion, observes, "Medium fere locum inter occupant respirationis musculi, septum scilicet transversum, musculi abdominis, et qui inter costas jacent, et quotquot cum costis ita conjunguntur musculi, ut horum contractio eas vel firmare, vel elevare vel deprimere possit." To these most physiologists would add certain muscles which Dr. Gregory regards as muscles of involuntary motion; those of the rectum and bladder, and their sphincters.

It is true that none of these muscles are either so perfectly

fectly subjected to the will as those of the limbs, nor so independent of it as the heart and blood-vessels: on a closer view, however, it will appear, I think, that this is not owing to any peculiarity of the muscles themselves, but to the nature of their functions; and that they are all in the strictest sense either muscles of voluntary motion, excited through the medium of the brain, or of involuntary motion, excited by stimuli immediately applied to them.

Can we give any definition of a muscle of voluntary motion, but that whose action we can renew, interrupt, retard, and accelerate at pleasure? This definition applies as strictly to the muscles of respiration as to those of the limbs: nobody feels any difficulty in renewing, interrupting, retarding and accelerating their action as often as he pleases. We cannot, indeed, interrupt it for a considerable length of time, but this is not from any want of power over the muscles, but for the same reason that we are obliged to call the muscles of the arm into action when a candle is held to the fingers; in both cases we are unable to bear the sensation excited, and instinctively call into action the muscles which remove it. It is true that the action of the muscles of respiration continues during sleep, but it is for the same reason that the action of the muscles of the limbs is exerted during sleep, when a continuance of the same position causes uneasiness. The muscles of respiration, then, are muscles of voluntary motion, whose action is habitually excited, by a peculiar sensation, and consequently through the medium of the brain.

Over

Over the rectum and bladder, on the other hand, the will has no direct power, and they are never excited except by the stimulus of their contents. It is true, indeed, that they are to a certain extent subjected to the will; but it is through the intervention of other muscles, which are altogether muscles of voluntary motion. We can increase the stimulus of their contents when they are of a certain bulk, and thus call their muscular fibres into action, by gently pressing them against these contents by the action of the abdominal muscles; and every one, with a little attention, may find, that without acting with those muscles, he is quite unable, at any time, to excite the action either of the bladder or rectum. And so connected is the action of the abdominal muscles with a stimulus applied to the rectum or bladder, that when it is considerable, they are called into action against every effort of the will, in the same manner, that the whole of the muscles of voluntary motion are called into involuntary action by the operation of an emetic.

We can also, at pleasure, interrupt the action of the rectum and bladder, if the stimulus which excites it is not very powerful; but neither is this, by any direct influence, exerted on their muscles themselves, but through the medium of their sphincters, both of which are muscles of voluntary motion. It will be evident, to any one who will make the trial, that he can, at pleasure, excite, interrupt, retard, and accelerate the action of these muscles. The circumstances which lead us at first view to suppose that we do not possess this power, are, that their range of action is very limited; and their anta-

gonists, the muscles of the rectum and bladder, only acting occasionally, they remain for the most part in a state of gentle and uniform contraction, in the same way as happens to the muscles of the one side of the face, when their antagonists, those of the other side, have become paralitic; or to the flexors of the limbs, when the extensors are divided; and this state of gentle contraction is as readily increased at pleasure in the sphincters, as in the muscles of the face and limbs. We have no means of lessening it, either in the one case or the other, except by acting with the antagonist muscles. In short, the sphincters of the rectum and bladder are muscles of the same nature with the orbicularis oris; only the range of action in the two former, partly from their being of less extent, and partly from their having fewer antagonist muscles, is more limited. They are called into action for the purpose of closing the cavities of the rectum and bladder, when our sensations teach us that the contents of these cavities are discharged, and consequently are stimulated through the medium of the brain; every one feels, as I have just had occasion to observe, that he can make this effort when he pleases, unless the stimulus applied to the rectum and bladder is so great as to overcome the efforts of volition.

It appears, then, that all the muscles of the body are either muscles of voluntary motion, excited through the medium of the brain, or muscles of involuntary motion, excited by stimuli immediately applied to the muscle itself.

By the foregoing observations some light seems to be thrown on the pathology of apoplexy. If the muscles of respiration

spiration continue to act in apoplexy, while those of the limbs have lost their power, it is not that the one set of muscles is more affected than the other, but that a powerful stimulus is applied to the brain, tending to excite the one and not the other. The patient breathes in apoplexy for the same reason that he moves his limbs when synapisms are applied to his feet; a due supply of air being withheld from the lungs excites the muscles of respiration, in the same way that irritating the feet excites the muscles of the limbs; and if he continues to breathe after no motion can be excited in the limbs, it is because the stimulus which excites us to breathe is stronger than any artificial stimulus we can employ. Men have held their hands in the fire till they were burned off; no man ever voluntarily interrupted his breathing till he became insensible. But as the quantity of the stimulus necessary to excite the muscles of voluntary motion, as it acts through the brain, must be greater in proportion as the sensibility of this organ is lessened, respiration is performed more slowly in apoplexy than in health.

I speak of apoplexy, properly so called, of the disease which is induced, for example, when a man in health receives a blow upon the head; not of the disease which has obtained the same name arising from gluttony, the excessive use of wine, &c. where derangement of various kinds takes place; and the affection of the brain is often not the primary disease.

From the respiration being performed more slowly than in health, the change which the blood undergoes in the lungs is

is less perfect, and hence the blood stimulates the heart less powerfully; for it has been found, by various experiments, that by this change, whatever it be, for its nature seems by no means to be ascertained, notwithstanding all that has been written on this subject, the blood is enabled to stimulate the left side of the heart. Thus it is, that in apoplexy the pulse is affected, although it appears, from what is said in the following note, that the most severe injury the brain can sustain does not directly influence the motion of the heart. The pulse becomes slow and oppressed, because the heart must be more distended by the blood, before it is excited to contract, in proportion as the blood has become a worse stimulus. The insensibility increasing, the muscles of respiration at length cease to be roused to any action; the change which the blood should undergo in the lungs is thus wholly prevented; the heart, consequently, being no longer supplied with a stimulus capable of exciting it, ceases to beat, and the animal dies. Is there not reason to believe, that an apoplectic might be preserved for some time by inflating the lungs?

In the fœtal state, the animal lives independently of the brain, because the circulation in the placenta, which is performed by muscles of involuntary motion excited, not through the medium of the brain, but by stimuli immediately applied to them, serves the office of respiration.

Note 32.—The experiments here alluded to, I have made both on the warm and cold blooded animals. The reader will find them detailed in the Appendix to the fourth volume

volume of my Treatise on Febrile Diseases. It appears from them, that neither chemical nor mechanical irritation of the brain can directly influence the motion of the heart.

In these experiments the interruption of respiration, of course, very soon in the warm, and in no great length of time in the cold-blooded animals, weakened, and at length destroyed the motion of the heart; and when the muscles of voluntary motion were thrown into violent contractions, the heart beat more frequently. The same effects would have been produced had the muscles of the limbs been excited, and the respiration destroyed by any other cause.

It was impossible, by any irritation of the brain, to excite irregularity of motion, or in any other way directly to affect the action of the heart. Even the instantaneous and total destruction of the nervous system seemed not at all to affect the motion of this organ.

After making a hole in the cranium, and another in the lower part of the spine, in several frogs, the thorax was laid open and the motion of the heart carefully observed. A strong solution of opium in water was then injected through the hole made in the cranium in such a manner, that it passed along the spinal marrow, and part of it came out by the hole made in the spine; a mode of applying opium employed by Dr. Monro, to shew how instantaneously the nervous system may be destroyed by this drug. The animals were immediately deprived of voluntary motion, and appeared quite dead; but the motion of the heart was not in the least affected: it continued with the same frequency and vigour as before the injection of the solution.

The

The strongest chemical and mechanical stimuli were applied to the brains of rabbits without producing any effect on the motion of the heart but that which arose from the convulsions of the muscles of the limbs; and after the death of the animals, it continued to beat with perfect regularity, gradually becoming weaker, as necessarily happened in consequence of the ceasing of the respiration.

I made similar experiments to ascertain how far the muscular coat of the intestines, which, next to the heart, may be regarded as the most important muscle of involuntary motion, can be influenced by stimuli applied to the brain, from which it appeared, that no irritation of the brain could at all affect its motion; while the muscles of the trunk and limbs were agitated by the most violent spasms, the peristaltic motion of the intestines remained wholly unaffected; its degree and regularity continued the same whether the stimulus was applied or withdrawn.

It has been asserted, that the motion of the heart may be affected by opium applied to distant parts of the body, and that opium applied to the heart will affect distant parts, through the medium of the brain. These opinions seem to have been founded on mistaken inferences from certain experiments which admit of a different explanation. But to enter on this subject, and the experiments by which the opposite opinions are, as far as I can judge, unequivocally established, would lead into too long a discussion. I must, therefore, refer the reader for it to the Appendix just alluded to.

A question may here occur, perhaps:—If the peristaltic

tic motion of the bowels is uninfluenced by the state of the brain, why is apoplexy almost uniformly attended by constipation? The rectum, we have seen, is never called into action without the previous excitement of the abdominal muscles, and these are only called into action in consequence of a peculiar sensation. (31.) It is not difficult, therefore, to explain the constipation which accompanies apoplexy, where both sensation and the power of the muscles of voluntary motion are so much impaired. The contents of the rectum not being regularly expelled, the whole intestines become inactive, and their contents soon acquire a greater than due consistence by the absorption of the thinner parts.

Why the heart and alimentary canal should be insensible to mechanical and chemical stimuli applied to the brain, and yet so sensible to mental stimuli, we shall never, perhaps, be able to explain.

As it would appear from the foregoing experiments, that we cannot influence the motion of the heart by the most powerful stimuli applied to the brain, so in tracing, as far as we can, the formation of the animal body, we find reason to believe that the former organ and some of its vessels perform their functions before the nervous system exists. The heart and umbilical vessels are the first parts observed in the formation of the chick in the egg. See Malpighi, De Ovo Incub. and Harvey, De Generat Animal. And in viviparous animals, the heart is vigorous while the brain is but imperfectly formed; and, indeed, as has already been observed, in monstrous cases, where the brain is never formed. When these

observations

observations are compared with what we see in the perfect animal, that by means of the sanguiferous system the various parts of the body are constantly nourished and renewed, does it not seem probable that the power of the heart and vessels is employed in the original formation of the other organs?

NOTE 32.—That the heart is not wholly insensible, we know from the pain which sometimes attends its diseases. Thus, in the *Anatomia Practica* of Bonetus, we find cases in which considerable pain was excited in the heart from worms fixing themselves on it. Lib. II, Sect. 8, Obs. XIX, Obs. XXV, § 2, &c. In other instances, however, mentioned by the same writer, the heart sustained the most severe injuries unattended by pain. In the section just referred to, a case is related, in which a worm was found, on dissection, adhering to the right ventricle. It was the cause of death; yet the only remarkable symptom was violent palpitation returning at intervals. Obs. XXV, § 3. See also Sect. 10, Obs. VII. The same writer gives cases of ulceration of the heart unattended by pain; some unattended either by pain or palpitation. Lib. II, Sect. 10, Additamenta, Obs. II. In one instance, in which the patient died with only the usual symptoms of bilious fever, with a very intermitting pulse, a calculus was found in the heart. Lib. II, Sect. 19, Obs. XXXIX. The reader will find similar cases in Morgagni's Epistle on the Diseases of the Heart, in his work, De Caus. et Sed. Morb. But not having this work by me, I cannot refer to the particular passages.

NOTE 33.

NOTE 33.—It is said, that in fœtus born without the brain the nerves are larger than usual; and it has been supposed by some, that in such cases they perform the functions of the brain. When, however, we consider the nature of these functions, no conjecture can appear more improbable. All the phenomena of the nervous system seem to prove that the brain alone is the active part of it, the nerves being little more than means of conveying its influence in the one direction, and in the other, the impressions which excite it.

NOTE 34.—That all causes debilitating the vital system are most felt in the extreme parts of it appears, from a great variety of phenomena, and seems to arise from two causes.

1st, The excitability of the capillaries being more readily exhausted than that of the heart and larger vessels. This is very observable in making experiments on living animals. I have found, with the assistance of a microscope, that a degree of irritation, which produces no sensible effect on a vessel of, perhaps, the twentieth part of an inch in diameter, deprives the capillaries of all power. Thus, rubbing the skin, or gently warming it, will so far exhaust the power of the capillaries that they will yield beyond the healthy degree to the *vis a tergo*, and become distended with red blood. It is only in this way we can explain the result of some experiments of Dr. Fowler in his Th. de Infl.

2d, The nature of the circulation. Although the power inherent in the arteries themselves tends, doubtless, to pro-

pel the blood through them, yet it appears from the pulse that the influence of the heart is felt throughout the whole arterial system, and is a chief means of exciting its action.

A debilitating cause, then, applied to the heart, is most felt in the capillaries, because they are most distant from this organ. A debilitating cause applied to the whole circulating system, is most felt in them, both because they are most distant from the heart, and because their excitability is more easily impaired than that of any other part of this system.

NOTE 35.—The *vis a tergo* supplies the stimulus which excites every part of the sanguiferous system. The heart contracts in consequence of being distended with blood thrown into it by the veins. The arteries are excited by the blood propelled into them by the heart, and its impulse is supported by the power of these vessels, and conveyed to the extreme parts of the circulating system. Thus the capillaries are excited; and when by any cause their excitability is impaired, an increase of this natural stimulus is the most probable means of rousing them to action.

In inflammation, for example, in which the excitability of the capillaries is impaired, as may readily be seen with the assistance of the microscope, the larger vessels of the inflamed part are excited to increased action. The natural stimulus which excites the capillaries being thus increased, the inflammation is often removed.

The difference between inflammation and fever seems only to be, that in the latter the debility of the capillaries is general,

general, in the former it is confined to one part. In the one the debilitated vessels are greatly distended, because the increase of the *vis a tergo* is great, compared with the number of vessels debilitated; in the other, the whole capillaries being debilitated, the distending power bears a much smaller proportion to the resisting. When, however, the increased action of the heart and larger arteries is great, the state of the capillaries in fever approaches to that of the capillaries of an inflamed part, the various surfaces appearing redder, more turgid, and much hotter than natural. Hence it is, that in this state of the system inflammation so readily supervenes. For if any cause occurs to add to the debility of the capillaries of a particular part, distension more readily ensues than when the action of the heart and larger vessels is less excited.

Note 36.—A debilitating cause acting on the central parts of the sanguiferous system, we have seen, is soon felt in its circumference. Experience also evinces, what the general laws of the animal œconomy, indeed, would have led us, a priori, to suppose, that debility of the circumference is always more or less felt in the central parts of this system. The diminished vigour of the secreting organs, therefore, is always attended with general debility till the retained excreta have accumulated in sufficient quantity to apply a preternatural stimulus to the heart and larger vessels.

Note 37.—In the Introduction to the second part of my Treatise on Febrile Diseases, I have endeavoured to prove, by

by direct experiment, that inflammation consists in the debility of the capillaries, and that it can only be removed by restoring their action. The larger vessels of inflamed parts are in a state of increased excitement, and the additional stimulus thus given to the capillaries appears to be the means which nature uses to remove the disease. The nature of fever, I have had occasion to observe in a preceding note, seems to differ from that of inflammation only in the one being a general and the other a local affection. All the differences of their symptoms, as well as modes of treatment, may easily be traced to this cause. In the first stage of both, our view is to excite the capillaries, and thus restore the due balance of excitement between them and the larger vessels. In the second stage of these diseases, our view in both, is to restore tone to the whole vital system. When the inflammation has assumed a tendency to gangrene, and the pulse has lost its firmness, and when synocha has changed to typhus, the principle of the treatment is the same.

Note 38.—So much does the appetite depend on the presence of the gastric fluid in the stomach, that we may at will destroy the appetite, as I have found by experiment, by clearing the stomach of it. We have reason to believe, that in complete anorexia there is no secretion of this fluid. I have found the stomach in such a case wholly destitute of it.

Note 39.—This state can be regarded in no other point of

of view but as that of general, though moderate inflammation of the surfaces. Did it appear in only one particular surface, nobody would hesitate to give it this name. See Note 35.

Note 40.—The consequence of a more rapid circulation is, that the blood is sent more frequently through the lungs, and thus becomes more arterious. From this cause, and from the blood's being propelled in greater quantity into the superficial vessels during the hot fit of fever, the colour of the skin is more florid than natural. By these causes also the sensibility is increased. On the same principles, we readily account for the paleness and diminished sensibility in the cold stage of fevers, in which the powers of the circulating system being impaired, the blood becomes less arterious, and is impelled with less force towards the various surfaces. It is to be observed, that these causes operate doubly, on the parts affected and on the brain, on the state of which the sensibility of every part depends. From the latter operation arises the feebleness of the muscles of voluntary motion in the cold, and their irregular vigour in the hot stage, of fever.

Note 41.—The frequency of the breathing is *cet. par.* proportioned to the rapidity with which the blood passes through the lungs. Hence it is that the breathing sometimes becomes extremely slow towards the fatal termination of many diseases, particularly of those in which the sensibility is much impaired. I have seen a patient under these circumstances

cumstances breathe only once in three or four minutes for the last quarter of an hour of his life.

NOTE 42.—Language has been defined to be the means by which we communicate our ideas to each other; but it also seems to be the chief means of their arrangement, and even the source of a large proportion of them. We find, that those who have always been deaf, and consequently dumb, are generally of weak intellects, unless they have obtained some substitute for language; and a large proportion of them are idiots. This will not appear surprising when we consider attentively the advantages we derive from language. Words are originally the mere signs of our ideas; but, like the letters in Algebra, which are the signs of quantities, other uses are made of them besides that of merely representing our ideas. By habit, the words themselves acquire certain relations towards each other, and we use them without recalling to the mind the particular ideas which belong to them. We have once satisfied ourselves, that certain ideas bear certain relations to each other. These relations are unconsciously transferred to their signs, and an endless labour is thus saved. We are thus enabled to go through long trains of reasoning with ease, which, without such signs, would to the strongest mind be impossible. To return to the illustration of Algebra, words are used in reflection and conversation, as letters are used in equations; we have once given to each its particular signification, and we combine and separate them in a thou-

sand

sand different ways, without recalling the ideas they represent till we arrive at the conclusion, perhaps not even then. Let any one attempt to pursue a train of reasoning of any length without the assistance of words, and he will find it impossible. He will find it much the same thing as going through a long arithmetical calculation without the assistance of figures. In this way, language enables us to compare our ideas, and by supplying us with results, at which we could not otherwise arrive, is the source of many of them.

When we make no use of language in thinking, the trains of ideas pass through the mind with great rapidity, and the reasoning power is very feeble. This seems generally to be the state of our minds in dreaming. The rapidity with which ideas often pass through the mind in sleep appears, at first view, incredible. Dr. Gregory used, in his Physiological Lectures, to relate the case of a lady, who was subject to dreams while engaged in the common occupations of life; their duration was so short that they often passed unobserved by those who conversed with her; yet it required much time to give an account of the crowd of ideas which in so short a time had passed through her mind. "The rapidity of the "succession of transactions in our dreams," says Dr. Darwin, Zoonomia, Vol. I. p. 205, "is almost inconceivable; in so "much, that when we are accidentally awakened by the "jarring of a door, which is opened into our bedchamber, "we sometimes dream a whole history of thieves, or fire, in "the very instant of waking." This rapidity is too great to permit us to compare our ideas, so that the most incongruous

do

do not seem at all wonderful. We are not surprised to find ourselves suspended in the air, because our ideas pass with too great rapidity to permit us to recollect that this never happened before. But when we converse in our dreams, the charm is dispelled, the rapidity of our ideas is arrested, and the train of thought becomes as consistent with our experience as in our waking hours. This I have often remarked, and on mentioning it to others, have found they had made the same observation.

Many of the phenomena of delirium, as far as I am capable of judging, may be accounted for on the same principles. But there seems to be this difference between dreaming and delirium, that in the latter although the rapid succession of ideas is prevented, we are incapable of comparing them together so as to draw the proper inferences.

Note 42.—In considering the symptoms of fever, I have of course confined myself to those which properly belong to it. With regard to a variety of symptoms from local affections which sometimes accompany it, as these form no part of this disease, it was not necessary for me to say any thing of them. The nature of many of these local affections, however, tends to throw light on that of the primary disease. By far the most common, are inflammations of various kinds. How nearly the nature of inflammation and that of fever are allied, I have already endeavoured to point out; and their frequent concurrence seems to confirm what was said on this subject.

NOTE 43.—Hence it seems to be, that obstruction of the mesenteric glands soon occasions fever of however indolent a nature the obstruction may be, while that of other glands seldom has this effect till they become inflamed.

NOTE 44.—That opium only affects the state of the pulse in consequence of its being received into the system by the absorbents, and immediately applied to the heart and blood-vessels, is ascertained, as far as I am capable of judging, by a variety of experiments which I made with a view to determine this point, to which I have already had occasion to refer. And the strong analogy we observe, in every instance, between the effects of opium and fermented liquors, leaves little room to doubt that their mode of operation is the same.

NOTE 45.—Much has been said of the effects of cold, and on the question, whether it is to be regarded as a stimulus or sedative. The observations I have had occasion to make on the effects of agents in general on the animal body, will, I believe, be found strictly applicable to those of cold. All agents, it was observed, occasion either excitement or atony, according to the degree in which they are applied; but the degree of atony which a large quantity of any agent produces, is in no particular proportion to the degree of excitement produced by a smaller quantity of the same agent; some agents being better fitted to occasion excitement, others to occasion atony. Now if we compare the effects of cold with those of opium or wine, we should say that its stimulant bears but a

small proportion to its sedative effects. It is only within a very limited range that it acts as a stimulus: compared with these, therefore, it deserves the name of sedative. But if, on the other hand, we compare its effects with those of agents still less capable of producing excitement, of tobacco, for instance, for the same reason it deserves the name of stimulus; and in this way, I believe, all that has been said on the subject may be reconciled.

Note 46.—The circulation, we have seen, is most apt to fail on the surface of the body, both because the excitability of the extreme vessels is more readily impaired than that of other parts of the sanguiferous system, and because being most distant from the heart, they are least influenced by the impulse communicated by this organ.

Note 47.—The application of cold air to the lungs is almost always invigorating, which seems to arise from two causes; as the temperature of the lungs does not vary like that of the surface, however cold the air we breathe, it is probable that the temperature in the lungs never falls below that range within which cold acts as a stimulus, (Note 45.) and the colder air being more dense, affords a more copious supply of oxygen.

Note 48.—The leprosy of the Jews, and other species of leprosy which raged in the twelfth and thirteenth centuries, are

are scarcely now to be met with; and we have a remarkable instance both of the production and disappearance of a contagious disease, in the Ephemera Britannica, described by Caius. *(Caius, De Ephemera Britannica.)*

The gangrenous sore throat, Allionius *(Allionii Tractatio de Miliarium Origine.)* observes, was scarcely known before 1610. The scarlet fever appears to be of still later origin. Prosper Martianus, who wrote about the middle of the seventeenth century, is among the earliest writers on this disease. It soon after made its appearance in London, and was described both by Sydenham and Morton; and, indeed, so little was it known at this period, that Morton does not always distinguish it very accurately from measles.

Even the small pox was unknown to the ancients. The Arabian Physicians are the earliest writers on this disease; and of those whose works have come down to us, Rhazes is the oldest who treats of it. The oldest writer on the small pox mentioned by Rhazes, is Aahron, who resided at Alexandria, in 640 of the Christian æra. Nearly the same observations may be made respecting the measles, although some assert that this disease was not unknown to the ancients. (See the Observations of Matthiew, in *Baldinger's Sylloge Select. Opusc. Vol. IV.*) The Arabian Physicians were certainly the first who accurately described it.

Most writers agree with Allionius, that the miliary fever which appeared at Leipsic in 1652, was the first fever of this kind of which we have any account. The aphthous fever seems also to be one of late date, although the term aphthæ frequently

frequently occurs in the works of the ancients. "Nam quæ "a priscis medicinæ conditoribus aphthæ describuntur, adeo "a nostris diversæ sunt, ut toto cœlo distent." *(Ketelaer, De Aphthis Nostratibus.* Dr. Dickson *(Transactions of the Royal Irish Academy, for* 1787.) says, that with the exception of one case related by Carolus Piso, he can find no distinct mention of the pemphigus before the days of Morton. Burserius doubts, if the discase mentioned by Morton be the true pemphigus. The following description of it, however, seems to leave no room to doubt this. "Febris synocha cum vesi- "culis per pectus et collum sparsis." *(Morton, De Feb. Inflam.)* The plica polonica, as we find from the Address of the Polish Physicians to the University of Paris, also made its first appearance in the seventeenth century.

Dr. Ferriar, in his Medical Observations and Reflections, remarks; "The yaws, the sibbens, and other national infec- "tious disorders, afford strong proofs of the variety of animal "poisons: and Mr. Hunter, in his excellent work on the "lues, has given good reason for believing that new poisons "are constantly produced among the poor of great cities."

Note 49.—It is remarked by Dr. Fordyce and others, that many brute animals are subject to typhus, when crouded together in ill-ventilated places. It has been observed to arise among hogs, and more frequently among sheep.

It is remarkable, however, that typhus cannot be communicated from brutes to men, nor vice versa. "Illud præ- "terea notabile est venenum pestilentiale hominibus infestum "non

"non nocere brutis, et e contrario, brutorum pestem non "nocere hominibus." (See the Observations of Waldschmidt, in *Haller's Disp. ad Morb. Hist. et Cur. Pert Tom.* V.) Nor does it appear that one species of brute can communicate it to another. It cannot, for instance, be communicated from hogs to sheep, nor vice versa. It is curious that the contagious fevers of white people are seldom communicated to negroes; and I have been informed by West-Indians, that there are among the negroes many contagious febrile diseases to which white people are not subject.

NOTE 50.—When the power of the contagion is very great, the system sometimes seems incapable of any effort; the retained excreta produce no increased excitement of the heart and larger vessels, their excitability seeming almost instantaneously destroyed by the action of the contagion. This has sometimes happened in the plague. It is almost unnecessary to observe, that such cases terminate fatally in a very short time.

NOTE 51.—Dr. Lind remarks, that he has often seen these observations confirmed in sailors after they had been for some time on shore. The fever, which seemed at first to be merely the effect of a debauch, or some such cause, soon assumed the precise form of that which raged in the ship they had left. They are farther illustrated by the observations of other writers, and particularly by what Dr. Rush says

says of his own situation while the yellow fever raged at Philadelphia. See his Account of this fever.

Note 52.—The effects of any injury done to the body depend as much on the nature of the part affected as upon the kind and degree of the injury. An extensive inflammation, for instance, may exist in the skin without much general derangement, but the slightest degree of inflammation in the stomach and bowels is immediately felt in every part of the system; and so true is this observation, that from the degree of general derangement which ensues on an injury being done to any organ, we might very accurately estimate its importance to the life of the animal. Thus affections of the mouth occasion less general derangement than those of the pharynx; of the pharynx, less than those of the œsophagus; of this organ, than of the stomach, which is the most important part of the alimentary canal. A similar observation applies to the affections of this canal, if we trace them from its opposite extremity to the stomach. Diseases of the rectum occasion less derangement than those of the higher parts of the great intestines; diseases of these, less than diseases of the small intestines; and of these, less than diseases of the stomach. The same is true of the diseases of the head and thorax; the more vital the part affected, we still find the general derangement the greater.

Note 53.—The time required for the contagion of typhus

typhus to produce its effects is very various. Sometimes, though very seldom, they are almost immediate. In general the infected feel no symptom of the disease for two, three, or more days, and in some cases, but much more rarely, even for weeks after they receive the contagion.

Note 54.—As all the organs of the animal functions are supported by the action of the extreme parts of the vital system, which, we have seen, are always debilitated in fever, we are at no loss to account for the injury occasioned by every thing which tends to excite these organs, which cannot be done without calling into action, and thus farther exhausting, the capillaries.

Note 55.—It appears, from experiments related in the Appendix to the third volume of my Treatise on Febrile Diseases, which I originally made with a view to ascertain the circumstances that occasion a deposition of lithic acid from the urine, that from the state of this fluid we may ascertain the degree of vigour which prevailed in the skin during the time of its secretion. It appears from these experiments, that when the skin is inactive the urine deposits a large proportion of lithic acid, that as we increase the action of the skin the deposition of the lithic acid is lessened, and that when the skin is most vigorous no lithic acid is ever spontaneously deposited from the urine, however long it is kept.

From the same experiments it also appears, that the deposition of lithic acid from the urine is more effectually pre-

prevented by the medicines which increase the insensible perspiration than by those which occasion sweat. The following are the observations subjoined to the experiments with diaphoretic and sudorific medicines. It is remarkable, that a small dose of tartar emetic more certainly prevents the deposition of lithic acid from the urine than a large one of Dover's powder, although producing a copious sweat, which may be accounted for in the following manner. It will appear, from what will be said hereafter, that the secretion of the matter which occasions the deposition of the lithic acid from the urine, depends not on the mere relaxation of the kidneys, but on their vigorous action. I should imagine that the same thing takes place in the skin, and that this matter is only separated by it, and thus prevented going off by the kidneys, in proportion to its activity; for it will afterwards appear, that the matter occasioning the deposition of lithic acid from the urine, also passes by the skin; and, indeed, from the experiments already related, we can hardly suppose otherwise. Now the effect of Dover's powder must in a great measure be attributed to the relaxation induced on the skin by the opium it contains; whereas the antimony seems only to increase the natural action of the skin.

To the same circumstances we must also attribute another difference in the effects of these medicines on the urine; while the Dover's powder, for the most part, produced no effect on it, after the sweat had ceased to flow, the antimony continued for several days after it was taken, in a greater or less degree, to influence the state of that secretion. I have also

also repeatedly observed, that the deposition of lithic acid from the urine was not so effectually prevented by this medicine when it produced nausea, as when it produced no sensible effect; which is to be explained on the same principles.

Note 56.—Both the external and internal use of cold water in fever, was known to the ancients; but it is only lately that the former practice has been revived and demanded much attention. Washing the body with cold water in fevers, is said to have been first practised in modern times, at Breslau, in Silesia; (See a Dissertation entitled, *Epidemia Verna, quæ Wratislaviam, anno 1737, afflixit, in Act. Nat. Curios. Vol. X.*) and it appears that the practice was followed in some of the neighbouring countries. The external use of cold water in fevers, however, has never been prevalent in Europe, perhaps, as Dr. Currie supposes, from the manner in which it should be regulated not having been understood.

Several late practitioners in warm climates, particularly in the West Indies, have employed it freely; and in 1786, Dr. William Wright, who had practised for many years in the island of Jamaica, gave an account of some cases of fever successfully treated by the affusion of cold water, in the London Medical Journal. Dr. Wright has since published some additional observations on the same subject, in a Letter to Dr. Garthshore, in the seventh volume of the Medical Facts and Observations, in which he gives an account of Dr. Gregory's manner of employing this remedy, and the

success which attended its use in the Royal Infirmary of Edinburgh; and in the second volume of Annals of Medicine, he again gives a favourable testimony of its effects in the fevers of the West Indies. Dr. Jackson, (See *his Treatise on the Fevers of Jamaica.)* and others, also employed it. Even the alternation of the warm bath and affusion of cold water has been practised in fevers, and it is said with good effect. But no other writer has bestowed so much attention on the external use of cold water in fevers, and so accurately observed its effects, as the late Dr. Currie. See his Treatise entitled, *Medical Reports on the Effects of Water, Cold and Warm, as a Remedy in Fever and other Diseases.*

Note 57.—Dr. Hamilton gives the following account of his practice, and of the circumstances which led to it. Speaking of the *calx antimonii nitrata,* he observes: "This "antimonial remedy was not ineffectual; but I remarked, "that it was beneficial only when it moved the belly. The "stools were black and fetid, and in general copious. On the "discharge of these, the low delirium, tremors, floccitatio, and "subsultus tendinum, which had prevailed, abated in some "cases; the tongue, which had been dry and furred, became "moister and cleaner; and a feeble creeping pulse acquired "a firmer beat.

"Reflecting afterwards on these circumstances, it oc- "curred to me, as the purgative effect appeared to have been "the useful one, that any purgative medicine might be sub- "stituted

" stituted for the *calx antimonii nitrata*; and that by this sub-
" stitution, the unnecessary debilitation of an exhausted pa-
" tient, by vomiting and sweating, might be avoided.

" More extended experience confirmed these conjectures;
" and I was gradually encouraged to employ purgative me-
" dicines early in typhus, and to repeat them in the course
" of the disease. And after having long and strictly directed
" my attention to this point of practice, I am now thoroughly
" persuaded that the full and regular evacuation of the bowels
" relieves the oppression of the stomach, and mitigates the
" other symptoms of fever." See *Observations on the Utility and Administration of Purgative Medicines in several Diseases, by James Hamilton, M. D. &c.*

I might, from my own experience, were it necessary, add many cases and observations in confirmation of these remarks. But it is sufficient to refer the reader to the Appendix to Dr. Hamilton's work, where he will find a sufficient number of cases to illustrate the effects of the practice there recommended. I shall therefore only add, that I have very generally seen good effects from spontaneous diarrhœa, even in the worst forms of typhus; an observation which obtruded itself upon me many years ago, when, with most other Physicians, I was prejudiced against the use of cathartics in this fever, except as far as was necessary for the regular expulsion of the fæces.

Note 58.—It is very remarkable, that in a disease of so much debility as typhus, blood-letting seems always to have

have been a favourite remedy. Galen and Celsus speak of it in the highest terms. Prosper Alpinus informs us, that the Egyptians let blood in all putrid diseases. Among the moderns, if we look into the works of Sydenham, Hoffman, Huxham, Mead, Hassenhorl, Eller, Pringle, Monro, and indeed all the best Physicians of their days, we shall find, that they recommend too indiscriminate a use of the lancet in fevers. Yet there is hardly any of these writers from whose works we may not collect sufficient evidence of its hurtful effects.

Huxham, in his work on Fevers, observes: "The first "blood in malignant fevers frequently appears florid; what "is drawn twenty-four hours after, is commonly livid, black, "and too thin; a third quantity, livid, dissolved, and sanious. "This is frequently the case in malignant fevers. I have "sometimes observed the crasis of the blood so broken, as to "deposit a black powder like soot at the bottom, the superior "part being a livid gore, or a kind of a dark green, and ex-"ceedingly soft jelly. Besides, the pulse in these cases, sinks "oftentimes surprisingly after the second bleeding; nay, some-"times after the first; and this I have more than once noted "to my great concern and astonishment, and that even when "I thought I had sufficient indications from the pulse to draw "blood a second time." In his Essay on the Ulcerous Sore Throat, he observes: "I have very often met with this buffy, "or sizy appearance of the blood in the beginning of malig-"nant fevers, and yet blood drawn two or three days after, "from the very same person, hath been quite loose, dissolved, "and sanious as it were; too many instances of this lately

"occurred

"occurred to me among the French captives here, who died "by dozens of a pestilential fever. In this fever the French "Surgeons bled every day, or every second day; and I "several times saw the blood of some of the officers a mere "sanious gore, on the third or fourth blood-letting."

On reading observations of this kind, we are inclined to ask, what were the advantages expected from blood-letting in these fevers, that such consequences should have been risked? To this question we have no answer but that which the favourite hypothesis of the time afforded, which taught, in opposition to every day's experience, that the cause of fever might be as it were drained off by blood-letting.

NOTE 59.—Mr. Clark, in his Observations on the Diseases of Long Voyages to Hot Climates, after relating the fatal terminations of three cases, in which blood-letting was employed to lessen violent excitement at the commencement of the remittent of sultry climates, observes, that he has since found it necessary to lay aside blood-letting in these climates, both at sea and on shore, except in cases of local inflammation.

NOTE 60.—The present practitioners of tropical climates, I have been informed, use cathartics more freely in fever than was done by most of those who have written on the fevers of these climates. On the cathartic effects of calomel in particular they place great reliance, and, indeed, seem often to trust almost solely to it. (57.)

NOTE 61.

Note 61.—When we see a patient labouring under symptoms of extreme debility, and find these symptoms almost uniformly relieved by a considerable quantity of wine, it is difficult, at first view, to persuade ourselves that the wine is pernicious; but an attentive observer will look beyond its immediate effects, and will then readily see sufficient reason to doubt the safety of this practice. He will find, that the temporary excitement he thus procured is succeeded by a greater degree of debility than that which the stimulus had removed, and if he perseveres in this plan, that in a large proportion of cases the pulse, upon the whole, will gradually become more frequent, and feeble, till it ceases altogether. These effects I have so often witnessed, that I cannot help thinking that almost any fever may be rendered fatal by a certain quantity of wine. And when we recollect that the excessive stimulus of wine is a frequent cause of fever, can we be surprised that the constant repetition of this stimulus should increase its symptoms?

Beside the apparent good effects of wine for a short time after its exhibition, Physicians have been led to an excessive use of it in typhus by another observation, the comparatively small effects it produces. That a pint of wine in typhus will not produce a greater effect than a glass in health, is adduced as an argument for the pint in the one case being as innocent as the glass in the other. But it is to be recollected, that wine in typhus, only produces less excitement than in health, in proportion as the remaining excitability is less, and, consequently, that a degree of excitement which would

would occasion little or no inconvenience in health, may produce a fatal exhaustion in typhus. Here there is no excitability to spare, and the first principle of the treatment seems to be as much as possible to prevent its farther exhaustion. A very moderate and uniform exhibition of stimuli seems often necessary, that the action of the central parts of the sanguiferous system may not fall too low to support that of the circumference, but all excitement beyond this seems to have no other effect but that of exhausting the little vigour which yet remains. See the Observations of Dr. James Hamilton on the Use of Wine in Typhus, in his work on Purgative Medicines.

There is a case, which, at first view, may be mistaken for the second stage of fever, in which I have witnessed excellent effects from powerful stimuli, that in which spontaneous grangrene appears in one or more parts, from the failure of the vital principle in particular parts of the capillary system. But here the various secreting organs do their office, and the action of the heart and larger vessels often differs little from that of health. In short, there is no general debility of the vital system, and there seems not the same risk in exciting it to increased action in order either to restore to vigour, or entirely to throw off, the debilitated part.

Note 62.—As in typhus the circulation, from the feebleness of the powers which support it, must be comparatively slow, the blood must pass less frequently through the lungs, and consequently be less subjected to the action of the oxy-

genous

genous part of the atmosphere than in health; and that this is really the case, appears from its being more venous. The effects of the less frequent passage of the blood through the lungs would probably, as far as respects the change occasioned by the oxygenous part of the atmosphere, be counteracted by supplying the patient with air containing a larger quantity of oxygen, in proportion as the circulation is less rapid. What effect correcting the venous state of the blood in typhus might have, it is difficult to say.

Note 63.—I have seldom found the powder of bark, although more efficacious than the infusion, tincture, or extract, well suited to the common continued fever of this country. It is apt to oppress the stomach, disorder the bowels, and prevent the return of appetite. When symptoms of malignity, and particularly when a tendency to gangrene appears, it seems to be a valuable medicine, and should perhaps be given in as large doses as the stomach can easily bear. In whatever form bark is given, its good effects seem generally increased, and it is rendered more grateful to the stomach by giving along with it some of the mineral acids, particularly the sulphuric acid, if there is a tendency to sweats which do not relieve the symptoms.

Both the mineral and vegetable acids, indeed, deserve the name of tonics; and in fever they sometimes seem more beneficial than any other we possess. In eastern countries the cure of fever is often trusted wholly to the juice of acid fruits.

When

When the appetite does not return readily, and the patient continues subject to renewals of feverish heat, I have found the more simple bitters, the gentian, for example, answer better than either the bark, or colomba.

NOTE 64.—We should not err much, I believe, by laying it down as a general rule, not to permit the use of animal food in any form while the pulse retains a considerable degree of frequency, and the patient continues subject to returns of feverish heat, whatever be the state of the appetite. I have found the disease more tractable since I followed this rule. Recovery seems to go on more uniformly, and the return of the strength, so far from being delayed, seems to be promoted by it, not to mention that the risk of relapse is much lessened. Dr. Fordyce, in one of his Dissertations on Fever, makes some excellent remarks on the use of solid animal food towards the favourable termination of fevers. "Even "after the disease has been terminated by a crisis, animal "food, in a solid state, should be rejected, there being no "cause which has produced relapses, so far as the author's "experience has gone, so frequently as using solid animal "food too soon. Supposing even that a complete crisis should "have taken place, and entirely terminated the disease, it "ought to be at least five or six days before any solid animal "food is ventured upon.

"The author wishes to press this more strongly, because "if a perfect crisis should take place, the appetite often re-"turns, and the patient is left in a very weak state. It has

" in this case been often conceived by the patient, and much " more frequently by the by-standers, that solid animal food " would restore his strength soon. It must, however, be " remembered, that when a complete crisis takes place, and " carries off the fever entirely, the depression of strength, " which was a symptom of the fever, ceases, and the weak- " ness which was produced by the exertions and derange- " ments of the faculties of the system, is no longer increasing; " and that the patient, with very moderate nourishment, and " the sleep and rest, which are so apt to ensue after the fever " has been completely carried off, will have his strength re- " stored in a very short time, without using any thing that " shall run any risk of re-producing the disease."

Although the patient should be restricted both with respect to the quality and quantity of his food, he should be allowed to eat as frequently as he pleases; and when the appetite begins to return, he should be reminded not to be too long without taking something. This both prevents the risk of his eating too much at one time, and the lowness which generally attends an empty stomach when there is any appetite. " Perhaps," says Sir John Pringle, " there is no rule " more necessary than never to let the patient, when low, " remain long without taking something cordial or nourish- " ing; as I have seen men, once in a promising condition, " sink past recovery, by being allowed to pass a whole night " without any support about the time of the crisis." See Sir John Pringle's *Treatise on the Diseases of the Army*.

FINIS.