

NATIONAL LIBRARY OF MEDICINE
Washington



Founded 1836

U. S. Department of Health, Education, and Welfare
Public Health Service

REMARKS

ON THE

BRUNONIAN SYSTEM.

BY JAMES JACKSON, A.A. & M.M.S.S.

Natura sui juris est, ac longius, latiusque patet, quam ut certos ei fines, angustosque
humani ingenii terminos constituamus, extra quos egredi non possit. *Baglivi.*

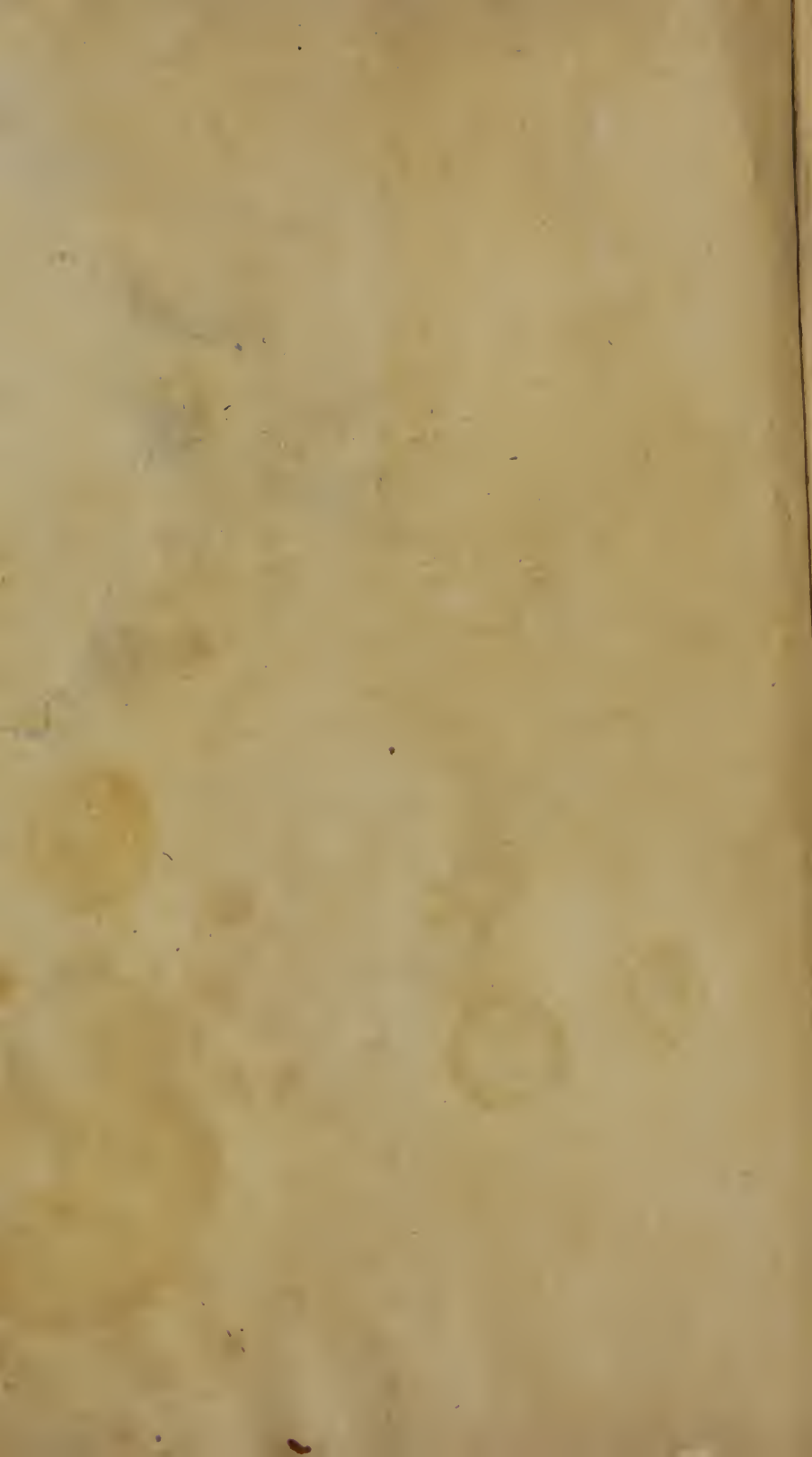
L'hypothèse facile de Theison se signale aujourd'hui sous un autre nom, par ses
imprudences et ses homicides. *Federer. Introduction de Phytolog. Posit.*

BOSTON :

PRINTED BY THOMAS B. WAIT & Co.

COURT-STREET.

1809.



AN
INAUGURAL DISSERTATION,

FOR THE

DEGREE OF DOCTOR OF MEDICINE,

READ AND DEFENDED

BEFORE THE REV. PRESIDENT AND MEDICAL PROFESSORS
OF HARVARD COLLEGE,

AT A PUBLIC EXAMINATION,

On the 25th day of August, 1809.

TO EDWARD H. HOLYOKE, ESQ.

M.D. A.A. & M.M.S.S.

DEAR SIR,

IN compliance with custom, this little work must have a dedication, and I cannot hesitate a moment to whom this dedication shall be made. By you I was taught to pay a sacred regard to experience, as the source of all medical knowledge; and by you I was forbidden to resort to speculative principles, as guides of practice, except only when experience failed, while humanity still prompted an effort to give relief.

Through a long course of years, while successive medical theories have had their rise and fall, you have watched the course of nature in the diseases of the human body, and with equal prudence and zeal have been her minister in their cure.

Should I point out the causes, which have made you so much respected and beloved among all who know you, your modesty would be offended. They are the same which induce me to seize this public opportunity of expressing my gratitude to you as my first guide in the study of medicine, and of assuring you that I am

With the highest respect,

And the most sincere affection,

Your obedient servant,

JAMES JACKSON.

Boston, August 31, 1809.



PREFACE.

IT has frequently been asserted, that "Brown's Elements of Medicine" opened the eyes of physicians to new and highly useful views in their science, and that the publication of his work marks an important era in the history of medicine. Accordingly, the improvements in medical practice, within the last thirty years, have been attributed to that work, as their source. It is because I do not assent to these opinions, that I have been induced to make this work the subject of the following pages. If indeed Brown had opened the eyes of others to useful views, we might rejoice that he wrote, although his own descriptions of those views were false. But is it not more true that he wrote when the wisest physicians had already entered the path of truth, that of observation, experience, and induction; the path which Hippocrates and Sydenham had trod with so much success; and at a period when all were desirous to follow in this path, although many were discomfited, because it was rough and rugged? Was it not at this moment that Brown, professing to follow Bacon according to the good new fashion, declared, that by the discovery of one principle he was able to explain all

the secrets of physiology and medicine ? It was thus, professing to confine himself to observation, he, in fact, imitated all theorists in distorting all the other phenomena of nature to accord with those few which he had observed.

The following pages do not contain a full and critical examination of Brown's elements. The detail of his system is not insisted upon by many of his admirers ; but they all contend that his fundamental principles are sound and incontrovertible. I have, therefore, confined myself to the consideration of those principles. Yet I cannot pretend to have examined them thoroughly, and the following is offered as containing only some remarks on them.

To any man who reads these remarks with candour, I think it will appear that I have engaged in this theoretical discussion, not from a love of theory, but from a desire to oppose a system, which, above all others, discourages a slow and careful observation of facts, and a fair induction of principles from them.

According to Brown, it is useless to record the phenomena of disease, to collate and compare cases ; for these phenomena, these symptoms, are fallacious. Disease has no distinction but that of degree. The actions of the body, affected by disease, are specifically the same as those in health ; the distinction of health and disease is arbitrary ; disease exists when the actions are above or below

those of health; and this point, this health, is to be determined only by the feelings of pleasure which accompany it.

This is directly in opposition to the plan of observation and experience, which many have thought to be the foundation of the science of medicine; to that plan which only has ever enabled physicians to cure diseases; and by which some men in all ages have been qualified to render more or less service to the sick, in spite of the various systems which the fashions of various ages have rendered popular. But to gain knowledge on this plan, requires labour; while a theory, such as Brown's, leads the student to believe, that his labour and study have arrived at their ultimatum.

“*Enimvero cum methodica hæc medicinæ tractatio artem ostendet integram, et quasi redactam ad metam, ultra quam progredi non possit; non solum homines minus industrios reddit ad nova indies investiganda quæ morborum historiam illustrent ac locupletant: verum ita sui certos, ut se hac in arte numeros omnes absolvisse existiment.*”



REMARKS
ON THE
BRUNONIAN SYSTEM.

THE age in which we live is distinguished by freedom of opinion. If every man does not think for himself, every man thinks as he pleases, unawed by authority. In the science of medicine, particularly, there is not any master whose name sanctions opinion; there is not any doctrine, the adherence to which ensures to us exclusive advantages; nor does any general disreputation attach to us for our faith, whether we follow Cullen or Brown.

ENJOYING these privileges, it becomes us to remember, that liberty, without knowledge and without restraint, leads to licentiousness. We cannot attain the benefits which our liberty exposes to us, unless, by laborious study and investigation, we seek out the truth. We must submit our principles to examination, and consider our opinions valuable, only when they are supported by experience and sound reasoning.

IF at this day the doctrines of any one teacher prevail in the medical world, more than those of any other, they are the doctrines of the late Dr.

John Brown, of Edinburgh. It would seem that the many practitioners are followers of Brown, almost without knowing they are so. Shall it be said, that those modes of practice which are most easily learnt, are, for that reason, the most readily adopted? Or, is it that mankind fondly embrace principles which are simple, specious, and boldly promulgated? Or, otherwise, is it true, that these doctrines merit the general acceptance which they have obtained?

THE importance which this question has acquired, has induced me to offer some remarks on the Brunonian Theory, and to endeavour justly to weigh the merits of its principles. A thorough examination of the subject would not be possible within the limits of this dissertation.

IT is not by a love of theory that I am led to this discussion. It is only by a wish to examine the principles which influence a large class of medical practitioners.

THIS discussion, following the subject of it, must be in a great measure metaphysical. In such discussions there is always a difficulty, almost insuperable, arising from the imperfection of language, and from our inaccurate use of it; from using words in different senses, without any definite ideas, or without any at all. I may certainly be permitted to say, in the outset, that in the writings of Dr. Brown there is not a small difficulty of this sort. For instance, in his seventy-second paragraph he says, that "life is not a natu-

ral but a forced state." What we are to call natural, if not life, it must puzzle every philosopher inferior to Brown to determine. It is impossible that any man could have written this sentence, who used the words *natural*, and *life*, according to their common acceptation.

I SHALL endeavour to meet this difficulty, respecting the use of words, in the best manner in my power; and shall rather submit to tedious periphrasis, than employ terms whose limits are loose and undefined.

IN Beddoes' edition of Brown's Elements of Medicine, we find his most general propositions reduced under eight heads. In these remarks I shall refer to those propositions; and, while I derive some advantages from the labours of Dr. Beddoes, the reader is assured of an analysis more ably and faithfully performed than I could furnish.

INTRODUCTORY to those propositions, Dr. Beddoes has these remarks: "The cause of *gravitation* has been left unexplored by all prudent philosophers; and Brown, avoiding all useless disquisitions concerning the cause of *vitality*, confines himself to the phenomena which that great moving principle in nature may be observed to produce."

IN all this Brown certainly acted wisely, and like a true philosopher, if so indeed he did act. No one can engage with advantage in physiological and pathological investigations, who does

not adopt this rule. It accords with the principles of the great Bacon, and was followed by many philosophers before Brown's day. Among the professors of our art, who had been distinguished in this respect, I might mention Haller, Whytt, the two Hunters, and George Fordyce. If I mistake not, it is to the labours of these men and of their contemporaries, that might be attributed the revolution in medical opinions, within the last half century, quite as much as to Brown.

WE will now consider the first proposition, as laid down by Beddoes.

“To every animated being is allotted a certain portion only of the quality, or principle, on which the phenomena of life depend. This principle is denominated *excitability*.”

HERE are, in fact, two propositions: First, *there is* a quality, or principle, on which the phenomena of life depend; and this principle is, by Brown, denominated *excitability*. Second, of this principle a certain portion only is allotted to every animated being.

THAT all the phenomena of life depend on some single and simple principle, is an idea very grateful to most physiologists; just as it has been popular to suppose life originating in and flowing from some particular organ in the body. It is very possible that there may be some such principle. If such a principle were known to exist, it would often times be convenient to refer minor principles, which we discover in living bodies, to this as their source. It would be convenient; but

that is the only advantage, for we gain nothing by knowing that there is such a principle, unless we know the laws of its operation in producing those peculiar properties which we discover in living bodies. If we do not know those laws, we are then left to investigate the properties of living beings by experiment. When we have thus gained knowledge respecting those properties, it is convenient to refer them to some general principle as their source. But we derive no other advantage, since we cannot go to this unknown principle as the source of any other knowledge, and its name is only one word to express a combination of several ideas. To say, for instance, that sensibility is to be attributed to the vital principle, is no more than to say, that sensibility is one of the properties of some living beings.

BROWN represents this excitability as a principle common to all living beings, vegetable as well as animal. This only increases the difficulty. Indeed it is impossible that there should be one *simple, unmodified* principle, which is essential to vitality, and from which all the phenomena of life flow, common to all living beings. For assuredly there is a great difference in the vital properties, not only of animals and vegetables, but of different animals from each other; and likewise of different vegetables from each other.

I AM not contending that there is not some principle, or property, common to them all; but that this common principle is not, so far as we

know, the source of all those properties which are peculiar to them as living beings. In short, the properties of all of them are discoverable only by observation and experiment; and we must, in the present state of our knowledge, regard these properties as ultimate facts. Our analysis has not led us to any more simple elements; and we give no aid to philosophy by referring them to an unknown, undescribable source. We cannot say that the principle of life consists in the combination of them all; for some beings, who are universally admitted to have life, as is the case with every vegetable, exist without possessing them all. We cannot say that this essential principle consists in certain of them only; for the others cannot be traced from any which can be selected for this purpose. Yet those others are equally properties of life; or if not, what are they? Irritability and mobility are common to every living being. But if these are selected as constituting the principle of vitality, how shall we trace, from them, sensibility, and the various other properties which are found in quadrupeds.

FROM the whole we are authorised to infer, that, although there may be some single principle from which all the phenomena of life arise, and even though the existence of this principle is so probable, and so generally believed, as to make it convenient to refer to it, in the same manner as if it had been actually demonstrated; yet, in the present state of our knowledge, we cannot deduce

from this principle any thing except what our actual knowledge of the individual properties of living beings will authorise. To prevent inconvenience, this imaginary principle will be referred to in these observations; and sometimes under the name given to it by Dr. Brown. This name is excitability.

IF it were certain that there is such a principle, although its nature be not well understood, it might be lawful to give to it any arbitrary appellation for the convenience of referring to it. But such a principle should not receive a name which implies any peculiar character, unless that character certainly belongs and is appropriate to it. Brown employs the term *excitability* as synonymous with the principle of life. In the fifteenth paragraph of his Elements of Medicine, taken in connection with some others which precede, we ascertain the extent of signification in which the term is employed. The exciting powers, he says, act on the excitability, and “the effects common to all the exciting powers are sense, motion, mental exertion, and passion.” It would seem, then, that under this name of excitability he includes those various properties on which depend sense, motion, &c. These properties are known by the names of irritability, mobility, sensibility, perception, memory, judgment, volition, imagination, and passion.

HE thus makes the most of the term, which its original signification will permit. But, thus ex-

plained, does it include all the properties which belong to life? If it does, the properties not enumerated must be such as may be traced from those enumerated, as their cause. Let us then inquire, is it owing to the irritability, mobility, sensibility, or to any of the mental powers in our systems, that the materials of our bodies resist those chemical laws which govern the composition of dead matter? Again, how do any of these properties, or that which comprehends them, the excitability—how do these account for the processes of formation, digestion, and secretion?

OF digestion, for instance, it is believed that it is performed in one of two ways; either by a direct action of the stomach upon the food, or by a fluid secreted by the stomach, the secretion of which must depend on some action of the secretory vessels. It depends, then, in either case, ultimately, on some action of the solids. Is this action to be accounted for by any operation on Brown's principle of excitability? Does any man believe that this is a simple motion of parts, differing from other simple motions only in quantity? Would it not follow, if this were true, that it would be possible, by a proper adaptation of stimulus, to make other organs perform the same office? Some further observations applicable to this subject will be made under another head. At present it does not appear that Brown's principle of excitability, considering it such as the name implies, or such as it is made to be by an expla-

nation derived from himself—it does not appear, that this is a principle on which all the phenomena of life can depend.

THE second part of the first proposition, stated by Beddoes, is as follows : Of this principle (excitability) a certain portion only is allotted to every animated being.

To this proposition many of Brown's disciples do not adhere ; nor indeed was he able to explain and apply his own principles in accordance with it. Take his eighteenth paragraph, in which this proposition is stated. " We know not what excitability is, or in what manner it is affected by the exciting powers. But, whatever it be, whether a quality, or a substance, a certain portion is assigned to every being at 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 nature of the subject, partly to the novelty of this doctrine, and partly to the poverty of language, that the phrases of the excitability being abundant, increased, accumulated, superfluous, weak, not well enough sustained, not well enough exercised, or deficient in energy, when enough of stimulus has not been applied ; tired, fatigued, worn out, languid, 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 been applied in neither excess nor defect—will be employed in the course

of this work. Both upon this, and every other subject, we must abide by facts; and carefully avoid the slippery question concerning causes, as being in general incomprehensible, and as having ever proved the bane of philosophy.”

IF then we are to abide by facts, and if Doctor Brown cannot find, nor form, any language by which he can describe the different states of the vital principle without speaking of its abundance, increase, waste, and deficiency, of its having been exhausted, and having accumulated, shall we deny that it has, in fact, an occasional increase and deficiency—that it fluctuates? Indeed, were we to deny this, could we account for the alternate vigour and debility which we see men possess? What are the facts to disprove that this secret principle is continually formed, or renewed, within us?—that it is increased by some process *in* the system, as well as exhausted by the operations *of* the system?

IF there are not any facts to oppose this supposition, and if the phenomena are best explained by it; still more, if it can be fairly inferred from the phenomena, we must prefer it to the hypothesis of our author; an hypothesis which he cannot employ in the explanation of facts in any known language, which is consistent with it.

FOR the formation of the vital principle in the first individuals of every species of being, animal or vegetable, we must refer to the fiat of Deity. But it is maintained by physical laws which he

has ordained ; by similar laws it is communicated from parent to offspring. Or will it be said that, in each case, it is given at a certain period by a new command from Heaven ? This supposition, if it cannot be positively denied, will probably have few adherents ; and they will find it difficult to form arguments in its favour. It need not detain us. At what period, then, is this “ certain portion only of the principle of life ” communicated to this offspring ? and how ? Is a certain quantity given from the parent at the moment in which the seed, or embryo, is formed ? If so, we must suppose, either that it is formed by that parent, or that it has been derived, through all his progenitors, from the father of his race. The last supposition is too absurd to be considered. If the first supposition is adopted by the Brunonians, they admit that this principle can be formed in, and furnished by, living beings.

LET us then take it for granted, that it is formed in living beings. This will not be denied by the disciples of Brown. But, upon this hypothesis, every being has the power to form this principle for another, in the quality of parent, although no one can form it for himself, or for his own personal use. If every being can form it, does it seem most probable that he forms it in the act of procreation, and then only, or that he continually bestows it upon that matter, which, as respects its material composition at least, he assimilates to himself ?

Again, according to this hypothesis, Dr. Beddoes has well observed, that "living beings ought to have proceeded through langour to death in one unbroken tenour of wakefulness." For why should we sleep to renew our vigour, if the principle of life is constantly wasting within us? And why, as Brown himself directs, why are we to diminish the use of stimuli for a season, that they may afterwards produce more effect? In this way, indeed, the ratio of waste is diminished. We may not fritter away life so fast in rest, or sleep, as in exercise; but we still rise from our slumbers with less of life than we had when we laid down; and should in vain hope that the stimuli of yesterday would suffice for to-day. All the fine stories of renovated nature would be tales of fancy, such as the lover of Aurora had never realized. We must constantly, daily, increase the force or the quantity of stimuli, or our excitement would be constantly diminishing.

THE second general proposition stated by Beddoes is as follows:

"THE excitability varies in different animals, and in the same animal at different times. As it is more intense, the animal is more vivacious, or more susceptible of the action of exciting powers."

THIS proposition will not be disputed, except so far as it contains, or relates to, principles already contested. But, after this exception, we shall leave nothing in the proposition peculiar to Brown's system.

THE third proposition by Beddoes :

“EXCITING powers may be referred to two classes.

1. External, as heat, food, wine, poisons, contagions, the blood, secreted fluids, and air.
2. Internal, as the functions of the body itself, muscular exertion, thinking, emotion, and passion.”

IF this proposition be not perfectly accurate, it is generally true ; but as was remarked of that preceding it, it cannot be considered as peculiar to the Brunonians. We shall not have occasion to make the same remark as to the next proposition, which is the fourth, as stated by Beddoes.

“LIFE is a forced state ; if the exciting powers are withdrawn, death ensues as certainly as when the excitability is gone.”

THIS is the peculiar doctrine of Brown. At least he so considers it, and deems it a grand discovery, and has founded his system upon it. He believes that life is a very unnatural state ; that we live only by compulsion ; that our elevation above the inanimate parts of creation is not according to nature ; but may be compared perhaps to that of the capital of a pillar, which is supported in its strange state by the materials below it, and which must fall, either when they are taken away, or when it, crumbling to pieces, loses the capability of being supported.

ONE would think that whatever opinions a great philosopher might advance, he would not express them in terms which render them ridiculous. But Dr. Beddoes omits to say, in Brown's own words, that life is not a natural state, and

only tells us it is forced. Let us take also the illustration which Beddoes quotes from one of Brown's disciples.

EXCITABILITY is considered as fuel; the stimulus corresponds to the air which maintains combustion; and combustion illustrates excitement. Disease consists in too rapid, or too slow, combustion. The remedy, therefore, is to supply more or less air; that is, to increase or diminish the quantity of stimulus. At last, indeed, the fire will go out, either because the supply of air is withheld, or because the quantity of fuel originally furnished is expended.

ALTHOUGH this illustration may be received as proof, by the disciples of Dr. Brown, we must be permitted to consider what difficulties occur in receiving the doctrine.

AGREEABLY to this doctrine, there cannot be life where there is not *excitement*; that is, if I understand it, *action*. That which has the capacity to be *acted upon*, in such a manner as that excitement may be produced, has not life. But, when it is acted upon, then life exists. Now, is it intended to say, that substances which have not life, which are therefore inanimate, can be made to live by stimulus? This must be maintained by the Brunonians, or how will they explain the natural history of an egg, or of a seed? or how account for the restoration of an animal which has been immersed in water, and in whom all action has been suspended?

I AM aware that, to the last question, the answer will be made by a presumption that action has indeed been continued, although so feebly as to elude our observation.* I shall only reply, that this is presumption against observation. But as to the egg and the seed, it will not be pretended that action is maintained in them; nor will it be denied that they possess life by any one who “abides by facts, and carefully avoids the slippery question concerning causes.” But if, in one case, life *exists without action*, it cannot be said, in any case, to *consist in action*. It may, notwithstanding, be true, that in complicated systems, life cannot be long maintained without action.

BUT it is needless to refer to the egg, or the seed. In every living being there must be some commencement of life. Now, on Brown’s hypothesis this commencement of life is owing to the stimulus applied to a body previously inanimate. Would it not better accord with truth to say, that life consists in one or more properties, communicated by one portion of matter possessing those properties, to another portion which had not possessed them. From this statement we can derive

* SUCH an answer could not be made consistently with Brown’s principles. For if, under such circumstances, life were not extinct in his sense of it, the temporary absence of most stimuli ought to make the system more sensible to those which are present. Now, there would be present blood and secreted fluids, if no other stimuli; and these ought to act on that excitability, which, owing to the imperfection of language, Brown calls “accumulated:” and they ought thereby to produce sufficient excitement

an explanation, both of the assimilation of nourishment, and of the generation of offspring.

How indisputable is the fact, and how important, that the component parts of living matter, whether motionless as in the egg, or in action as in the perfect animal, that these parts preserve their proper composition, contrary to the laws of chemical affinity in dead matter. This property, surely, is one of the properties of life, and one which distinguishes animate from inanimate matter. Now, in his tenth paragraph, Brown says, "in all the states of life, man and other animals differ from themselves, in their dead state, or from any other inanimate matter, in this property *alone*; they can be affected by external agents, as well as by certain functions peculiar to themselves, in such a manner as that the phenomena peculiar to the living state can be produced. This proposition extends to every thing that is vital in nature, and therefore applies to vegetables."

BUT the property above mentioned is one of the properties of life, it is one of the "phenomena peculiar to the living state." Yet this property is not maintained by stimulus; it is not affected by external agents. Should it be pretended that this property is maintained by stimulus, I should ask, what is the stimulus by which it is maintained in the egg and the seed?

THE truth seems to be, that action is produced by stimulus; although this proposition appears to be subject to some exceptions, which we do

not well understand. What are the stimuli by which are produced the growth of the body generally, and the evolution and commencement of action in particular organs at certain periods of life? How is it that the body of man increases more between the tenth and twentieth, than between the fiftieth and sixtieth years of life? Before we admit that reply which is obvious to every one, let us consider whether it does not prove too much—too much, at least, to be admitted by a Brunonian.

THIS obvious reply would be, that the common stimuli, as food, the blood, &c. operating on the unexhausted excitability of youth, produce excitement, or action, more than sufficient to maintain the integrity of our frames, and accordingly it is expended in their enlargement; while the same stimuli, operating on the exhausted excitability of old age, are scarcely sufficient to support the common functions of life.

LET us consider how this explanation will apply to those phenomena which take place at the age of puberty. At that period of life, organs previously useless are evolved, new actions take place in various parts of the body, and at the same time the growth of the whole is suddenly increased. We often see more change and enlargement, in one year at that period, than during three or four which have preceded. Now, at this period of life, the original stock of excitability is, according to Brown, very much diminished. It

has been exhausted by the growth of the body, from its commencement to that period; from the size of an atom, to that of a mass weighing forty or fifty pounds. At this period of comparative exhaustion, without the application of new or greater stimuli than before, these effects are produced. Let us not be deceived by supposing that those stimuli are the cause of this change, of which we are only made susceptible by the change; that it is produced by the stimulus of passion; which passion we do not feel till the change has been wrought.

THE truth is, that this change, and the growth of our bodies generally, are ultimate facts, beyond which our analysis of natural history has never yet carried us. But like many things, which we cannot explain, they are stumbling blocks in the way of hypotheses drawn from a limited view of the phenomena of nature.

STILL it is true, that action, generally, is excited and maintained by stimulus. Of actions in complicated living systems, such as that of man, there are some which may or may not be excited indifferently, without any immediate injury to health. Others, if imperfectly performed, or not at all, or if performed in excess, will occasion greater or less disturbance in the important functions of the system. Again, others are so important, either directly or indirectly, to the maintenance of life, that a very short suspension of them occasions death. Yet life does not con-

sist in action, nor is it essentially dependant on action. It is a property inherent in, or attached to, the matter in which it is found. It is a property which may be communicated from that which does possess it, to that which does not, but which each portion, in its turn, must relinquish. So far is it from truth, that life depends on either organization or action, that both of them depend on life. Dead matter can never operate on dead matter, so as to produce vital action; the matter in which such action can be excited must first be endowed with vitality*.

WE now proceed to the fifth proposition.

“The excitement may be too great, too small, or in just measure.”

WE shall omit some less important remarks, and take this opportunity to notice the opinions of Dr. Brown on the various states of excitement. Brown appears to use this term as equivalent to action in living beings; to living action, if the expression be admissible. If the term is understood to include also the idea of sensation, it will not affect the remarks which follow. In this action

* IT is usual to talk of the mysterious principle of life; and it is almost considered a reproach to physicians, if they do not offer some explanation of this principle. But, in fact, it is not more mysterious than the principle of gravitation, of cohesion, of electricity, of magnetism, or any of those principles or powers which cause motion in matter. To explain one of these principles, by any other of them, simplifies our philosophy, but does not lessen the mystery. In truth, it is not more necessary for a physician to explain the principle of vitality, than for a mariner, guided by the compass, to explain the principle of magnetism.

he does not appear to contemplate any other variation than that of quantity. He makes it to differ only in more or less. Now it merits consideration whether it does not differ also in quality, or species. This question has already been touched upon*, but has not been fully discussed. It will not lead us to idle speculations, but to investigations, than which there are not any more interesting in the doctrine of diseases, and of the methods of cure.

THERE are not many actions in living bodies which it is possible for us to see, and therefore we are able to judge of them only by their effects. In the larger muscles we see only contraction in greater or less degrees. The same remark is true as to the large blood vessels, so far as we discover either, by direct observations on them, or by noticing the effects of their actions. They do not seem to require any greater variety of action; for they serve only to distribute the fluids from the fountain to the various parts of the body, and to return them to the same source.

CAN the same be inferred respecting the minima vascula, the capillary vessels? Those we do not see, and must form our opinion only from their effects. They are the artificers of the body; they do the work; and however important or indispensable to them are the more noble organs, they are ultimately of the highest importance to every living system. By their agency are separated,

* See page 8.

or secreted from the blood, the various solids of the body, for its growth, and for the supply of its waste. By them are performed all the secretions, whether healthy or morbid.

Now the secreted fluids differ in their properties, not only from each other and from the blood, but from any thing to be found in the blood. They are not therefore strained from the blood. This fluid, or some parts of it, must undergo changes in the act of secretion; and these changes are wrought in the secreting vessels. As the blood is not exposed there to any thing except the vessels, the changes must be wrought by those vessels. It is useless to suppose that the blood meets, in each secretory organ, some peculiar fluid by which the change is wrought; for the same difficulty remains to account for this fluid. Nor does it help us to employ the enchantment of fermentation; for we cannot attribute the variety of the fermentation in different organs to any thing except the influence of those organs; which influence must be some peculiar mode of action. Here we must stop. How can the secreting vessels produce those changes, except it be by peculiar modes of action? No hypothesis tolerably satisfactory has ever been suggested, to account for these phenomena, which does not resolve itself into this.

IF this opinion be correct, it must necessarily follow, that the difference in the fluids secreted must be in consequence of a difference in the action of the secretory vessels; or, in other words,

that the secreting vessels of the various organs have, in each, their peculiar modes of action. These modes of action are all natural and healthy.

DIFFERENT from these, and from each other, are the various morbid actions which take place in living systems. These actions, in a general view, may be considered analogous to the natural actions of the capillary vessels. Parts are often newly organized for the purpose of performing these actions, and, in some cases, they may be distinguished by their form and peculiar external characters, as well as by the properties of the matter secreted. Thus the organs formed in the vaccine or variolous inflammation are distinguished by characters as peculiar as those of the pancreas or the liver. They might be called the variolous and vaccine glands. So too are the fluids secreted by them distinguished by peculiar properties. At least by their effects, they demonstrate properties as peculiar as those of the bile, or the gastric juice.

ALL these phenomena may be accounted for by referring them to peculiar modes of action in the vessels, by which the parts are formed, and in those vessels which constitute them, and act in them. But they cannot be accounted for by supposing that these actions differ only in being more or less energetic; that they differ in quantity, indeed, but are the same in kind.

THIS argument might be extended very much, and variously stated. It probably, might be

shown that the curative action of some remedies could be best explained by the supposition, that they induce in the capillaries new actions, whereby those of the disease are interrupted and changed.

LET it be denied that the secretions of each organ depend on laws and actions peculiar to that organ, and you may, without any further loss of reason or disregard of fact, deny that animals essentially differ from each other; and aver, that it is owing only to the degree of stimulus applied to it, that the embryo of a man does not grow into an ass or an elephant.

THE excitability then of different living beings, and of different organs in the same individual, is not one simple and unmodified principle. Simple irritability may be defined a capacity of being excited to action upon the application of stimulus. But this is simple irritability defined as a general property. Parts endowed with this property are, in addition, disposed to act in certain modes only, according to their respective uses. Now Brown's hypothesis leads to the opinion, that their various actions differ only in quantity or degree of energy. This is much the same as to say, that a poet and a mathematician, engaged in their respective vocations, differ only in the energy of their labours, not in the species of them.

THE various irritability and actions of different parts may be further illustrated by a reference to the organs of sense. The powers of those organs all depend on that property which is called sen-

sibility. But the sensibility is not affected in different organs by the same stimuli; nor, when affected, does it occasion in the mind perceptions of the same kind. Thus light does not affect the ear, nor do undulations in the air produce sensations in the eye; neither do any sensations in the ear convey to the mind a perception of colour or figure, nor vice versa. Now it would well accord with Brown's hypothesis to deny all this; but when the evidence of *all* the senses is against it, even one's darling hypothesis must be left imperfect.

FROM effects we are authorized to believe that the modifications of the irritability in the various organs are precisely analogous to those of the sensibility. They are operated on by different stimuli, and differently affected by the same stimuli; and the actions produced in different organs vary from each other, as do the sensations in the different organs of sense.

SIXTH proposition.

“BY too great excitement weakness is produced, because the excitability becomes defective; this is indirect debility. When the exciting powers or stimulants are withheld, weakness is induced; this is direct debility. Here the excitability is in excess.”

THE distinction pointed out in this proposition is certainly a very useful one. No objection will be offered, except to the last part, which says, universally, that in cases of direct debility the excitability is in excess.

THIS does not strictly accord with Brown's own opinion concerning excitability. For if a certain portion only of excitability is given to each living being, and that at the commencement of its existence, the withholding of stimuli can only prevent the waste, but will not occasion an increase, of this principle. Something of this kind has already been stated.

BUT let us inquire whether the principle is true, however it may disagree with other parts of Brown's system. That such is the effect of a short suspension of accustomed stimuli is not to be denied; such as an unusual abstinence from food, and even the usual abstinence for certain portions of time. But how is it where there is a long continued suspension of stimuli, as in the squalid pauper, scantily supplied with food from its birth, and deprived at the same time of many other common external stimuli? In such an one do we find an accumulation of excitability? If this were true, we ought to find that, by a due adjustment of stimuli, the true healthy excitement might be produced in such a person at once. We ought also to find, that in the accidental exposure to ordinary stimuli, the meagre brat would be peculiarly liable to what Brown calls *sthenic* diseases, in consequence of his abundant and superfluous excitability.

IF, on the other hand, it be true that excitability is formed in the system, we ought to find that in such a person this principle would be defec-

five ; and we might look for those diseases which arise from a feebleness in the powers within, as well as in those without. Accordingly such a subject should be brought gradually to the healthy standard by the cautious administration of stimuli, and the supply of nourishment*. It is true that a small quantity of unaccustomed stimuli would produce great excitement in such a subject ; but this would not demonstrate excess of excitability ; it would only accord with the effects of unaccustomed stimuli on all other subjects. It is also true that such excitement, so produced, would be followed by indirect debility ; but this, so far from demonstrating that the subject had previously had excitability in excess, would show the contrary ; or in plainer English, would show that there was not power sufficient to support great action.

LET experienced practitioners decide whether I have pointed out the true mode of restoring health to such a subject ; and in so doing they will decide on this portion of the Brunonian system.

THE next proposition given by Beddoes is in the following words :

“ EVERY power that acts on the living frame is stimulant, or produces excitement by expending excitability. Thus, although a person accustomed to animal food may grow weak, if he lives upon vegetables, still the vegetable

* BROWN speaks of food, throughout his works, only as a stimulus. But it is to be considered not only as that which excites, but also as that which is to be converted to nourishment, which shall acquire excitability, and be itself excited.

diet can only be considered as producing an effect, the same in kind with animal, though inferior in degree. Whatever powers therefore we imagine, and however they vary from such as are habitually applied to produce due excitement, they can only weaken the system by urging it into too much motion, or suffering it to sink into langour."

WE have already considered whether there is any difference between stimulants, other than that of degree. We are now told that if an agent produces any effect on the living system, it must be stimulant either in a greater or less degree.

THIS is one of Brown's favourite opinions; an axiom which he considers as his own discovery. That sedative effects may be produced by proper agents he does not deny; but he says that these agents act either to diminish the sum total of stimulus, or to exhaust the excitability.

HE illustrates his ideas in the following manner.... The blood is a stimulus; taking away blood diminishes excitement by diminishing the quantity of stimulus. Just so the excitement is lessened by diminishing the quantity of heat. On the other hand, wine is a stimulus, and so is muscular action, but if used in excess they diminish the excitement by exhausting excitability.

HE then employs the following argument..... Since in certain known cases the excitement is diminished in the modes which have been stated, it follows that if in other cases the excitement is diminished, this effect must be produced in one of the same modes; inasmuch as "identity of known

effect always implies identity of cause, though unknown."

LET us first examine this last opinion, which Brown delivers with great confidence, and then we will inquire whether there are any substances in nature which appear to produce sedative effects.

AN argumentum ad hominem may be made out by applying this rule to some of Brown's own doctrines. He says there are two species of debility; the one he calls direct, where stimulus is deficient; the other indirect, where stimulus has been applied in excess. In both cases the common observer would notice only that there was weakness. Here then is identity of effect, and the position we are examining, if admitted, would prove that there was identity of cause; that is, that too much and too little excitement are the same.

APPLY this same position to other natural phenomena. Place a stone in the atmosphere, and it falls to the ground. Sir Isaac Newton tells you, that this motion is occasioned by the attraction of gravitation; that by this principle every mass of matter attracts every other, and that the motion is vastly greater in the stone than in the earth, only because each mass is acted upon inversely according to its quantity. Now another philosopher, holding a glass tube in his hand which he has been rubbing, directs you to present a piece of down to it. Immediately you see the down move toward the tube, and adhere to it. You say

that here is an effect precisely similar to that of the stone falling to the earth ; and on the principle under consideration there must be identity of cause ; that is, the down must be influenced by the attraction of gravitation.

EVERY philosopher sees the fallacy of this argument, and the absurdities to which it leads. But it is answered that this rule holds good where it is strictly observed ; that if the effect is truly and in every respect the same, the cause must be the same, or of the same nature. It is meant then to refer to the immediate cause, or that link in the chain of events which immediately precedes the effect. To the position thus strictly applied perhaps there will not lie any objection ; but it must then be of very limited use in natural philosophy, and least of all in that branch of it which relates to the phenomena of life. In very few instances are we able to determine the identity of effect with sufficient accuracy, among the operations of a living being, to make use of this position.

BUT let us now inquire whether there is in nature any power which produces, on the living frame, any directly sedative effect ; that is, which produces a sedative effect without either taking away any stimulus which was previously acting, and without exhausting the powers of the system by any primary excitement.

Is this question to be determined by reasoning a priori from principles such as have been discussed in the preceding pages ? Or shall we not

rather descend to the humble paths of observation and experience? Shall we not take the gross facts from nature, and analyse them as far as our ingenuity and skill will permit; believing that we shall gain nothing by placing an hypothesis between two individual circumstances which are always noticed in succession*.

A MAN has pains in the bowels, and frequent efforts to evacuate them, when he has only small discharges. He takes a grain of opium, and after thirty minutes his pain and efforts cease for several hours, when they return in the same manner as before. This is a case which occurs daily.

THE efforts to empty the bowels must be owing to one of two causes. Either there is an unusual stimulus applied to the intestines, or their irritability is increased so that the usual stimuli produce extraordinary effects. From the pain we may infer the same results respecting the sensibility. In this case how has the opium removed, for a time, the troublesome symptoms?

HAS it taken away the stimulus which occasioned the irritation and sensation? This has never been pretended. Has it stimulated the parts to so great a degree that they are no longer capable of being excited as before, because their powers to act and to feel are exhausted? This last question is, as far as I understand it, answered in the affirmative by the Brunonians. But do the facts correspond with their answer? Inquire of

* See Brown's introduction to his observations on Zoonomia.

the patient, and he has remarked nothing more than the subsidence of the symptoms. Did he perceive a previous augmentation of pain, or increased frequency or force, in the efforts of the bowels? He did not. But, for a few minutes, there might perhaps have been noticed an increased action in the arteries, and somewhat more violence in the pain. Such circumstances have been observed, although for my own part I have frequently watched for these appearances, and with full expectation of perceiving them*; but have most frequently been disappointed. Grant however that they do take place, is the power of action in the intestines so easily exhausted? If it is, why does it happen that the same patient shall, at another and later period, have the same disease in a much more violent degree for a long time without the same effects taking place which are attributed to the opium? If it is, why will not any other stimulus answer quite as well to check the action of the bowels, and to relieve the pain, as opium? That opium has some of the properties of a stimulant I am not prepared to deny. But that its sedative effects are the most remarkable, and are those for which it is most employed in medicine, and that these effects are not secondary, owing to a previous exhaustion of the powers of the system, facts will I think warrant me in asserting.

* THIS expectation has been grounded on the observations of a physician whose accuracy I greatly respect. The result of my own observations has been as I state; yet I am willing to admit in the argument that such very short increase of the symptoms does occur.

BUT the ever ready answer is, that the Turks use it as a stimulant, just as we use our wine, and surely it must always act in the same and not in two contrary modes. If the Turks use opium as a stimulant, does it follow that when I take a grain of that article for the toothache, and find my pain relieved—does it follow that I must believe, against the evidence of my own senses, that my relief was the consequence of an excitement which exhausted my power to feel? that in fifteen minutes this unnoticed excitement had been greater than that produced during a whole day of hard work? If, in eight or twelve hours after I am relieved, I find myself yawning and listless, must I believe, that the langour I now experience is the cause of the relief which I obtained several hours before?

A MAN is coughing very often and with violence, and although sleepy, is prevented from losing himself. He takes a grain of opium; the cough diminishes, and in twenty or thirty minutes he falls asleep, and continues to sleep six hours. He does not notice any remarkable effects other than these. Is this man to be persuaded that he was not relieved until by excess of excitement he was exhausted? Could such remarkable excitement have passed his notice?

WHAT is the benefit which every man acquainted with opium proposes to himself in the use of it? Who takes it to produce any action or sensation in the system? Is it for a purpose like this

that some persons are in the habitual use of it? Inquire of such persons, and you will find in almost every case that the habit of using opium has been induced by the desire of relief from some corporeal pain or mental anxiety, which relief they find produced by that article. The removal of such complaint, if no more than just enough for the purpose has been taken, leaves to the sufferer the use of his other powers and faculties, though not entirely without interruption.

WILL it be urged against the arguments which have been offered, that Dr. Brown's ideas respecting excitement have been misrepresented? Is it said that excitement does not necessarily mean action, and that in the cases of increased and irregular action which have been stated, there has been a debility which has been removed by the stimulus of opium? This explanation would deserve some attention, were it not certain that a diarrhæa or cough may be arrested, in the most vigorous as well as in the most feeble subject, by the use of this article of the *materia medica*. It is likewise true that all those effects, which are supposed to result from the stimulant properties of opium, are often produced by it in the most enfeebled subjects by a dose sufficient only to remove a slight irritation.

IF I should venture to offer an explanation of some of the effects of opium which appear to disagree with the opinion that it is directly sedative, it would be this: In consequence of diminishing

the irritability of the system, opium arrests both the secretory and excretory processes throughout the whole body, unless indeed it is necessary to except those of the skin. If it is true that any real secretion is augmented on the skin, this is an exception from its general effects; and, whatever may cause that exception, our inferences must be made, not from that, but from the general effects.

Now, whenever either a secretion or excretion is arrested in the human, and probably in all living systems, the effect is to produce great disturbance in the whole system. At the first this disturbance is perceived mostly in the actions of the vascular system. The arterics evidently labour, and are oppressed; the head is dizzy, or aches, and there follow nausea and often vomiting. When, as often happens from the use of opium, there is a diminished secretion, and no excretion of urine, the cause of all the disturbance in the system becomes more obvious; for the disturbance is precisely such as is produced by this state of the urinary organs arising from any other cause*. But when the effect on the urinary organs is less than has been described, it is of the same kind on those and other secretory and excretory organs, and accordingly the same disturbance takes place in the system. All this disturbance occurs more after some hours, than at first, as would be expected if the explanation offered is just; for a

* I HAVE been obliged several times to introduce the catheter in consequence of the use of opium; and the excitement, which had been produced by retention, was removed with its cause.

retention of urine, for instance, would not produce general irritation till the usual period of evacuation had elapsed; and, *cæteris paribus*, this irritation would be prevented during the first few hours, when the irritability was the most diminished by the opium.

ACCORDINGLY during the first few hours there occur ease of body, and either sleep, or that state of mind in which it seems to be relieved from the burden of the body, and is regardless of all the troubles of the world. At length irritation ensues from the causes which have been stated. In some peculiar habits this irritation takes place very early, and in a remarkable degree, in consequence of the interruption of the natural functions; while in others the reverse is as evidently noticed.

WHETHER this explanation of some of the phenomena following the use of opium be correct, or not, it appears very certain that it has some directly sedative effects, which cannot be explained on the Brunonian theory.

IT is impossible, without very far exceeding the limits which this dissertation ought to have, to go into a minute consideration of other articles of a sedative nature. Let it suffice to request any Brunonian to show an instance where *Folia Digitalis Purpureæ*, or *Acetis Plumbi*, have produced sedative effects, in consequence of either the removal of stimuli, or the exhaustion of the principle of life. That these, and other sedatives, differ in their effects, I am well aware. There is

a difference in the kind of effect of sedatives, as it has already been urged there is in that of stimulants. To a question concerning the cause of this difference, no reply is offered.

BUT it does not appear that Brown's inferences are logical, although his premises be granted. Wine, according to his system, and water, both stimulate, although in different degrees. If a man drink, in the course of an hour, a pint of wine, his system is stimulated to a certain degree. In an hour after, while his system continues to be excited in the same degree, let him drink a pint of water, and that no effect may be attributed to the cold, let the water be warm. If this water is a stimulus, although much inferior in that respect to the wine, it should increase the excitement. It is as one pound to an hundred, and may turn the beam which was before in equilibrio. So ought we to reason, if we adopt Brown's principles.—The fact, however, is against all this, and Brown, seeing that, professes to infer the fact from his principles in the following manner. Although, he would say, water would produce some degree of excitement in a system very little excited, yet, in this case, its effect is to diminish the excitement; for as its stimulant power is less than that of the wine, it will not permit the excitement to remain so high as it was raised by the wine. That two and two will not always make four in physics is very certain; but according to the simple principles of Brown two and two should make four.

WE come at length to the eighth and last proposition laid down by Beddoes :

“ EXCITABILITY is seated in the medullary portion of the nerves, and in the muscles. As soon as it is any where affected, it is affected every where ; nor is excitement ever diminished in a part, while it is increased in the system ; in other words, different parts can never be in opposite states of excitement.”

LET us abide by facts, and enumerate all the properties which appear, by experiment and observation, to belong to the nerves and muscles ; and let us then inquire whether these are all the properties of living beings ; or whether the others may be considered as derived from these.

The following are the properties of the nerves :

1st. To convey impressions of external objects, or internal states or actions, to the brain, and of course to the mind.

2d. To convey from the brain the mandates of the will to the voluntary muscles.

3d. To convey impressions from one part to another, independent of the will, and without the observation of the mind, whereby those actions are produced which are called sympathetic. It is unnecessary to determine whether impressions are so conveyed through the medium of the brain, &c. or by communicating branches directly from one nerve to another.

THE following are the properties of the muscles:

1st. To alter their dimensions in the course of their fibres, that is, to contract and to relax. This is called mobility.

2d. The property called irritability, i. e. to be excited to contract by impressions made on them. The powers by which those impressions are made are called stimuli. These stimuli are of various sorts, and do not all affect every muscle. Thus almost any foreign matter may be made to act as a stimulus to every muscle. The will, through the medium of the nerves, is capable of exciting very powerful actions in most of the muscles of animal bodies; but over certain muscles the will has no power. Certain muscles are peculiarly affected by certain stimuli, to which they are adapted, and which stimuli do not produce the same effect on any other muscles; for instance, the heart and sanguiferous system are powerfully stimulated by the blood, while no other muscles are affected in the same manner by the same substance. The respiratory muscles are powerfully affected by stimuli applied, not to themselves, but to the bronchial surfaces, while no other muscles are affected in the same manner by the same cause.

THIS is a short sketch of the properties belonging to the nerves and muscles; but I believe all their properties are comprehended in it. Have I enumerated among them all the properties of life? Surely not. Can the other properties of life be considered as derived from these? To prove the negative in answer to this question might lead us into too much detail; when any one gives an answer in the affirmative, and offers any proof in support of it, it will be time enough to consider

that proof. It has not, that I know, ever yet been attempted. In answer to Brown's general assertion, it is sufficient to make a general denial.

AGAIN, "as soon as it, excitability, is affected any where, it is affected every where," says this theory. And there is added to this, as a sort of consequence, "nor is excitement ever increased in a part, while it is diminished in the system; in other words, different parts can never be in opposite states of excitement."

THIS doctrine seems very plausible at first sight. But let us not, in this age, sit in our studies and determine by reasoning a priori from a few insulated facts, what are the laws of animated systems. Go to the bedside of a patient tossed by the most violent convulsions, or tortured by the most excruciating spasms; there let the Brunonian deliver this law of his master, and satisfied with this authority, let him decide that the arterial and sensual systems are enduring a proportionate excitement. It is here that the real philosopher can disprove his doctrine. He can show him that the action of the arteries is not increased, either in force or frequency, and that the sensibility in every part, except the seat of the disease, is comparatively paralyzed; the eye scarcely sees, the tongue does not distinguish tastes, nor does the ear hear. I pretend not that such is always found to be the case; but instances of this sort must present themselves to the observation of every practitioner.

BUT Dr. Brown thinks these very convulsions are the effect of debility ; that they show the excitement to be small, and that therefore they correspond with the other phenomena. I will not notice the obvious absurdity of calling the excitement small, where the action is so powerful and violent ; but will only observe that the subjects of such cases often give every demonstration of perfect vigour immediately before the attack, and again in a few hours afterwards.

As to opposite states of excitement, this opposition is confessedly an arbitrary thing ; it has not reference to any fixed point or time. The actions in animated beings, according to Brown's theory, have no other difference than that of degree ; and he no where points out any exact degree as the point of health. " Good health consists in a pleasant, easy, and exact use of all the functions." These terms, pleasant, easy, and exact, are all relative, and he no where gives us the objects of comparison. The words, exact use of all the functions, though they have the sound of accuracy, are of no avail in this instance ; for they must be construed to mean just such a use as there ought to be. The question recurs, what that use ought to be. The other words are of more import, for they tell us, it should be such an use as is easy and pleasant. But it is probable that different persons would fix on very different states of excitement as being the most pleasant.

" SOME place their bliss in action ; some in ease."

THE quiet and industrious bookworm may be satisfied with that state of existence in which action just keeps on without halting; while the jovial epicure calls that state health, in which every capillary carries red blood, and action is increased to a degree, which in the temperate student, would produce delirium. How then are we to determine when two parts of the system are in opposite states of excitement, when we cannot agree what is the point of health, the fixed point to which this opposition doubtless is meant to refer?

LET us adopt the jargon of system, and suppose that there is a fixed point of good health, as that of forty degrees on the imaginary scale of the Brunonians. It is admitted by Brown that a part may be excited to a greater degree than the general system, or vice versa. We will suppose that the brain, for instance, may be excited to a greater degree than the general system. May it not happen that this degree of excitement in the brain shall be just forty-five? Nothing even pretended by Brown can show why this may not happen. Let the brain then be excited to forty-five degrees, and the system to ten degrees less, of course to thirty-five; and then we find the system and the part in opposite states of excitement. This is not a forced case, but it is one which, upon Brown's principles, would be very likely to be of frequent occurrence.

AN objection to this proposition will also be found in the consideration of local diseases. But

this subject will be more conveniently noticed, when making some general observations on our subject.

HAVING thus examined those general propositions, which are given us by the editor of Brown's elements of medicine, as containing his leading principles, I will make some comments upon the system in general, or such of the parts, not yet examined, as appear most important; noticing some points which, in the previous discussion, were referred to this place; and suggesting some of the causes which have tended to make this work popular.

BROWN seems to affect to follow the same rules of philosophizing as Newton. This pretension was noticed in the early part of this dissertation, but was not then examined. If Brown has arrived at truth by the close observation of nature which characterized Newton, it is unfortunate that his demonstration of this truth has not been more perfect. But I conceive that Brown's mode of investigation is precisely opposite to that of Bacon, Newton, and the best modern philosophers, and is the mode employed by the antients. The ancient mode was synthetical, the modern analytical. The former assumes certain principles as being primary in the order of nature, and endeavours to prove their truth by showing that they can explain all the phenomena in the subjects of them; or in other words, that the combination of them will produce all the effects observed in the subject of investigation. The latter, or analytical method, is

to observe phenomena, arrange them into classes, and subdivide them according to their general and special resemblances; and from this close observation of phenomena, regarding their series and order, to determine the composition and laws of the subject of investigation. In other words, this method teaches to separate and distinguish facts, till we reach the simplest of which our powers are capable of taking cognizance. In this method the philosopher is not permitted to consider any opinion the basis of future reasoning, of which the truth is not previously demonstrated.

THE two modes are well exemplified in the ancient and modern opinions concerning the elements of the material world. The ancients, for the most part, entertained a very simple and ingenious theory, that there were four elements; and they endeavoured to show that by the combination of these all the substances in nature could be formed. The moderns have laboriously and accurately decomposed as many as they could of material substances, and they have reduced them to a certain number of elementary bodies. This number is much larger than that of the ancients. But further than they can go in experiment and demonstration the principles of Bacon will not permit them to go in theory. They neither affirm nor deny that the substances, which they cannot further analyze, are simple or primary elements; but call bodies, yet undecomposed, elements as they respect our present knowledge. Thus they

give the result of their observations, without shutting the door against the further discoveries and improvements of industry and ingenuity.

THE same mode of philosophizing must be adopted in medicine, and all branches of physical science, if we would attain real and useful knowledge. That occasional use may be made of the synthetic method, in pursuing physical investigations, is not to be denied; but the other mode must govern our general course.

BROWN'S system, if true, ought to afford us an explanation of all the phenomena both of health and disease. It is not, like a system formed by a regular induction from facts, true as far as it goes, but leaving room for much more elucidation and augmentation by the further observation of facts. It commences with the statement of certain propositions, from which he endeavours to show that the phenomena of vitality, and the causes of health and disease, may be deduced. If the system is true, we ought to find every phenomenon in animated beings accord with, and explicable by it.

Now, in the very outset, the author seems to shut out from his consideration local diseases. They are not within the province of the physician, and he will not notice them.* But if his system be true, local diseases, although produced by lo-

* A late writer doubts whether there be any such thing as a disease affecting the whole system. See Clutterbuck on fever, vol. 1. chap. 1. sec. 3, where this doubt is very plausibly defended, to say the least.

cal injury, must follow the same laws as general diseases. The parts cannot have more properties than the whole. As his system gives to the whole but one property, the parts of course cannot have less, unless it is denied that they have any. This will not be pretended. They have then the same property as the whole. As they have the same property, they must be liable to the same and only the same affections, subject to the same laws. It would seem then that the exclusion of local diseases from his consideration was at least useless.

BUT further, according to his system, there cannot be any such thing as a strictly local disease ; nay, it would seem that every disease must be local in just the same proportion as every other.

To show the truth of these assertions, let us see how general or universal diseases arise *in his system*, and then let us inquire in what way there can be excited any other disease in conformity to his principles.

GENERAL disease arises from stimulus, either too much or too little. The same stimulus is never applied at once to every part of the system. General disease must then arise from stimulus applied to some one or more parts. For instance, too much or too little stimulus applied to the skin, or to the stomach, will produce general disease. General disease is thus produced by local stimulus, because excitability is one and indivisible ; when affected in a part, it is affected in the whole ;

and the affection of the whole, we are told, is much greater than that of the part, although less in proportion*.

It is in this way that general diseases arise. Now, in what other way can local diseases arise? If parts are to be considered as belonging to the living system, that is to be parts of it, must not disease be produced in them in the same manner as above described? Whatever may be the cause, as for instance, a wound, if it operate on the living system or a part of it, it must, agreeably to Brown's principles, produce excitement. If it does not, it will not, according to his principles, be a disease. If it does, we may first imagine an excitement only in the part; but we cannot suppose it confined there. The excitability affected any where is affected every where. It is at once then a general disease. It appears, therefore, that agreeably to this system, there cannot be a local disease. If this be true, let us go star-gazing to learn the practice of medicine.

THAT our excitability may not be too much exhausted, various other remarks, which the subject suggests, shall be omitted. It is not designed to represent that this system bears no resemblance to truth, but that it differs from it too widely to be adopted, as affording rules of practice. The general propositions only have been considered, without any reference to the details. If it has been shown that the foundation is not solid, it will

* Elements of Med. Sec. 50.

be needless to examine the superstructure. If this has not been shown, I must be further enlightened before engaging in that examination.

THE following may be stated as the leading objections to the system which has been considered.

1. IT is not regularly inferred by deductions from a sufficient number of facts.

2. IT does not account for many natural and healthy operations in living beings; and indeed does not accord with all the facts observed on this subject, although it is rendered plausible by a reference to some of them.

3. IT does not account for the variety in the forms and course of diseases, as the same disease preserves its distinctive form, while it varies exceedingly in its degree of violence.

4. IT does not account for the effects of habit, or association.

IT now only remains for *me* to notice some of the causes of the popularity of this system.

ITS apparent simplicity is certainly one cause of the favour with which it has been received. In the eyes of a student in medicine the whole practice of medicine becomes clear, and plain, and simple, after reading Brown. He daily wants to correct the prescriptions of his master, and wonders, above all things, that he allows patients to remain weak, when he might so easily make them strong. But a tyro, with industry and opportunity of experiment, will soon learn that this simplicity is alto-

gether in the theory ; while he will be lost and confounded in endeavouring to conform his practice to it. It describes little more than an internal and hidden property ; and when he visits the sick he knows not how to understand the indices which mark the states of that property. It would almost seem, indeed, that there cannot be any such indices ; for symptoms he is told are fallacious ; and what are symptoms but the phenomena or appearances to be observed in the subject of disease. If he is not to be governed by these, how shall he distinguish one disease from another ? Hence it has arisen that there are contests among the disciples of Brown, and that different sects are diametrically opposed in their modes of treatment of the same disease. One of them, and a lecturer too, has pointed out the only course which, according to the leading principles of his master, can determine the mode of practice in any case. This is to administer a moderate dose of alcohol, or of some other diffusible stimulus, on first visiting a patient. The result of this experiment will determine the nature of the disease, and the mode of treatment to be pursued.

THIS system is popular, because it indulges indolence ; as it exempts us from poring over the observations of others in musty old books, and from the wearisome task of close observations, and slow and careful inductions. Brown, like the modest Mahomet, tells us that all truth may be found in his Koran.

THERE is another cause, which has made many sensible men speak well of this system, who have not examined the subject very thoroughly. The Brunonians say, that this system has produced a salutary revolution, and is really the basis of the modern practice of medicine, even among those who profess to undervalue it. Let us examine these pretensions.

IN former times many and various were supposed to be the powers of the articles of the *materia medica* ; and of course proportioned to these were the powers of the physicians in operating on their patients. Some articles diluted the sharpness of the humours ; others obtunded their acrimony. Some were astringent, others restrigent, others refrigerant, &c. The plain taste of modern science has abolished all these fine characters ; and truth has obliged us to acknowledge that the powers of medicine are of a more limited nature than our ancestors believed. Observation has shown that the only obvious effects of most remedies are to increase or diminish the actions of the system in general, or of some particular part of it. Some articles indeed are supposed to act as alteratives ; or to cure diseases, by causing actions differing in kind, but not necessarily differing in degree, from those of the disease. It is also supposed by some physicians, that “ every medicine possesses properties more or less peculiar to it, which determine its action to some particular part or organ, in preference to others.” But articles of these

descriptions are not acknowledged by all practitioners, although there is certainly great evidence in favour of the suppositions. Of course the curative efforts of physicians seem to be confined mostly to increasing and diminishing action, either in the whole system, or in some of its parts.

FROM this practice, which certainly had an origin very different from the Brunonian theory, its disciples have drawn inferences in favour of that theory.

IF, say they, your efforts are confined to the increase and diminution of action or excitement, does it not authorize a presumption that all diseases are dependent on too much or too little excitement? Far from it. This practice does not prove the truth of any theory of pathology; it only evinces the limited powers of our agents for curing diseases.

I WILL not notice the favour which this system has acquired among some sensible men out of the profession, in consequence of its recommending stimulants and restoratives in most cases of disease. I will only remark, that it is impossible for a man of sense to follow this practice for any number of years; for experience teaches every physician the truth of John Hunter's maxim, although every one does not make the abstract remark, viz....that to increase action is not necessarily to increase power or vigour. Every one learns by experience, even if he does not advert to the cause, that those articles of the materia me-

dica, called tonics and stimulants, will not, in all cases promote the recovery of the weak and feeble.

IN this dissertation I have endeavoured to confine myself to the examination of the most prominent features of the Brunonian system; I will not add to my remarks, already too far extended for the occasion, a laboured conclusion.

IF the objections which have been urged are well founded, every one should join in deprecating the popularity of a work, calculated to deceive and mislead the young, and to make the practice of medicine a curse instead of a blessing to mankind.

Med. Hist.

WZ

270

J 13r

1809

c.1

