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ESSAY LVI.

REPLY

TO A

FRENCH REVIEW

OF ESSAY LII.

BY

WILLIAM SHARP, M.D., F.R.S.

“In natural science there is one language universally intelligible
—the language of *facts*; it belongs to nature; it is the same to the
citizen of Paris and of London.”

SIR HUMPHREY DAVY.

LONDON:
GEORGE BELL AND SONS,
YORK STREET, COVENT GARDEN.

1889.

ANALYSIS.

Antipraxy—the contrary action of certain larger and certain smaller doses of each drug—is acknowledged as true to some extent, but the Reviewer contends :—

- I. That voids are left in the question of *Medicines*. Experiments are asked for with crude drugs. What is the contrary of zero? The questions are answered.
- II. That voids are left in the question of *Symptoms*. In increase and decrease of functions Antipraxy is allowed to be triumphant. But perversion of functions and anatomical lesions *cannot have contraries*. These contraries are shown.
- III. That voids are left in the question of *Doses*. Infinitesimal doses have not been studied. Why they have not is explained.

A paragraph on Idiosyncrasy or Predisposition.

And the Reviewer gives *a new definition of Homœopathy*.

ESSAY LVI.
REPLY TO A FRENCH REVIEW OF
ESSAY LII.

(THERAPEUTICS OUGHT TO BECOME A SCIENCE).

“ True fortitude of mind consists in not letting what we do know be disturbed by what we do not know.”

PALEY.

PIONEERS in an unknown country are not expected to furnish a correct map of it throughout its whole extent. They may be congratulated if the report of what they have seen is sufficiently attractive to induce others to follow them, and to extend the survey of the new country till its boundaries are reached. Columbus is remembered as the discoverer of the South American Continent, though he did not reach the shores of the Pacific.

The same is true in the search after all the knowledges of nature, and pre-eminently in the arduous pursuit of the laws which govern its phenomena. When it is the privilege of any man to discover one of these laws he must be content to leave it to others to find out further applications of the law, in this direction and in that, till its limits are reached, when the map of its territory may then be portrayed. And especially must he leave the expansion of the law itself, or the absorption of it by another and wider generalization. Every branch of physical science having for its foundation a law of nature—and no knowledge of nature is a science without such a foundation—is an example of this unavoidable division of labour.

The contrary action on the living body of man, alike in health and in sickness, of certain larger and certain smaller doses of drugs—to which the name of *Antipraxy* has been given—was discovered by experiments with them in health, and is undeniably confirmed by innumer-

able experiments with them in sickness. As was to be expected, the discovery has been keenly criticised and vehemently condemned. But in a Review published in a medical journal in Paris (in November 1887) the case was summed up in these words:—“*les faits sur lesquels repose l'antipraxie sont incontestibles*” ;* and they have since been summed up by an English physician in a letter to me, in the words:—“*Antipraxie is an established fact.*”

This is the present stage of the enquiry, and it is highly satisfactory. Nevertheless, there must be a great deal yet to learn about so new a subject, and it is incumbent upon us to look out for further progress to be made in our acquaintance with Antipraxie. On the principle of “doing the work that is nearest to us” this is our immediate duty, only while doing it let us remember that we are not to let what we do know be disturbed by what we do not know.

There has been another Review in the same French medical journal (in January 1889), which will help us very agreeably in our endeavours to make this further progress, and some of its sentences may be quoted for the purpose of eliciting in the replies the information we are in search of:—

“*Votre étude est encore incomplète, et vous avez laissé des lacunes dans la question des médicaments, dans celle des symptômes, et dans celle des doses.*” “Your study is still incomplete, and you have left voids in the question of medicines, in that of symptoms, and in that of doses.”

“I. *Medicines.*—You have not experimented with inert medicines in their natural (or crude) state. . . . What in your view are the larger and smaller doses of *common salt*, of *chalk*, of *lycopodium*? You know well that the large doses of this last are very nearly without action on a healthy man; if the small doses ought to act in a contrary manner, what is the contrary of zero?”

When it has been ascertained that a drug is inert in its crude state, further experiments with it in this state are unnecessary. There are many drugs of this sort; one of them, Quicksilver, has been long known and has been treated in this manner—triturations of it have been

* *Bibliothèque Homœopathique.*

made for more than a hundred years; in "blue-pill" it is triturated with conserve of roses, in "grey-powder" with chalk. Other inert substances, as several metals, the earths, &c., are now in the same manner triturated with sugar of milk. It is with these mechanical subdivisions of such drugs that experiments have to be made, and contrary actions, or actions in contrary directions, between the first and second or third triturations—that is, between the hundredth part of a grain and the ten thousandth, or the millionth, may be expected. The three examples given by the reviewer have to be thus dealt with.

Common salt.—This is one of those border substances which baffle the ingenuity of human classification. No one hesitates to put lions and horses in a different class from that containing oaks and roses, but to this day the line of demarcation between the two classes of animals and plants has not been satisfactorily drawn. So bread as a food, and arsenic as a poison, cannot be classed together—they must needs be put into separate classes—and yet there are substances which have a claim to appear in both these classes. *Common salt* is one of these substances. *Chloride of Sodium*, in the form known as common salt, is taken by us in every meal, and is essential to the process of healthy digestion; in this form, therefore, it is not a drug according to the definition several times given in these Essays. To become a drug it must be divided into minute particles either by trituration or by solution in water; and when successive subdivisions have been made in either of these ways, experiments may be undertaken with them, and contrary actions between certain larger and certain smaller doses will, no doubt, be discovered.

Hahnemann admits that "salt when ordinarily used, has no pernicious effect upon the organism," but he gives provings of it by himself and four other physicians. His direction is "dynamise one grain of the crystals in the usual fashion." From which it must be concluded that these experiments or provings were made, as just now suggested, with these minute subdivisions. The list of symptoms recorded, amounting to many hundreds, cover forty pages, and include almost every imaginable uncomfortableness which human nature has to bear. But Hahnemann does not give us any intimation of the

doses taken by the provers. It is probable that different doses were taken, for contrary symptoms appear in the long catalogue, but we are not told what these different doses were, nor what symptoms belonged to each. By these omissions the experiments have been rendered nearly useless, and the entire work must be done again before any precise knowledge can be obtained. This is my "view of the larger and smaller doses of *common salt*." If it is said that the value of Hahnemann's provings is certified by the cures which have since been effected by small doses of salt, it may be replied that these cures are the results of experiments with "*Natrum muriaticum*" on the sick, more than suggestions derived from the provings.

Chalk.—Hahnemann's chalk was this:—"Take a clean *oyster shell*, somewhat thick; of the soft, snow-white calcareous substance, which is found between the internal and external hard shell, take one grain, which is then to be triturated in the usual manner." This is the drug, and experiments ought to be made with the different triturations and, no doubt, actions in contrary directions will be discovered. Hahnemann, as he does of other drugs, gives contrary symptoms in his provings of *Calcareo carbonica*, and these, by fresh experiments, have to be assigned to the doses to which they belong.

Lycopodium.—Pereira, among others, in his *Materia Medica*, gives an account of *Club-moss*, with engravings of the plant and also of the sporules highly magnified, these latter resemble minute nuts. It has lately been found that these sporules or pollen when triturated, burst and there issues out of them a little oil, and it is reasonably assumed that this fact accounts both for the inertness of the crude substance, and for the activity of the triturated powder. It will be remembered that Antipraxy is the contrary action of *certain* larger and *certain* smaller doses of the same drug. This has always been insisted upon, and in the case of *Lycopodium*, as in that of *Common Salt*, in that of *Chalk*, and in many other cases, the contrast of action is to be sought for by experiments with *different triturations*. The various triturations of *Lycopodium* are its larger and smaller doses, and the question about *zero* is answered.

The expressions "larger" and "smaller" are quantities relative to each other. Larger doses, therefore, do

not necessarily mean so many grains of a drug ; they may mean the hundredth or the thousandth part of a grain.

In some of the substances, which are inert in their crude state, trituration may effect a mechanical, and in others a chemical, change ; but without being engrossed with these changes, the physician's most pressing duty is to learn the different kinds of actions of the various triturations of the same drug ; and this learning is to be obtained by careful experiments on the healthy and on the sick.

Charcoal is another example, which may be added to the three given by my Reviewer, and I have lately tried experiments with it on myself, which may be recorded here. Early in the month of August of this year (1889), I began to take the thousandth part of a grain of *Carbo vegetabilis*, twice a day. On rising in the morning of the 12th I suddenly found myself disabled by vertigo—not dizziness or swimming before the eyes, but giddiness in the head—I could not stand nor walk steadily, but felt in danger of falling. This continued through this and the four following days. I did not feel otherwise ill, and enjoyed my food, but was glad to go to bed early each evening. On the 13th and 14th I took at dinner some brandy and water, a very unusual thing with me, and on the 15th and 16th some porter and port wine, without any effect upon the giddiness ; my pulse was slow—from sixty to seventy. I was told that I did not look ill, but I felt unequal to any mental or bodily exertion.

In the afternoon of the 16th I took the millionth part of a grain of the same drug—*Carbo vegetabilis*—and I quickly began to feel less giddy. The dose was repeated in the evening, and the next day the giddiness had nearly departed. One dose was taken that day, and on the following day (the 18th) I was as well as usual.

“ II. *Symptoms*.—Vous avez fort bien observé les symptômes compris dans la classe des augmentations et diminutions de fonctions, et c'est là le triomphe de l'Antipraxie. . . . ” “ You have well observed the symptoms included in the class of augmentations and diminutions of functions, and this is the triumph of Antipraxy ; but are there not other symptoms ? The *perversions* of functions constitute the majority (?) of

morbid accidents and *cannot have contraries*. What is the contrary of albuminuria? And since you have a marked preference for anatomical lesions, what is the contrary of fatty or cancerous degeneration?"

These sentences deserve serious attention, and, as far as I am able, they shall receive it from me. And first, let me express my pleasure at the acknowledgment, and my gratitude to Dr. Vincent Léon Simon for having made it, that Antipraxy has triumphed to some extent. It is acknowledged to be true of morbid processes consisting of an increase or deficiency of function; or, as it has often been expressed by others, of any function having a *plus* and a *minus*. As almost every organ of the body has a function which may be disordered in this manner, it follows, I think, that such disorders must form the majority of common ailments; and as a matter of experience, I think every medical man finds them to be so in his daily practice. It is admitted that our knowledge of Antipraxy has reached to this considerable extent; we are to hold it fast, as we are to hold all knowledge fast, and not suffer it to be disturbed by our ignorance of what is beyond our present attainments.

And we are to grow in knowledge; but the discovery of truth is a subtle process, and success is dependent not only on sincerity of purpose, but also upon straightforwardness of procedure. It is so easy to be turned aside from the straight path into some alluring by-path of error. Among these dangers are three great ones—a change of the objects looked at; a shifting of the ground stood upon; and altered instruments. To be led astray in any of these ways is to make a fatal mistake. Great care, therefore, is necessary to avoid them.

Now, the *facts* concerning the action of drugs on man's organs in health and disease, are the objects aimed at in these Essays. The uniformity and continuity of natural phenomena are the ground on which we are standing. Experiments, without hypothesis or theory, are the instruments employed. To avoid the mistakes before us all these must remain the same—there must be no shifting of any of them. The change that is unavoidable is a change in the point of view. How miserably little of the heavens would the astronomer become acquainted with if he never altered the direction of his telescope!

The point of view, therefore, may lawfully be changed. Bearing all this in mind we may now proceed.

We are reminded by the Reviewer that, besides the increase and decrease of functions, there are also *perversions* of functions and *anatomical lesions*—for, as there is no distinct line of separation between these, we may take them together—and of these it is asserted that they *cannot have contraries*. Indeed, more than one physician has declared that such contraries are “impossible” and “unthinkable.” Let me patiently show both their possibility and their thinkableness, which seem to me to be very plain. The difficulty experienced by my friends arises, I think, from a misconception of the subject.

As the first step towards the removal of this misconception it will be useful to revert to the disorders which consist in an augmentation or diminution of functions, and of these only. As an illustration of such disorders perhaps the simplest cases we can take are those disorders of the *Heart*, whose beats, in a given time, may be increased or diminished. A certain dose of *Aconite*, taken when the heart is beating healthily, will make it beat faster, and a certain smaller dose will make it beat slower. Now, let it be observed that these quicker and slower movements of the heart are the *visible effects* of the action of the doses of *Aconite*; *they are not the action itself*. *Aconite* reaches the heart by circulating in the blood, and acts within that organ; *how* it acts there we do not know. What we do know are the visible effects of the action—we see that the effect of the larger dose is a quicker beating of the heart, and that the effect of the smaller dose is a slower beating. We see that these effects are contrary ones, and we conclude that the actions which produced them have been in contrary directions. I think this conclusion is incontrovertible. Need it be added that, as the quicker and slower beats of the heart are the visible opposite effects of invisible opposite actions, so the increase and decrease of secretions, the contraction and relaxation of muscles, and all other similar opposites, are in like manner visible opposite or contrary *effects* of invisible opposite or contrary *actions*?

The experiments relative to those effects of drug-action which consist in increase or diminution of functions, and

by which Antipraxy was first proved, were made with persons in health. As I have often said, they were made to solve a question in Science, and the solution is true without any reference to the use of drugs as medicines. Nevertheless, the solution thus arrived at may become the most practical and the most successful guide in the treatment of the sick with these drugs as medicines which has yet been discovered.

There come next, experiments with the same doses of drugs in disease. Let us continue the *Heart* and *Aconite* as the typical organ and drug, and we find, on giving the larger and the smaller doses of Aconite, when the heart is not in health but beating too fast, that the larger doses aggravate this condition, that is, make the beats of the heart still quicker, and that the smaller doses make the beats slower. Hence it is concluded, in respect to the increase and decrease of the functions of organs, that the action of each series of doses on the sick is *in the same direction as it is on the healthy*.

This is looking at the subject from two points of view, and the two aspects thus seen may be taken as proved and admitted. But truths are like geometrical solids, which have more than one side, and we may again change our point of view, and now a third aspect may be looked at with equal satisfaction. We have compared the effects of the larger and smaller doses of drugs in health. We have compared the effects of these doses in disease. We may now compare the effects of the larger doses in health with the effects of the smaller doses in disease. For example :—We know that the larger doses of *Aconite* disorder a healthy heart by quickening its beats, and that the smaller doses of the same drug restore a heart beating too fast to its healthy beating. These visible effects are contrary ones, and we unavoidably conclude that the action of Aconite in the larger doses *in health* and of the smaller doses *in disorder* are in opposite directions.

Another typical example may be given, namely *Opium*. Certain larger doses taken in health constipate the bowels ; certain smaller doses remove constipation. Here is the appearance and the disappearance of disorder—contrary visible effects of contrary invisible actions. Close attention to this reasoning may, perhaps, find some

welcome relief, as well as the reasoning find some confirmation, from the following case, lately sent me by Dr. E. A. Applebe of Coggeshall, Essex, who has since cheerfully given me leave to publish it with his name:—

Case.

“A girl, twenty-two years of age, engaged as a milliner, suffered from most obstinate constipation. She had been used to go ten or twelve days without relief, and had taken all sorts of drastic purgatives and enemata of all the ordinary and extraordinary drugs in and out of the Pharmacopœia. I had been looking over your Book, (Therapeutics founded upon Antipraxy), and reading what you have said about *Opium*. I candidly tell you, when I prescribed it I was very sceptical about its having any action. I put two minims of the B. P. Tincture of Opium into twelve ounces of water. I told her to take one teaspoonful every morning an hour before breakfast. The medicine acted like a charm, and the girl has now every morning a ‘regular action.’ She is now perfectly well—how or why I do not pretend to say, but the fact remains as I have stated.”

In this case the constipation, if not originally caused by the larger doses of drugs, certainly was not removed or even mitigated, but on the contrary was continued and intensified by them. The smaller doses counteract in disease what the larger doses cause in health. How is it that medical men do not see this?

We are to remember that the facts looked at from this third point of view are the same as those looked at from the two former ones, and that there is no shifting of the ground, nor any change of instruments. It will also be seen that this aspect of the facts is independent of those previously looked at, and that the conclusion drawn rests upon proofs of its own, that is, upon experiments on the sick compared with experiments on the healthy. The contrast is between the larger doses in health and the smaller doses in sickness. The visible effects are contraries. The invisible actions must be contraries also. The visible effect of the larger doses in health is the appearance of disorder. The visible effect of the smaller doses in sickness is the disappearance of disorder. Why

may it not be concluded that the invisible actions are contrary ones?

We are to remember that it is the point of view that admits of change, and that we have made this change three times.

(1) We have looked at the contrary action of certain larger and certain smaller doses of a drug, when taken in health.

(2) We have looked at the same contrary actions of the drug, when taken in disease.

(3) We have looked at the effects of the larger doses in health, and the contrary effects of the smaller doses in disease.

All this with reference to the increase and decrease of functions has been a long preliminary introduction, but it may be hoped that it will facilitate the understanding of what follows.

We will now, without changing the objects we are viewing, and without shifting our ground, or altering our instruments, and continuing the third point of view from which we have been looking, turn our attention to perversions of function and anatomical lesions. With reference to these we have simply to make a comparison between the effects of the larger doses in health with the effects of the smaller doses in disease. Nothing can be better suited to this purpose than the first example given in the Review:—"Perversions of functions," it is said, "cannot have contraries. What is the contrary of *albuminuria*?"

What is *albuminuria*? It is the presence of albumen in the urine, which is made visible by chemical tests, such as boiling the water and adding a small quantity of nitric acid. The albumen is coagulated, appears in white flakes, and falls to the bottom of the test-tube. It is the *visible effect* of a "morbid process" going on in the kidney. The albumen has come from the serum of the blood, has escaped from the vessels which contained it, and has passed, along with the urine, through the tubuli uriniferi and ureter into the bladder, and so its presence in the urine can be detected by analysis.

On a previous page of the Review the history is faithfully given of my experiments with *Titanium*. It is shown that among other serious effects it had upon me,

when taken *in the first trituration*, there was a copious albuminuria, and this so persistent that it continued two years, and was then speedily cured by taking Titanium *in the second trituration*, that is, in doses a hundred times less than those which had caused the discharge of albumen. Here are contrary *visible effects*—the larger doses taken in health are followed by the appearance of albumen; the smaller doses taken in disorder are followed by the disappearance of albumen. Can it be doubted that when the discharge of albumen was arrested by the smaller doses, their action was *in a contrary direction* to that of the larger doses? It may be added to these experiments on myself that I have more than once seen albuminuria, arising from other causes, cured or greatly diminished by the same second trituration of Titanium. The following is a case already related (in Essay XVII, published in 1867), but I wish to add what happened after that was written.

Case.

In November, 1865, a young lady was taken to London to be seen by an eminent physician, her friends being greatly alarmed at her condition. Their fears were confirmed by this consultation. The physician wrote to the surgeon in the country as follows:—"The urine is highly coagulable, and every symptom characteristic of *Bright's disease* is present in her case." Among these symptoms was extensive dropsy of the body and lower limbs. *Acetate of ammonia* with *wine of iron* and *saffron*, and *cream of tartar* with *jalap* and *capsicum* were prescribed on the 13th of November, 1865. In January, 1866, she was brought from some distance to me, in the state above described, but getting worse. The quantity of albumen was great, and the dropsical swelling of the body and limbs formidable; it was painful to look at her pallid face. *Titanium* was given her. Improvement began from that time, and in October she was to her own feelings, and to all appearance, well; the dropsy had entirely disappeared. A small quantity of albumen remained, but she considered herself not needing further treatment, and I did not see her again for some time. During the severe weather of the following winter there was some return of the symptoms. *Iron* (*Ferrum metallicum* 1, three times a day), was given her, for I wished

to learn if Iron would act as well as Titanium, but *this had no effect*. Titanium was again had recourse to, and in a week there was already great improvement; the dropsy again gradually disappeared, and the albumen, which had increased, was again much reduced in quantity. In August of the same year (1867) she wrote to say that she was well. Up to this point the case was reported in Essay XVII. Afterwards I saw her once, and now copy the note then made, "She looks better than I have yet seen her; no dropsy; there is still albumen." The Titanium was repeated. Again her letters were, "going on *most* satisfactorily," until January, 1868. About this time she lost an excellent mother, went to another part of the Kingdom, and I heard no more from her. So that, though wonderfully benefited, I do not know and cannot say that she was permanently cured. The doses she took were very small quantities of Titanium 1. In the experiments on myself I had taken two grains in each dose. I have no doubt *now* that Titanium 2 would have done still better. It will be noticed that it is more than twenty years since this case was treated, and at that time I had necessarily much to learn as to how to prescribe Titanium.

It is evident that this young lady's illness was one in which there was far more than a mere perversion of function giving rise to the escape of albumen. There must have been serious, there may have been fatal derangement of structure—*anatomical lesion*—in the kidneys. While looking at the amount of benefit derived, the apparently dying condition of the patient ought to be remembered.

Perhaps it will again be a relief to some of my readers if a short digression is made here, and it will be interesting to inquire what we are taught by modern writers as to the occurrence of albuminuria. We are told by Dr. Bristowe that it is met with, "In many specific fevers, and in other febrile disorders; in congestion of the kidneys due to heart disease, bronchitis, or obstruction of the renal veins or arteries. The most important causes are inflammation of the kidney, and those various chronic lesions which are comprehended in the term 'chronic Bright's disease.'"*

* *Practice of Medicine*, 2nd ed., p. 826.

If these cases are reflected upon I think it will appear that, while the immediate local cause is a morbid process going on in the kidneys themselves, there is in most of them if not in all, a remote cause in a morbid condition of the blood. Sir Thomas Watson, in his admirable lecture on Albuminuria, seems to take this view, he says :—“How are these head symptoms to be explained? They result, no doubt, like all the other intercurrent disorders from a *poisoned condition of the blood.*” I think it will be found that an unhealthy state of the blood not only generally follows the appearance of albumen in the urine, but also that it generally precedes it. In the experiments on myself with Titanium the action of the larger doses seemed to be this :—they acted primarily on the blood, and disturbed its healthy condition; and through this derangement they unsettled and weakened the nervous system, made vision imperfect, greatly diminished the strength of the muscles, making a short walk fatiguing, upset digestion, destroyed courage, and caused a persistent albuminuria. The action of the smaller doses was in a contrary direction, the blood was restored to health, and all the other evils disappeared.

We are also told that operations have been performed on living animals with the view of discovering the nature of the diseased action which produces albuminuria, such as tying the renal veins, and tying or compressing the renal artery. I venture to say that such experiments are not only valueless, but commonly misleading, which is worse. The circumstances under which they are performed are such that trustworthy observations are not likely to be made.

In taking leave of Titanium it may be remarked that the experiments with it on myself are an example both of the law of Antipraxy, and also of the principle for Toxicology suggested in Essay XXVII, published in 1875.

We have now reached “anatomical lesions” proper. Examples of the contrary action of certain larger and certain smaller doses of the same drug in morbid changes of structure are not difficult to find. Everyone knows that some larger doses of *Mercury* cause ulcers of the throat; it is equally certain that smaller doses cause such ulcers to disappear. In like manner *Arsenic* in larger doses

causes gastritis, and in smaller doses cures it; *Phosphorus*, in larger doses causes enteritis, and I have seen it cure a severe case of this inflammation in a time so short as to be incredible. *Corrosive sublimate* is a well-known cause of dysentery, it is an excellent remedy for this disease. *Aloes* and *Ignatia amara* cause, in the larger doses (among other effects) hæmorrhoids. For many years I have prescribed both these, especially the latter, in the smaller doses for bad cases of piles both in England and in India with very gratifying success. In all these serious diseases, caused and cured by larger and smaller doses of these various poisons, there is placed before our eyes in a startling manner contrary visible effects proving, beyond doubt, contrary invisible action.

It is with great pleasure that I have received permission to give the following extract from a letter dated June 3rd, 1889, from Dr. H. C. Shann, of York, and to add his name.

Case.

“Now, to take one of the drugs which to me is a new one—certainly in its present form—*Ignatia*. I had a patient who came to me with as aggravated a condition of piles as one could well meet with—a raw fringe of protruded bowel—I gave the *Ignatia* 1 in one-drop doses three times a day, and at the end of a week—he was well! I don’t know whether he or I was most pleased. It was a case of long standing, and the result so far most satisfactory.”

In another letter dated August 8th Dr. Shann writes:—“You are quite at liberty to use my name in connection with the case of hæmorrhoids. It was quite one of the worst cases I had seen of the kind, almost like *an excoriated cancerous growth*. I believe the man is quite cured, for he has not been again lately, and he was just about well when I last saw him. I have used the drug in other cases with good results—drop doses two or three times a day.”

It would be easy to pursue this course of illustrations with many other drugs and diseases, but it would be a fruitless labour. Unprejudiced minds will, I think, be convinced already, and others, after any number of examples, will still say, “We are not satisfied.” This portion

of my reply to Dr. Léon Simon's able criticism shall, therefore, now be concluded with a few reflections.

(1). *Plus* and *minus*—more or less—are not the only contraries. There are many besides these. Presence and absence; appearance and disappearance; disease and health; making ill and making well; these, with reference to our present subject, are contrasts or contraries as much as more and less.

(2). These contraries are effects of causes. The effects are within the cognizance of our senses; the causes are beyond our recognition.

(3). Nevertheless, we cannot escape from the conclusion that such contrary effects have contrary acting causes.

(4). It will be admitted that, with regard to the increase and decrease of functions, some larger doses taken in health, and some smaller doses of the same drug taken in sickness, produce contrary effects, and it is evident that these contrary effects have contrary acting causes. Assuredly, it is now equally clear that perversions of functions and morbid changes of structure (anatomical lesions) are subject to the same laws. Larger and smaller doses of drugs produce in them contrary effects, which also have contrary acting causes.

(5). It is true that we do not see these contrary causes, and what does this prove? It proves how limited are our senses, and how great is our present ignorance. It does not prove the non-existence of such contrary actions. We do not see the electrical currents passing along the wires of the telegraph, and in contrary directions. Do we doubt their existence?

(6). So that these contrary actions are not only possible and thinkable, but their existence cannot reasonably be doubted. Why should it be thought incredible that God should work after this manner?

(7). In Essay XXXII, (1877), an assertion frequently made by Dr. Drysdale of Liverpool, was noticed. It is that "one and the same dose produces two opposite actions . . . There are always two actions from one and the same dose. This is an universal law." The statement is still reiterated. If by it is meant that *all doses* of drugs produce two opposite actions, I can only say that it is daily contradicted by facts. It is true of the *intermediate* doses of each drug, but there are doses,

both *above* and *below* these, which have only one action. If the statement were true that *every* dose has two opposite actions, every dose that we give to a patient, however suitable the remedy, must first cause an aggravation of the disease before it can cure it. To contend that this double action belongs to all doses is a profound mistake.

(8). It has been said for many years, but it is quite in vain to say, that the primary action of some doses is so brief that it escapes our observation. This is an assumption we have no right to make. It is passing from the observation of facts to the invention of a theory.

(9). If it is asked, what are the intermediate doses of those drugs which in larger doses cause, and in smaller ones cure, perversions of functions and anatomical lesions? It is replied—the middle doses are those which first aggravate, and then cure or mitigate the diseases for which they are given. Medical men accustomed to the old method, and beginning to try the new one, are often troubled by such aggravations; because there is in their minds a natural unwillingness to make the doses sufficiently small. It was by the frequent occurrence of these aggravations that Hahnemann was compelled to make his doses smaller. All doses which have two opposite actions are, for that reason alone, unfit for use as medicines. The proper doses for physicians to prescribe are doses with an action in one direction only.

(10). Again another fact:—The “medicinal” or larger doses commonly given, often produce more powerful effects than was desired or expected, when they were prescribed. They have acted in the direction which was expected, not contrary to it, but in too violent a manner. The dose given has again been too large. Similar experience faces us with the smaller doses, but less frequently. Dr. Lauder Brunton’s experiments with *Opium* for constipation will illustrate my meaning. He says:—“I began with one drop of tincture of opium given in a teaspoonful of water every night. To my astonishment this dose was not only in most cases sufficient, but in one case it proved excessive, doing no good, [the action of the larger dose], while *half a drop* acted as a *brisk purgative*.”*

* *Pharmacology*, 3rd ed., p. 386.

(11). Medical men who are accustomed to prescribe the larger doses of drugs find it difficult to appreciate these statements, and are even ready to refuse to believe them. It must not be forgotten that all these experiments, whether on the healthy or on the sick, are made with one drug only—all combinations of more than one being rigidly avoided. With such experiments the majority of practitioners are unacquainted, and consequently not able to estimate either their truth or value. Again, from time to time good cures are effected by the use of compound prescriptions, and by these medical men are lulled into the belief that their method does not stand in need of reformation. The explanation of such occasional cures is this:—the prescription contains the true remedy, but in the larger dose; this, if given by itself, would aggravate, not cure the disease, but among its partners in the mixture there happens to be its antidote, and by this its action is reduced to that of the smaller dose, and so the good result comes about. A remarkable illustration of this occurred towards the close of the 17th century, in the practice of Dr. Groenevelt, who cured above a hundred cases of disease of the bladder with *Cantharides*, for which he was committed to Newgate for *mala praxis* by the President of the College of Physicians in 1692.* The tincture of *Cantharides* was given in the larger doses, but it was always combined with *Camphor*, which is its antidote, and so the actual result was the action of the smaller doses, and in this manner the cures were effected.

(12). In Essay XL, (1880), it was seen that Dr. Hughes of Brighton has raised the same objection to Antipraxy as that so ably stated by Dr. V. Léon Simon. He says:—"I think that Dr. Sharp will find that he has not yet touched the subject of nutritive as distinct from functional disorder." The objection, as stated by Dr. Hughes was easily answered in his own words. As re-stated by Dr. Léon Simon, it has required a much more searching reply. This reply has now, I think, been given, and my friendly Reviewer will examine the facts and reasoning respectfully put before him in this Essay. If, by this examination, he is satisfied that the facts are as they have been represented, and that the reasoning is

* *Tutus Cantharidum in Medicinâ usus internus*, per Joannem Groenevelt, M.D., e Colleg. Med. Lond.; ed. secunda, p. viii.

sound, I am sure he will have pleasure in acknowledging that the triumph of Antipraxya is complete.

“ III. *Doses*.—M. le Dr. Sharp est le premier à reconnaître qu’il n’a pas encore étudié les doses infinitésimales.”
 “ Dr. Sharp is the first to acknowledge that he has not yet studied infinitesimal doses.” It has not been possible for me to study them, as they require to be studied, in the time I have had at my disposal. Moreover, for many years it has been my conviction that no one has done this. Such minute fractions of matter have never been examined in a manner entitled to be called scientific, and indeed, it appears to me that the time has not yet arrived when they can be examined either scientifically or medically with success. We have only to look at the chaos of wild thoughts in which they are enveloped, to be convinced that we are not near enough to see the facts which are true in them, much less are we able to discover any laws that govern them. Under such conditions any use of them in practice we may try to make can only be empirical. It is wise, therefore, to admit that, in the present state of our knowledge, infinitesimal doses are outside the limits of Antipraxya.

“ Since he has himself limited Antipraxya to the third dilution or trituration, he is not authorised to present it as a general law, before dethroning the homœopathic law, which professes to apply to *all* doses, especially to infinitesimals exclusively created by Hahnemann.” This is a surprising paragraph, and suggests more than one reply. It may be said :—Dr. Vincent accepts Antipraxya as true of morbid augmentations and diminutions of functions. Antipraxya thus limited he admits; then it may have a more extended application and be true of other morbid processes, and *still be limited*, and not embrace any more doses than it did before. If, therefore, I am not authorised to present the larger fact as a “ general law,” how is it that Dr. Vincent accepts the more limited fact as one ?

Dr. Vincent here uses the expression “ general law ” as if it meant a rule for prescribing medicine, and which to be general (meaning universal) ought to apply to all doses. Antipraxya is no law at all in this sense. It means the fact that some small doses of each drug act,

both on the healthy and on the sick, in a contrary direction to some larger doses, and it suggests that this fact may be exceedingly useful to physicians in their practice.

The law-fact of Antipraxy, and the "homœopathic law"—*similia similibus curantur* or *curentur*—do not run in parallel lines. The first is a fact in science, which admits of inferences useful in therapeutics. The second is *now* generally called by homœopaths a "rule of practice"—*let likes be treated with likes*—and *as such* is simply a precept in medicine, like the rule of antiphlogistics and tonics, or stimulants and depressants, or hot and cold, or any former empirical hypothesis.

Again, the paragraph may be replied to by asking—What is a "general law"? It is a general fact, distinguished from an individual fact by being seen by the mind only, and not by the bodily senses. It is limited or has boundaries; it has no power outside these limits; but governs supremely within them. All the facts of this world, whether individual or general, are finite.

In what sense is Antipraxy a "general law"? Antipraxy is the expression of a *fact*, and it can express this fact legitimately only so far as the fact is at present known—it may in nature extend further, and this extension, if it exists, may be discovered, but until it is discovered, it must be looked upon as outside the limits of Antipraxy, that is, outside the limits of our present knowledge, and we are not to let what we do know be disturbed by what we do not know. We do know that there is a group of smaller doses of each drug, and a group of larger, the actions of which are in contrary directions. We know that there is a large group between these larger and smaller doses having both their actions. And clearly, it is also true that there are doses above the larger and below the smaller doses, which are at present outside the law-fact expressed by the word Antipraxy. The limitation of Antipraxy is now plainly stated and justified. Before dismissing this paragraph of the Review, it may be remarked that when it is said that the "homœopathic law" applies to *all* doses, it is assuming what has not been proved.

My Reviewer enquires if Antipraxy has studied Idiosyncrasy; and recommends it as deserving to be discussed in my next Essay. And as an illustration of its

difficulty, he mentions two medical men, whom he knows, of whom one cannot drink a cup of *Coffee* without having diarrhœa, and the other cannot do the same without being constipated. In reply I have the pleasure of telling him that this subject was carefully studied in Essay XLIII. on "Predisposition," published in 1881, and also he may be reminded that Coffee, as an article of diet, is not a drug. At the same time it is readily admitted that drugs may easily be mentioned of which the same doses produce opposite effects upon different persons.

The difficulty which Idiosyncrasy or Predisposition imposes upon all medical treatment is great indeed. No method of prescribing can escape from it. With Antipraxy the difficulty assumes this form:—The action of a drug in this or that direction cannot be fixed upon one dose, but requires a group of doses. For example: Two drops of tincture of *Aconite* have one action upon my pulse—they quicken it. On my friend Mr. Haslam they have also one action—his pulse is made slower. In Dr. Crumpe's experiments with *Opium*, given in Essay XXXII, the effect of one grain on himself was excitement only; on a young man to whom he gave the same dose, the effects were first excitement, and afterwards depression. On many other persons the effect of one grain of *Opium* will be depression only.

No doubt, the difference in the sensitiveness of different persons to the action of drugs is a formidable difficulty to practical Antipraxy, as it is to all practical work in Medicine; but it is *there*, and has to be faced. Difficulties are things to be overcome.

An important feature in the groups of doses now described in connection with predisposition is that they overlap each other. A dose often belongs to two adjoining groups, as the group of smaller doses and the intermediate group, or the group of larger doses and the intermediate one; but it not unfrequently may be found in all the three. A grain of *Opium* in the experiments of Dr. Crumpe just referred to, is an example of this last. Upon himself it had the one action of excitement; upon his young man, it produced the two effects of the intermediate group; upon many others, as is familiarly known, it has the one action of the larger doses, that of depression. So far from hiding the countless difficulties in the

practice of Medicine, great pains have been taken in these Essays, to place them prominently before us. We are not to imitate the ostrich.

While on the subject of difficulties, I will remind my readers of another, though it is a much less formidable one than Idiosyncrasy. It is the variation which will frequently happen in the relative strength of different specimens of the same drug, especially of plants. For instance, the variable percentage of water, plants contain when some are gathered in wet, and some in dry seasons. This difficulty the compilers of the British Homœopathic Pharmacopœia have endeavoured to cope with. There is also a difference in them arising from the different soils they grow in. Professor Christison gives a remarkable example of this in *Ænanthe crocata*, "an abundant plant in some localities throughout this country, and has usually been held one of the most virulent of European vegetables. . . . the plant grows in great abundance and very luxuriantly in a locality not far from Edinburgh. I have found it in that situation, to all appearance, quite inert." After relating several experiments upon dogs and rabbits, he says:—"Finally, the resinoid extract of eight ounces of the root, analogous to that which has proved so deadly in my hands when obtained from *Woolwich* plants, has no effect whatever, when prepared from those growing in the neighbourhood of Edinburgh. Relying on these results, I ate a whole tuber weighing an ounce, without observing any effect, except its disagreeable taste; which was the only circumstance that prevented me from trying a larger quantity."* Another cause of difference in power of action, which could scarcely have been anticipated, arises from the different months in the year in which the plants are gathered. Sir Robert Christison gives a striking illustration of this difficulty in *Conium maculatum*—a plant which I hold in very high estimation. He found the root collected in November had no effect. Orfila had previously observed that the root had no effect gathered in April, while at the end of May it killed a dog in six hours; and Christison "found the alcoholic extract of the juice obtained from six ounces of roots on the last day of May, kill a rabbit in thirty-seven minutes, when intro-

* *A Treatise on Poisons*, 4th ed., p. 860—864.

duced in a state of emulsion between the skin and muscles of the back; and the effects were analogous to those obtained with the extracts of the leaves. The differences depending *on season* will probably account for various persons having found the juice of the root harmless.* This is the *Idiosyncrasy* of drugs. As patients differ in their sensitiveness to the action of medicines, so the different samples of a drug differ in their power of acting.

It has been remarked more than once in these Essays, that it is useless to discuss any subject without beginning with *definitions* of the meaning of the technical words to be used on both sides. Such definitions sometimes remove the differences supposed to exist between the combatants, and so put an end to the controversy; sometimes they show that the two sides are using the same terms in such diverse senses that there is no subject left to be discussed; and when the result is not quite so happy as in these two issues, they reduce the discussion within precise limits, and enable each side to see clearly wherein its view differs from that of the other side.

This necessity for definitions is conspicuous in the controversy on Homœopathy. There is no agreement on either side as to the meaning of the word; and consequently, there is at present no hope of either side coming to a satisfactory conclusion. "Homœopathy" is not understood in the same sense by its own practitioners, and the meaning attached to the word by practitioners of the old school is very vague indeed.

In Essay LII pains were taken to ascertain what is a correct definition of the word *Homœopathy*. Hahnemann's first definition and his last were given, and it was shown how greatly these differed from each other—so greatly that one was an *induction* (but a mistaken one) from facts, while the other was a *deduction* from a fanciful hypothesis. The most recent definitions by writers on both sides also were given, and the correction of these by Dr. Dudgeon, a physician thoroughly acquainted with Hahnemann's works. The subject was concluded with these words:—"This [definition of Dr. Dudgeon] is a distinct return to Hahnemann's early definitions, and I

* *Ibid.*, p. 855.

think, puts the matter beyond further discussion.” (Page 11).

It was, therefore, a great surprise to me to read in the present Review, a correction of Dr. Dudgeon, and a new definition of Homœopathy. Dr. V. Léon Simon writes:—
 “ M. le Dr. Dudgeon a dit avec raison : ‘ L’homœopathie consiste à donner, pour guérir une maladie, un médicament qui ait la propriété de produire sur l’homme sain des symptômes semblables à ceux de la maladie à guérir. Les doses faibles ou fortes n’ont pas à intervenir dans une définition de la loi homœopathique. . . . Les règles posologiques dérivent de l’expérience.’ Nous ajouterons, pour être plus précis : *un médicament homœopathique peut guérir une maladie exactement à la même dose à laquelle il a fait naître chez l’homme sain les symptômes de cette maladie.* Cette proposition est démontrée par des faits sans nombre.” “ Dr. Dudgeon has said with reason : ‘ Homœopathy consists in giving for the cure of disease a medicine which can cause symptoms on the healthy similar to those of the disease to be cured. . . . The rule for the dose is the outcome of experience.’ We will add, to be more precise : *a homœopathic medicine can cure a disease by exactly the same dose as that by which it has caused in a healthy man the symptoms of that disease.* This proposition is proved by facts without number.”

This is the latest definition of Homœopathy that I have met with, and I admire Dr. Vincent’s courage in stating it. It is true that Hahnemann at first gave his drugs in the usual doses, or nearly so, but he was quickly compelled, by the aggravations they occasioned, to reduce them to smaller ones. In the same manner, in the long list of quotations he makes from previous writers, to which Dr. Vincent refers, a similar distinction occurs, *e. g.* “ Fritze saw Dulcamara produce *convulsions*, and De Haen witnessed the same effects attended with delirium. On the other hand, convulsions attended with delirium have yielded to *small doses* of Dulcamara administered by the last-mentioned physician.” (*Organon*, p. 65). Again, “ According to Vicat, J. C. Grimm, and others, Opium produces an almost irresistible tendency to sleep, accompanied by profuse perspiration and delirium. This was the reason why Ostloff was afraid to administer it in a case of fever which exhibited similar

symptoms. However, after having exhausted in vain all the known remedies, and seeing his patient at the point of death, he resolved, at all hazards, to administer *a small quantity* of opium, whose effects proved salutary." In most of the examples quoted by Hahnemann, nothing is said about the doses given, and as the prescriptions generally contain more than one drug, little can be proved by them.

In Hahnemann's and Dr. Dudgeon's definitions the change in one of the elements in the problem vitiates the conclusion drawn in neglect of that change. The drug used in the treatment of the sick is the same as that experimented with in the Provings, but the dose is not the same, and the conclusion arrived at, without noticing this change, is a mistaken one.

If the definition now given, which insists upon the same dose as well as the same drug, can be *proved*, Homœopathy will be proved, and must sooner or later be accepted as established on a true basis.

The new definition is intensely interesting to me, because it places the distinction between Homœopathy and Antipraxy very prominently before men's eyes. It makes plain the contrast there is between them.

One remark more.—In Essay I (*What is Homœopathy?*) a page is devoted to Hippocrates, as quoted by Hahnemann, as translated into Latin by Cornarius, and into English by Francis Adams, and to the comments of Dr. Adams upon the passage quoted from Hippocrates. "It thus appears," writes Dr. Adams, "that the principles both of *Allopathy* and *Homœopathy* are recognised by the author of this treatise." So I thought, along with others, when my first essay was written in 1851. But I hope, as Solon hoped, to "go learning on," and now, looking at the definitions referred to above, and at my own experiments in later years with small doses, I rejoice to be able to see that the prescription written by Hippocrates:—"Give the patient [suffering from mania] a draught made from the roots of mandrake *in a smaller dose than will induce mania*"—really belongs, not to Homœopathy, but to Antipraxy—it is not *similia similibus*, but *contraria contrariis curantur*.

Success is reached only by struggling through mistakes. I have made mistakes, and when they are perceived they are willingly acknowledged and sincerely

regretted. But when they are seen by the light reflected upon them from some newly discovered truth, my sorrow is turned into joy.

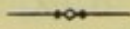
In conclusion, I wish to express the admiration with which I have read this Review of Essay LII, and to beg leave to offer to M. le Dr. Vincent Léon Simon, my grateful thanks for the friendly spirit in which he has written it.

Postscript.—It was intended to add a second Chapter to this Essay, devoted to the doses of a drug which are larger than the three groups at present included in Antipraxys. Several gentlemen are now testing for me some of these doses on themselves. This requires time, and it seems better to send these few pages on their errand, than to keep them back for what may follow them, if it please God I live. But I am often reminded of a candle nearly burnt down to the socket, which a puff may blow out at any moment. This investigation of the action of the different doses of each drug is a new enquiry. There are difficulties to be overcome; and, doubtless, mistakes will be made, to be owned and regretted; and there is opposition and neglect to be patiently borne. But work is put into our hands to be done, and results are retained in the hands of God.

HORTON HOUSE, RUGBY;

Sept. 28, 1889.

BY THE SAME AUTHOR.



ESSAYS ON MEDICINE.



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