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ESSAY LII.

THERAPEUTICS

ought to become

A SCIENCE.

BY

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



Thus growing old, I still go learning on."

SOLON. (Plato, Erastæ, § 2.)

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ANALYSIS.

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ESSAY LII.

I. *The method of reasoning in Science.*

“It is madness to think that those things which have never been done as yet, can be done except by means never as yet tried.”

LORD BACON.

(*Novum Organum, Aphor. 6.*)

THESE Essays have been devoted to the study of that part of Therapeutics which concerns the employment of drugs as medicines—a study to which my life has been devoted. Not in any exclusive sense, except that which unavoidably attaches to one man’s time and ability. There are many other departments of Medicine also worthy of a life’s devotion from other men.

It is confessed that Therapeutics is not yet a Science, and certainly Lord Bacon’s Aphorism applies to it, that it cannot be made a Science except by means never yet tried. A Science is a branch of human knowledge founded upon natural laws. Such laws are, to this day, so absent in Medicine that it is strongly held by physicians that they have no existence in the province of Medicine, and that theory or empiricism are the only paths open for them to follow. It requires, therefore, a persevering effort to be made to remove this stumbling-block, and to show that if there is “marvellous complexity” in the phenomena of health and disease, yet that we ought not to doubt that there is also “unbroken order.” At the present time to discover that “law” exists in disease, and ought to exist in its treatment, should be the first object of the medical student. How can this be done?

Not without first having a clear view of the method by which it may be done. What is that method? Theory and empiricism have been followed for more than two thousand years, and have failed. It is very obvious that

neither of these can help us. Observation and experiment remain. These also need a method by which they may be guided and utilized. The thing desired is that Therapeutics, so far as concerns the use of medicines, should become a Science; then it must be founded upon laws, and the method by which these laws can be discovered is called reasoning by *Induction* and *Deduction*—but what are these? They are words very obscurely understood, and so give rise to unprofitable controversies; hence the need of definitions, which, in spite of John Hunter's hatred of them, are indispensable. There can be no science without an intelligible definition of words. In considering, therefore, the method of reasoning in science, we must begin with *definitions*.

Definition of Science.—The word is a Latin one, *scientia*, and the English synonym is *knowledge*; but it has now a technical meaning, namely, knowledge founded upon natural laws. Astronomy is a Science, because it is knowledge based upon the laws of gravitation. Chemistry is a Science, because it is knowledge resting upon the laws of combination of elements in fixed proportions. For Therapeutics to become a Science it is indispensable that it be founded upon some laws of nature. It ought not to be doubted that such laws exist, and physicians ought to have no peace of mind until they are discovered.

Definition of Induction.—There are two kinds of Induction, essentially different from each other, though this distinction is not always perceived. There is a logical induction, and there is a scientific induction. For the word "induction" is appropriated by two distinct classes of reasoners and applied by them to two subjects so different that unless the two meanings are clearly defined, and adhered to by the writers on these subjects, endless discussions having no useful result are inevitable. Definitions in such cases are essential. The two sets of thinkers who use the word *induction*, each in their own sense, and each sense very different from the other, are students of logic and students of natural phenomena.

Logical induction is thus explained in a recent textbook:—

"*Inductive.*— c, c', c'' , are Some—A. c, c', c'' , are All—B. \therefore All—B. is Some—A. In the inductive syl-

logism the predicate of the second premise *must be universal*, or, in other words, c, c', c'' must be constituents of a given whole. When this is the case the induction is said to be complete. It is worthy of observation that if this predicate be *indefinite* [as all natural phenomena necessarily are to us], *no conclusion can be drawn*; the premises c, c', c'' , are Some—A, and c, c', c'' are Some—B, being incompetent. For example:—

Isaac, Levi, and Abraham are avaricious,
 Isaac, Levi, and Abraham, are All—the—Jews ;
 ∴ All Jews are avaricious.

This conclusion is just; but if the three individuals named do not constitute all the Jews, no conclusion at all can be drawn. It is true, that by indefinitely increasing the number of individuals that collectively form the subject of both premises, that is, by indefinitely approximating to the universe of Jews the conclusion can be made more and more *probable*; but until our induction exhausts the universe, that is, until we have found every individual Jew to be avaricious, we have no right to predicate that fact apodictically; and it must be remembered that logic *deals with none but necessary reasoning.*"

It follows that *logical induction* is impossible to the student of Natural Science; and from this it follows that if there is any inductive reasoning for him, it must be of a kind with which Logic cannot deal. From all this it is evident that logical induction is utterly inapplicable to investigations in Natural Science.

Scientific Induction.—What then is the definition of the word *induction* when it is used by writers on what is called Physical or Natural Science? It may, I think, be expressed in some such words as these:—It is the careful collection by observation or experiment of similar individual properties or facts belonging to a number of substances, by which they are put together into the same class. By further observation or experiment another property or fact may be found to belong to some of these substances so put together. When this has been discovered, guided by the law of continuity or uniformity now accepted in science, it becomes safe to conclude that this, to us, new property or fact will also belong to all the other substances in the same class. For example:—

Substances which, when taken by a man in health, act more or less injuriously upon him in all quantities large

or small, are classed together under the name of *Drugs*. It has been found, by careful experiment, that some of these act in a contrary manner in certain larger and smaller doses, and *scientific induction* permits us to conclude that the same property will belong to, the same fact be true of, all other drugs.

Those who reason according to Logic object to this; they contend that the conclusion is only a *probability*, and would have it called a "working hypothesis." Those who reason according to Science are bold to assert that it is more than hypothesis or theory, that for all practical purposes, it may be relied upon as true, and, therefore, they call it by some name implying this, such as a "law;" a "general fact;" or a "law-fact."

At the close of my paper in the 'Practitioner' for June, 1879—the fourth of a series on "the laws of healing by Drugs," which appeared in that journal in 1878 and 1879—the Editor, Dr. Lauder Brunton, made some comments, and among them is this:—"There are many drugs which in small doses will produce an action the contrary of that which they produce in large ones." In Essay XXXIX, "Answers to recent objections," it is said of this remark:—"First, the admission shall be recognised that *many* drugs have contrary actions in larger and smaller doses. It has been made clear in a former Essay, that these are individual facts, and so the statement here made is *empiricism*. When it is seen that *all* drugs have this contrary action, it becomes a general fact, and the statement of this belongs to science." The following is a criticism on this passage by one from whom it is a grief to me to differ, but he will let me have my say in charity, as he has said his:—"Is this known [that all drugs have this contrary action] by experiment with *every drug*, (in which case it is the short-hand statement of the result of *enumeratio simplex*)? Or, is it experimentally known of *some drugs*, (few or many does not matter), and assumed to be true of others, (in which case it is a 'working hypothesis')? It will hardly be granted that there is sufficient scientific certainty about these sorts of experiments to exclude the necessity of the *enumeratio* in full; the statement is, therefore, only an hypothesis about those drugs that have not been tried."

Bishop Temple, in his Bampton Lectures at Oxford in 1884, is on this side. It is there argued that Science

cannot go beyond *generality* in its inductions—never to *universality*. The inductions contended for by Bishop Temple, it is said, must be *demonstrated*.

We can now see clearly that all these arguments are in support of the induction of Logic—the old induction of Aristotle, not the modern induction of Lord Bacon. In every class of natural phenomena the numbers of any individual fact will always be indefinite and innumerable to us, so that an enumeration in full is impossible; we can, therefore, also see that logical induction cannot be successfully made use of in the investigations of physical science. Such an induction may have some value in Logic, but in the studies in which medical men are engaged, it is not only inapplicable but mischievous.

Shortly after the above was written, a friend called to see me, and he was asked to read it. He then said:—“You will find a pleasant confirmation of this in Mill’s Logic.” He sent me the book, and referred to Book III, Ch. 1—3 and 7—9. No doubt I ought to have read Mill long ago, and often wished to do so, *if I could*. The same may be said of many other books, but “Life is short, and the Art is long.” A brief extract from Mill is all that can be afforded in this place:—

“Induction is that operation of the mind by which we *infer* that what we know to be true in a particular case or cases, will be true in all cases which resemble the former in certain assignable respects. In other words, Induction is the process by which we conclude that what is true of certain individuals of a class is true of the whole class, or that what is true at certain times will be true in similar circumstances at all times. This definition excludes from the meaning of the term Induction, various *logical* operations, to which it is not unusual to apply that name. Induction, as above defined, is a process of inference; it proceeds from the known to the unknown, and any operation involving no inference, any process in which what seems the conclusion is no wider than the premises from which it is drawn, *does not fall within the meaning of the term*. Yet in common books of Logic we find this laid down as the most perfect, indeed the only quite perfect form of induction” (Book III, Ch. 1 and 2).

In the last sentence Mr. Mill deprives Logic of the word Induction. In this I think he goes too far. The term

belonged to Logic before Lord Bacon appropriated it, in a new sense, to Science. It would have prevented much confusion had Bacon adopted a new word—such a slight change as “Conduction” would have sufficed—to express the mental process he was advocating, instead of giving, as he did, a new meaning to an old word. He says:—“In forming axioms, *we must invent a different form of induction* from that hitherto in use. . . . The induction which proceeds by simple enumeration is puerile.”* It is too late now to change the word; to do that would increase the confusion. But if the students of science understand the meaning of Scientific Induction, and when they use the term, will adhere to this meaning, the confusion will disappear.

Definition of Deduction.—Scientific Deduction is the reverse process of Scientific Induction. When reasoning inductively we ascend from particular or individual facts to general or law-facts. When reasoning deductively we descend from these general facts or laws to particular or individual facts. This process might have been called “Eduction.”

As an example of deductive reasoning in reference to Antipraxy—a test experiment was proposed to me in 1876, by a very able man of science, it was *Castor oil*. The experiment was immediately tried by another scientific man—Mr. G. M. Seabroke—and was afterwards repeated by him two or three times. On each occasion the confirmation of the law was complete. The details are given in Essay XXXI. Since these experiments with Castor oil in health, I have had many times similar confirmations of its action in small doses in my practice, and others have had similar ones in theirs.

Deduction is also very commonly applied to hypotheses. It is possible for hypotheses to be true, but generally they are not, and consequently, deductions from them are generally false. When hypotheses have been proved to be true they cease to be hypotheses, and become facts, and deductions from such facts, if lawfully made, will be true. Hypotheses are much more popular than facts. There is something captivating to the imagination in guesses, but they are great hidiers of truth. While Hooke and Mayow were investigating combustion by experiments, Stahl

* *Novum Organum*. Basil Montagu's edition, vol. xiv, p. 32.

announced his hypothesis of *phlogiston*—that inflammable bodies are a compound of some base and phlogiston or fire—for example, when metallic zinc is heated to redness, it burns with a brilliant flame and is converted into a white earthy substance; zinc, therefore, is composed of this earth and phlogiston. Notwithstanding the fact, which was then known, that the burning of these inflammable bodies *increased their weight*, when according to the theory, it ought to have diminished it, the notion was enthusiastically embraced by chemists and prevailed for many years. Medical men translated it into their language, and for a long time nothing was heard of but the treatment of fever and inflammation by *antiphlogistics*. The drugs so named were no more entitled to it than those which, for so many centuries, had been ticketed hot and cold and dry and wet, to suit the prevailing hypothesis, were entitled to be so distinguished. Medical students, beware of hypotheses!

Definition of Drugs.—This word also requires a definition; it has been given before more than once, but it will be useful to repeat it here. The word *Drug* means a substance, which, taken in any quantity great or small by a healthy human being, acts injuriously on some part or parts of his body.*

Definition of Homœopathy.—One of the roughest battles ever fought in the Medical Profession has been carried on for nearly a century, on the subject of Hahnemann's Homœopathy. As often happens in hot quarrels the thing quarrelled about has not been clearly understood on either side. As the discussion has probably yet to run on for a long time to come, it may be of service to consider calmly, and for the benefit of both sides, what is really to be understood by the word Homœopathy.

It is due to Hahnemann to notice first *his* definitions of the word. The first given by him is in his 'Essay on a new Principle,' printed in Hufeland's Journal in 1796, and is as follows:—"My maxim is—in order to discover the true remedial powers of a medicine for chronic diseases, [acute diseases were included afterwards], we must look to the specific artificial disease it can develop in the human body, and employ it in a very similar morbid condition of the organism which it is wished to remove.

* See *Therapeutics*, p. 107. Geo. Bell and Son. 1886.

The analogous maxim is—in order to cure radically certain chronic diseases, we must search for medicines that can excite a similar disease, (the more similar the better), in the human body.”

This definition is intelligible. It is a statement of what Hahnemann believed to be a general fact. Unfortunately, he did not abide content with it, but sought an explanatory hypothesis, and so became both obscure and false. His latest definition is:—“A weaker dynamic affection is permanently extinguished in the living organism by a stronger one, if the latter (while differing in kind) is similar to the former in its manifestations.”

Hahnemann’s first definition makes Homœopathy an *induction* from individual facts; his last, from the ‘Organon,’ which Dr. Dudgeon tells us “contains the principles of his doctrine in their most perfect and matured state,” makes Homœopathy a *deduction* from hypothesis.

It is necessary here to ask attention to the fact that in neither of these definitions of Homœopathy given us by Hahnemann himself, is there any reference to *doses* of any kind.

Let us now look at the definitions most recently given by writers on both sides. Dr. Lauder Brunton, in the 3rd Edition of his ‘Pharmacology,’ published last year (1887), says:—“The mere fact that a drug in small doses will cure a disease exhibiting symptoms similar to those produced by a large dose of the drug, does not constitute it a homœopathic medicine, for this rule was known to Hippocrates, and the rule *similia similibus curantur* was recognised by him in some instances” (Preface, p. x).

The Editors of the ‘Monthly Homœopathic Review’—Drs. Pope and Dyce Brown—in the No. for June 1887, say:—“In very truth it is this ‘mere fact’ which does constitute a drug a homœopathic medicine! We have, indeed, seldom seen the correct definition of a homœopathic medicine more concisely stated.”

Is it not evident that these definitions, given by a Professor of the old school, and accepted by the Editors of a Journal of the new school, are very different from those given by Hahnemann? He says nothing about doses, and consequently, nothing about the contrary actions of certain larger and certain smaller doses.

In the same Review for Sept. 1887, there is a criticism of my latest tract—'Homœopathy and Antipraxy,' by Dr. Dudgeon, of whom I will venture to say that there is no man in this country so well acquainted with Hahnemann. In this paper there are many errors, but there is also a declaration of the truth we are at present in search of so clear that it should put a stop to discussion.

Dr. Dudgeon quotes the definition given by Dr. Lauder Brunton, and endorsed by the Editors of the 'Homœopathic Review,' "That homœopathy consists in giving for the cure of disease a *small dose* of a drug which will in a *large dose* produce symptoms similar to those of the disease to be cured;" and says—it "is not strictly correct, it should rather run: '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.' *Small and large doses are out of place in a definition of the homœopathic law.* The rule for the dose . . . is the outcome of experience."

This is a distinct return to Hahnemann's early definitions, and I think puts the matter beyond further discussion.

Such are a few definitions, others will be given in the course of the Essay. Before concluding this section something more must be said on behalf of *scientific induction*. It is a subject apparently very little understood by the majority of the medical profession. It has become customary to speak of the present empiricism of medical practice as *rational medicine* because its rules of practice are only of *partial* and not of *universal* application; while homœopathy because it claims to have a rule of universal application, is stigmatised as a system of *quackery*. As a justification of these extraordinary assertions it is contended that Hippocrates says that *similia similibus curantur* is true of the action of some drugs, and that *contraria contrariis curantur* is true of others. This seems to me a marvellous mistake, for:—

(1). Hippocrates not only did not know of the existence of a law like homœopathy or antipraxy governing the action of drugs, but he did not know that laws of this kind existed at all, or were possible, in any department of nature. The entire absence of modern scientific notions at that period of the world's youth, and almost until the

time of Lord Bacon, was explained when speaking of Aristotle in the beginning of Essay XLV.*

(2). For any branch of knowledge to be without a foundation on a law-fact, is a proof that that branch is not a science. It is nothing better than empiricism. Large and important a branch of knowledge as Medicine is, this absence of law in it is fully acknowledged, consequently it is presumption to call it "rational."

(3). Though Hippocrates could not be expected even to look for natural laws in Medicine, to do so now faces us sternly as a duty, and if a true generalisation or induction from individual facts can be arrived at, it is a truth in nature from which, by deduction, safe rules of practice may be obtained, and be relied upon without hesitation.

(4). Homœopathy claims, whether justly or not is not the present question, to be in possession of such a generalisation or induction, and *because* it does so it is condemned as a system of quackery. By such reasoning as this Astronomy and Chemistry are the greatest quackery in the world; for, the first is founded on the laws of gravitation as *universally* applicable within their province; and the second, in like manner, on the laws of combination in fixed proportions. These Sciences are "rational." Therapeutics, when it becomes a Science by being founded on a law *universally* applicable within its limits, will be entitled to be called *rational*, but not before.

(5). In this point of view Antipraxy stands where Homœopathy stood, and is exposed to the same condemnation. How unjustly! It claims to be a law *universally* true in its own sphere. It must not be forgotten that this sphere is limited to the action of *drugs* in health and disease. Within these limits it is capable of constituting Therapeutics and Science.

(6). When it is urged as a proof that those who accept a law like this for their guidance in prescribing medicines for their patients, neglect their duty to do all they can for the sick, and throw aside all other means of treatment, an amount of misunderstanding and unintentional misrepresentation is betrayed, which is astonishing. Surely, it is only common sense to say that laws like these, applying only to drugs, leave a physician perfectly free as to the use of all other means.

(7). It may be worth while to remind the writers we

* *Therapeutics*. Bell and Son. 1886.

are now addressing that Hippocrates, great a man as he really was, seeing that he took Medicine out of the hands of the priest-physicians, who at that time were "orthodox" and "regular," would certainly be branded by them a *quack*, however he may now be revered as the Father of medicine. It was not till six hundred years after his death that his reputation was first established, and then eclipsed by Galen.

(8). It is now argued that Science cannot go beyond probability or generality in its inductions—never to universality. If that is true it must be admitted that Science is a very poor thing. I venture to say that this view of Science is a mistake. The inductions, it is said, must be *demonstrated*; they are, therefore, the old *logical* inductions of Aristotle, and not the modern *scientific* inductions of Lord Bacon which are meant by these objectors.

It is surprising how little is understood in these busy days of what true Science is. Dr. Dudgeon quotes my words, that the contrary action of larger and smaller doses is on the *healthy*, and is "independent of disease, and has no necessary connection with therapeutics," as if I admitted a crushing defect, whereas it is a crowning merit. It is this independence of disease that makes Antipraxy a *scientific truth*, which medical men ought to avail themselves of by deducing practical rules from it, as astronomers and mariners get their practical rules from the laws of gravitation, which laws are independent of human affairs.

Our plain duty is, first, by *induction* to seek to become acquainted with the laws by which God governs natural phenomena, and next by *deduction* from these laws to obtain rules to guide us in our daily life, so far as we are concerned with these natural phenomena.

It is in this path that we may hope to see Therapeutics become a Science.

II. Hypothesis and Fact.

The wit and mind of man, if it work upon matter, which is the contemplation of the creatures of God, worketh according to the stuff, and is limited thereby; [*Facts*] but if it work upon itself, as the spider worketh his web, then it is endless, and brings forth indeed cobwebs of learning, admirable for the fineness of thread and work, but of no substance or profit [*Hypotheses*].”

LORD BACON.

(*Advancement of Learning.*)

For many years my friends have carried on a lively controversy with me as to what is hypothesis and what is fact; and especially, as to what are the uses and value of hypotheses. My views on these subjects have been so often repeated that they may be given very briefly on this occasion.

There are two kinds of hypotheses, or, at least, two very different uses to be made of them. These hypotheses are conjectures or guesses on any subject, which have not been proved to be true; because the moment they are proved to be true they cease to be hypotheses and become facts. In the work medical men are engaged in hypothesis is conjecture applied to natural phenomena; and it is evident that conjecture may be so applied in two very distinct ways. One of these ways is useful. The other is exceedingly hurtful. The useful way is to *suggest experiments*, and makes no pretention to go beyond this. The hurtful way is much more pretentious, and seeks to *explain* unexplained phenomena—to solve unsolved problems.

The first use is admissible, and conjecture of this kind may at any time be had recourse to. It is generally contended for as indispensable, but I have often taken leave to deny this. I am quite sure that discoveries in science may be made without hypotheses of any kind—not merely accidental or unexpected discoveries—but discoveries of laws or general or law-facts by experiment, observation, and reasoning, without the help of any hypothesis.

The second use of hypothesis—when it is sought by conjecture to explain or assign the causes of phenomena—strongly as this use is supported by distinguished writers, I cannot hesitate to condemn as one of the greatest hindrances to the progress of knowledge.

It may be helpful to give an example:—A new metal—Titanium—had been put into my hands. Thirty-two years ago (1856), I conjectured that it might be a valuable remedy. It was taken when in health, and it made me very ill—it had poisoned my blood. After two years a patient came to me who had been ill seven years; he seemed to be suffering from a similar blood-poisoning; he had Titanium given him in smaller doses than those I had taken; in a fortnight he was well. This is the good use of hypothesis. If I were to conjecture an *explanation* of all this it would, as it appears to me, be to undertake a task alike impossible and hurtful. Again, one of the accompaniments of this illness of mine was a serious albuminuria, which, in spite of all I could do, or my physician could prescribe for it, continued for two years. I conjectured that if Titanium had been the cause of it, it might also be the remedy. It was taken in very small doses, and this serious and persistent ailment was very quickly and permanently cured. There has never been the smallest return of it. Other cases of albuminuria have since been cured by it. If I were to start some hypothesis to *explain* all this I should think myself very foolish.

But even in the first sense of suggesting experiments, in my opinion, hypothesis is thought too highly of. The Life of Mr. Charles Darwin has just been published, perhaps no one was fonder of hypothesis than he was; he says:—“From my early youth I have had the strongest desire to understand or explain whatever I observed. .

. . . But on the other hand, I have steadily endeavoured to keep my mind free so as to give up any hypothesis, however much beloved (and I cannot resist forming one on every subject), as soon as facts are shown to be opposed to it. Indeed, I have had no choice but to act in this manner, for with the exception of the Coral Reefs,* I cannot remember a single first-formed hypothesis which had not after a time to be given up or greatly

* From the Transactions of the Victoria Institute for March 1888, it appears that Mr. Darwin would now have given up this solitary hypothesis.

modified. This has actually led me to distrust greatly *deductive* reasoning in the mixed sciences."*

After all this it will surprise some of my readers to find that Darwin may be brought forward as an example showing that discoveries in science may be made without hypotheses of any kind. He himself says of his greatest book—'Origin of Species'—"I worked on true Baconian principles, and *without any theory* collected facts on a wholesale scale."

When he did what he was so fond of doing and invented hypotheses, he tells us he had to give them up; this led him to distrust *deductive* reasoning. Let it be observed that this deductive reasoning was from a starting point of hypotheses which were not true, and, of course, conclusions drawn from them were not likely to be true. When the deductive reasoning starts from a law-fact there is no need to distrust the conclusion.

There are two kinds of physical facts—individual, with which we become acquainted by the exercise of our five bodily senses, and concerning which more need not at present be said; and—general or law-facts, with which we become acquainted by a mental process of reasoning already described as scientific induction.

The general or law-facts with which we are acquainted are strictly limited to their sphere of action, *e. g.* Chemical attraction is limited to insensible distances, takes place only between particles of a different kind, and is connected with fixed ranges of temperature. So, also for example, Antipraxy does not include *all* large doses and *all* small ones. It has already been expressed as existing between *certain* larger and *certain* smaller doses of each drug, and attention is earnestly called to this. These ranges or groups of doses, of course, will vary with each drug, and to a less extent with each person. Hitherto the dose, in both old and new schools, has always been the "outcome of experience;" it will be so still, but with this important distinction, that it will now have a scientific guide, when before it had only an empirical one.

We may be mistaken, and accept as a law-fact something which is not true. Doubtless! So we may also be mistaken about an individual fact. Happily, our mistakes do not vitiate either individual facts or law-facts. Nature

* *Life of Charles Darwin*, vol. i, pp. 103, 149.

goes on just the same, whether we understand its operations or mistake them. Mathematicians and logicians may sometimes err, but no one blames Mathematics or Logic for this.

I have just heard a new objection, which is hopeful as showing that the old ones are beginning to hide themselves.—“We call your law-fact a ‘*working* hypothesis,’ which means something more than the word ‘hypothesis’ by itself.” Then it is desirable to give it a better name. That language may be understood it is necessary to put limits to the meaning of words. Elasticity in the meaning of words is the cause of endless misunderstandings and hopeless controversies. “Working hypothesis” applied to a law-fact is a misleading phrase as well as a disparaging one. Are we to think that the law-fact of *gravity* is nothing more than this? It is replied:—“The law of gravity has the advantage of age, and of many additional confirmations.” Yes; but it was as true on the day it was discovered by Newton as it is to-day. Its present advantage is an artificial one—it is now *orthodox*; so that any one presuming to question its truth would lose caste and be despised or thought deranged. In the first instance the fact of gravity and its laws, like every new truth, had to pass through a painful stage of opposition and slight.

But again it is said:—“Speaking academically, that is, either as a mathematician or a logician, we should call your induction of a law-fact only a probability or hypothesis, because its subjects cannot be dealt with as *necessary* truths.” We have seen (in the first section) that natural science cannot be restricted to necessary truths; that it is impossible to make the enumeration of the phenomena of any type complete; that, therefore, logical induction is inapplicable. Other methods must of necessity be resorted to, and the results of the scientific induction defended in these essays, when worked out with sufficient care, are so true that, in practical life, they may be trusted without fear. These results are certainly more than “working hypotheses.”

Mathematics and Logic deal with *necessary* truths. Physical Science deals with *actual* truths, but which are *not* necessary ones. Therefore, the two methods of reasoning must be different; but it does not follow that the method of science cannot attain to anything more than a probability or hypothesis. Let us take an example:—

The law-fact of Chemistry, which is its foundation as a Science, is the combination of elementary bodies (so-called only because not yet decomposed), in *fixed* proportions. Now, common salt is a compound of Chlorine and Sodium in the proportions by weight of three parts of Chlorine and two of Sodium. It cannot be asserted that these proportions are *necessary* in the mathematical or logical sense. No reason can be given why the combination might not have been fixed to take place in many other and quite different proportions; nevertheless, we cannot doubt the truth of the combination in the proportion of three and two. In no other proportions will Chlorine and Sodium combine to make common salt. As, on the one hand, it would be going absurdly beyond our knowledge to say that this is a *necessary* truth; so, on the other hand, it is, I think, equally falling short of our knowledge to say that this truth is only a conjecture or hypothesis—working or not working—*because* it is not a necessary truth—it is an actual truth though not a necessary one; it is a fact, and not a guess.

III. *Small Doses.*

“It is the duty of every one to spread what he believes to be the truth.”

CHARLES DARWIN.

There can be no wise treatment of the sick that is not based upon *truth*, and it is confessed that Therapeutics is not yet founded upon truth. Indeed, so far is this confessed that the possibility of it is thrown into the remote future by one who ought to know. He writes to me:—“By its nature Therapeutics must, as a science, or as more than *a mere set of rules from experience*, be the most difficult part, and the nearest to the end and climax of medical knowledge. I do not yet feel *near the end* of pathology and diagnosis.” Surely, this is sufficiently discouraging. How many generations of men, women, and children, are, humanly speaking, to go on dying prematurely, as they have done for so many thousands of years, before medical men, whose paramount duty it is to discover a true

science of Therapeutics, can show to some appreciable extent, that they have fulfilled this duty?

What are the great impediments? They are prejudice and self-interest. Many years ago* my contemporaries were reminded of the invention of the telescope and the discovery of Jupiter's moons by Galileo, and how this discovery was treated. "Many positively denied the possibility of such discoveries." Even Kepler "considered it totally incredible." "Moreover," another argued, "these satellites are *invisible to the naked eye*; therefore they can exercise no influence on the earth; therefore they are useless; therefore they do not exist." One of Galileo's brother Professors "pertinaciously refused to look through the telescope." Other evil things were said, "concluding with attributing the alleged existence of these stars to Galileo's thirst for gold!"

So it appears that one of the characteristics of truth is its rejection by those who ought to be most interested in it. No one now hesitates to condemn the persecutors of Galileo, and at the same time how few hesitate to follow their example! Nearly all oppose to the uttermost of their power that Truth which is struggling to make itself known in their own times. This opposition is a broad road, and the present generation is pursuing it with as much alacrity as did their fathers.

But Truth is not the less true because it is rejected, and there will always be a few who are honest and unselfish searchers for it, and who, when they have found it, can accept and admire it. To them truth is beauty and a joy for ever.

The *small doses* considered in this Essay are one grain of a solid drug, as of minerals, and one drop of a liquid, as of the sap of living plants, and the divisions of this grain or drop till they reach the millionth of a grain or drop. These divisions are appreciable quantities and not infinitesimal. As it is the action of the smallest of these doses that is questioned, it shall be proved of them that:—

1. *They exist.*

A short time ago a medical Professor, with whom I had had some pleasant intercourse and correspondence, wrote

* Essay XII, 1854.

to me that he could not believe in the millionth part of a medicine. In my reply he was urged to try the experiment of testing the millionth part of a grain of iron dissolved in sulphuric acid with a minute quantity of Prussiate of potash, that he might *see*, by the production of a faint blue on a white plate, a proof that there was present *an appreciable quantity of iron*, a quantity *sufficient to act upon the optic nerve*, and therefore, presumably sufficient to act upon other nerves of a living body. To this request he replied:—"Your suggestion about the iron test and Prussian blue I venture to think altogether beside the mark, for we were not discussing chemical tests but actions upon the human body." True: we were not discussing chemical tests, but actions upon the living human body, and the experiment he was asked to try was not a chemical but an *optical* experiment—an experiment to discover the presence of iron *by its action on the retina*.

This experiment was tried by me with success many years ago. It has now been repeated for me by two very able men in Rugby and the resulting colours I have to show to any one who will come and *look* at them. But it will be far better for others, who are sufficiently acquainted with chemistry, to try the experiment themselves. It is this—take four grains of proto-sulphate of iron (gr. 3.846 contains one grain of iron) and dissolve it in 100 drops (there is also the water of crystallisation) of distilled water. Add one drop of this first dilution to 99 drops of water and shake it. Add one drop of this second dilution to 99 drops of water and shake it. This is the third dilution and each drop will contain the millionth part of a grain of iron. Put upon a white plate a minute quantity of a crystal of the red Prussiate of potash (ferrid-cyanide of potassium) and drop upon it one drop of this third dilution and allow it to evaporate, there will remain a visible blue stain. To this experiment others may be added. For example—the third dilution of musk can be recognised by the olfactory nerve; and the third dilution of creasote can be both tasted and smelt.

In the interesting books of Dr. Lionel Beale there are many beautiful engravings illustrating the views of bioplasm or living matter. They are given from magnifying powers of from 1500 to 3000 diameters, and even 5000 diameters is mentioned.* The millionth of a grain

* *Life Theories*, 1871, pp. 62, 63.

of iron must be a bulky and heavy thing compared with the delicate component parts of this living matter. The particle of iron requiring to be magnified 5000 diameters to become visible would be as $\frac{1}{125,000,000,000}$ to $\frac{1}{1,000,000}$ As the one hundred and twenty-five thousand millionth to the one millionth of a grain.

After these examples it cannot be rationally doubted that a drug may easily be divided into a million of appreciable parts.

2. *They have power to act upon the living body of man.*

Up to the present hour this fact is stoutly denied by the party in the Profession which, being the greatest number, has the loudest voice. And in the words of Dr. Liddon:—"our experience shows that when the human will is strongly disposed to ignore *the practical consequences* of a fact, it has a subtle and almost unlimited power of blinding the intellect to the most elementary laws of evidence." There are some who are not willing to be blinded in this manner. They are asked to read, and to test in their own practice, what follows:—

First, it shall be shown them by the unimpeachable testimony of Charles Darwin what exceedingly small doses have power to act upon living plants—a *fortiori* must not similarly small doses have power to act upon living animals? The following extracts are made from the Life of Mr. Darwin just published. They prove both the mechanical and chemical power of exceedingly small doses:—

"To Sir J. D. Hooker, (1860)—Here is a fact for you which is certain as you stand where you are, though you won't believe it, that a bit of hair $\frac{1}{78,000}$ of one grain in weight, placed on a gland [of Drosera], will cause one of the gland-bearing hairs of Drosera to curve inwards, and will alter the condition of every cell in the foot-stalk of the gland." Vol. iii, p. 319.

"To Sir Charles Lyell—I declare it as a certain fact, that one organ is so sensitive to touch that a weight 78 times less than that, viz. $\frac{1}{1000}$ of a grain, which will move the best chemical balance, suffices to cause a conspicuous movement. I am perfectly sure that this is true." P. 320.

"To Dr. Asa Gray, (1860)—You will laugh, but it is

at present, my full belief (after endless experiments), that they [the leaves of *Drosera*] detect (and move in consequence of) the $\frac{1}{2880}$ part of a single grain of nitrate of ammonia." P. 318.

"To Dr. J. Burdon Sanderson, (1873)—I made a solution of one part of phosphate of ammonia by weight to 218,750 of water; of this solution I gave so much that a leaf got $\frac{1}{8000}$ of a grain of the phosphate. I then counted the glands, and each could have got only $\frac{1}{1,552,000}$ of a grain; this being absorbed by the glands, sufficed to cause the tentacles bearing these glands to bend through an angle of 180° . Such sensitiveness requires hot weather, and carefully selected young yet mature leaves. It strikes me as a wonderful fact. I must add that I took every precaution, by trying numerous leaves at the same time in the solution and in the same water which was used for making the solution." P. 324.

Here is the evidence of a man whose whole life was devoted to Science, and spent in minute observation and experiment. He testifies to the power of action on living plants of small doses culminating in less than a million and a half of a grain. Mr. Darwin, however, does not conclude with this, he writes:—

"To Professor F. C. Donders of Utrecht in 1874—My son George tells me on your authority of a fact which interests me in the highest degree. It relates to the action of one millionth of a grain of atropine on the eye. . . . The $\frac{1}{4,000,000}$ of a grain [of phosphate of ammonia] absorbed by a gland (of *Drosera*) clearly makes the tentacle which bears this gland become inflected; and I am fully convinced that $\frac{1}{20,000,000}$ of a grain of the crystallised salt (*i. e.* containing about one third of its weight of water of crystallisation) does the same. Now I am quite unhappy at the thought of having to publish such a statement. It will be of great value to me to be able to give any analogous facts in support."

Mr. Darwin looked for support to the analogy of the action of small doses on living animals; we may still more confidently look at his experiments on living plants, as offering us the strongest support that analogy is capable of giving.

Medical men now very generally admit the theory of germ diseases—diseases caused by minute living things, *e. g.* the *Bacillus*, which we are told in the second edition

of Dr. Williams' book on "Consumption," measures from the 8000th to the 12,000th of an inch in length, and from the 40,000th to the 60,000th in breadth ; (we are not told its thickness). How much will one of these creatures weigh? And how much each of its organs or parts?

It is not necessary to adduce other causes of disease, such as the infection of scarlet fever, measles, mumps, &c. &c. These are far more minute than we have need of as proofs of the existence and power of the millionth part of a grain of a drug. Mr. Darwin has already carried us into the region of infinitesimals among which we profess not to be travelling. For, let me again remind my readers, my experiments with small doses in health were not carried beyond the millionth of a grain or drop—that is, they ended with appreciable quantities, and did not advance to infinitesimal doses. Let me also remind them that, *practically*, the *hundredth* part of a grain or drop was the dose commonly employed both in experiments in health and in prescribing for the sick. This is the smallest dose with which any one should begin his experiments. Experience thus gained will qualify him for going further.

What Mr. Darwin anticipated I have experienced for six and thirty years. The language used with reference even to these appreciable doses has often surprised me, coming from men whose self-respect alone, without other considerations, one would have thought sufficient to restrain them from using it. And how are ridicule and abuse to be replied to? They are not argument. I have appealed to facts, and have described experiments. These are called "Dr. Sharp's affirmations," and it is said "something more is needed."* Obviously this "something more" must come from others; it cannot come from me. Let my experiments be repeated by competent and unprejudiced men. Let *Aconite*, and *Digitalis*, and *Phosphorus*, and *Spigelia* be tested in small doses as to their action on the heart, in healthy persons first, and afterwards on sick ones. Let *Chamomilla*, and *Myrica*, and *Mercury*, and *Podophyllum* be carefully tested as to their action on the liver in like manner. Let *Opium* be tested as to its action on the brain and bowels. Let *Castor oil*, *Bryony*, and *Veratrum album* be tested as to their action on the bowels. Let other known drugs be experimented

* *Medical Press and Circular*, Dec. 7, 1887.

with, and let the effects of all, when clearly ascertained, be honestly published, and if truth can be discovered let it prevail.

Having said thus much about my experiments with small doses of drugs taken in health, I may confidently claim that the action of these doses will be the same, or at least tend to be the same, when taken in sickness. Drugs are not conscious beings that can recognise disease, and in consequence act differently. The sphere of proof of what that action is, is in this manner immensely extended, because many medical men have been giving these small doses as medicines for many years, and have had manifold evidence every day of their power to act upon the organs of a living man. This being the case, it is no presumption to say, as Mr. Darwin did, though he knew he should be laughed at:—"Here are facts which are as certain as that you stand where you are, though you won't believe it." This leads us to another "affirmation" that

3. *The power is sufficient to be available in the treatment of disease.*

This ought not now to be a questioned statement. It has been verified by the success of what has been called *Homœopathy* during more than two generations, in spite of all the imperfections and difficulties which have encompassed the new method. The force of this evidence is supposed to be destroyed by calling those who practise in this manner "Quacks" and "Impostors." What power can abuse have against facts? For a time it has a great deal, but the power is not permanent, and it perishes in disgrace. My own practice, like that of others, has been before the world and in the face of the Profession, and, notwithstanding the most watchful and persistent opposition that it was possible to carry on for thirty years, my patients recovered far beyond what has happened to me during the previous thirty years of successful (as esteemed by others) orthodox prescribing. But it will be said again—"we need something more than Dr. Sharp's affirmation." Permit me to say in reply, to every medical practitioner—"Go, and do likewise."

And they may do so now with less to fear. The air is becoming impregnated with "small doses," and before

long their influence will be irresistible. Already there are signs of the approach of this consummation. On Saturday the 3rd of March, Sir James Paget delivered the annual address to students of the London Society for the extension of University Teaching at the Mansion House. It was significant to hear Sir James, while discoursing on "knowledge being really happiness," telling the assembly how he had been charmed with small doses of music. He had heard Mdlle. Janotha play 5995 notes in four minutes and three seconds, each note involving three distinct muscular motions or 72 motions in every second; then there were design, and memory, and distinct sense of touch, and distinct sense of sound; so for every second of time there were 200 transmissions of nerve force from and to the brain!

4. This is the best use of medicines which has yet been discovered.

The reason for the existence of a medical profession is that there are always suffering sick people who are craving relief. The object which medical men are believed to have in view is to administer this relief. The fact that, up to the present time, they often aggravate this suffering instead of mitigating it, is notorious. The most eminent medical men have been constrained to acknowledge the unsatisfactory condition of medicine. Some are labouring earnestly in various ways to improve it. Some will only "follow their leader." While some are so low in intelligence that they contend that Medicine is not a science, but an art or trade, and practise, as other artificers work, simply for a living. I have no right, and certainly no wish to give the experience of others, but it is a duty to give my own, and this has been given so often already that it would be an impertinence to offer it again.

It may be worth while to remind my readers that the magnifying powers of microscopes mentioned by Dr. Lionel Beale are not the limits now attained by the makers of them. Sir Henry Roscoe in his Address last year (1887) as President at the Meeting of the British Association at Manchester, speaks of 6000 and 8000 diameters and tells us that the diameter of an atom of oxygen or nitrogen is $\frac{1}{10,000,000}$ of a centimetre; while with the highest known magnifying power we can distinguish only the $\frac{1}{40,000}$ part

of a centimetre. Plainly, therefore, microscopes however powerful are limited by their powers—not by the confines of natural phenomena.

I have again and again stated that my experiments with small doses in health have been limited to appreciable doses, they have not gone beyond the third trituration or dilution—that is, the millionth part of a grain or drop; and, in every experiment, the dose actually taken is recorded.

In this difficult investigation of Hahnemann's system it has been my practice to study one thing at a time—a method not without objections to it, but which may be recommended to others—and to this it may be added, that the time for the satisfactory study of *infinitesimal* doses has not yet arrived.

IV. *The local action of Drugs.—Organopathy.*

“It went bang to the spot!”

A MILITARY PATIENT.

Mr. Darwin, in a letter to Sir J. D. Hooker in 1875, about his book on “Insectivorous Plants,” wrote:—“I begin to think that every one who publishes a book is a fool.” Certainly, if the author “seeks the praise of contemporaries, or pants for posthumous renown,” Mr. Darwin was not far wrong. But there are other motives besides these by which men may be urged to write books, and when they are compelled to write them by these, the unfair criticisms of their contemporaries and the dark cloud which hides the future will not deter them from going on with their work.

The Essay XVII, called “Organopathy,” was published twenty years ago, (1867). It was addressed to the disciples of Hahnemann. From them it met with, at first, unseemly

ridicule ; then vehement opposition ; then, after about a dozen years, it was accused of being plagiarized from a German ; and as we shall presently see, it is still fiercely condemned and rejected by homœopathists.

Now it is beginning to be noticed by physicians of the old school and is criticised thus :—“ The doctrine which you called ‘ Organopathy,’ and which asserts that each drug acts more powerfully on certain tissues and organs than on others is a view which has been held by many ever since the days of Hippocrates, and I never even heard of any reasonable medical man who doubted it.”

This criticism I might decline to notice beyond asserting that I have not the slightest wish to rob any of my predecessors of the merit which belongs to them. But as silence may be misinterpreted a sufficient reply shall be given.

(1). At the time ‘ Organopathy ’ was written, the subject which was engaging my attention day and night, was Hahnemann’s system of Homœopathy. By slow degrees the *local action* of many individual drugs so forced itself upon my observation, that at length, by scientific induction, it became a clearly established *law-fact*. In the Essay referred to it is said :—“ Hahnemann looked upon all diseases as derangements of the *vital force* or life of the body ; and he asserts that the action of drugs is also upon this force or life. This view implies a *general action* both by morbid influences and by drugs.” The statement of *local action* now made was, therefore, a direct contrary to the doctrine of Hahnemann ; and the conviction of its truth arose, as I have said, in my own mind, while testing in actual practice the teaching of Hahnemann.

(2). Had the Paper been written now, and been addressed to the old school, it would have assumed a different form. But as it stands, there is more than enough in it to show that I was not only well aware of the empirical knowledge of local action which had long existed, but took advantage of it in my statements. The ‘ Nosologies ’ of Cullen and Mason Good, the ‘ Materia Medica ’ of Pereira, and the writings of Christison, are largely quoted. This proves that I could not have any desire to ignore what was already known.

(3). But the empirical knowledge of local action possessed so long and so universally by the Profession, falls very far below what is meant by Organopathy, both as

regards the fact of local action, and also the use to be made of it; so that there is no excuse whatever for a charge of plagiarism here.

(4). As regards the *fact* of local action, the knowledge possessed was loose and hazy, instead of being precise; it was that *most* medicines acted on *some* parts of the body more than on others, but that there were also *some* which had a general action, and the giving of these was called constitutional or general treatment, as by alteratives and tonics. It was empirical not scientific knowledge. Christison is quoted in the Essay as one who, in respect to *poisons* contended against this general action. He says:—"Poisons have been often, but erroneously, said to affect remotely the general system. A few of them, such as arsenic and mercury, do indeed appear to affect very many organs of the body. But much the larger proportion seem, on the contrary, to act on one or more organs only, *not on the general system.*"* In another place he says:—"It appears that an action of such a kind [*i. e.* a *general action*] is rare."† Christison's knowledge of the action of *poisons* was not formulated by him into a law-fact, for these do not admit of exceptions however rare. The learned Professor, to whose criticism I am now replying, has not perceived this fundamental distinction between empirical and scientific knowledge.

(5). As regards the *use*, which up to that time, (1867), had been made of the local action of drugs, such knowledge as was possessed of it was employed in a very different direction from that which I am anxious to recommend. As every one knows, drugs were classified as Emetics, Purgatives, Diuretics, Diaphoretics, Febrifuges, Narcotics, &c. &c.—than which nothing can be more wanting in precision. The question asked was not—On what organs does the drug act? but—What effects does it produce? If Ipecacuanha was to be given, it was not because it acts on the stomach, but because it produces vomiting. If Opium was prescribed, it was not because it acts on the brain, but because, in the large doses usually given, it puts people to sleep—sometimes never to wake again. It is pleasant to see that some effort is now being made to get away from this discreditable position. Dr. Lauder Brunton, in his recent 'Pharmaco-

* Christison's *Treatise on Poisons*, 4th edition, p. 19.

† *Ibid.*, p. 4.

logy,' takes much pains to indicate the organs upon which drugs act. He has not yet seen his way to 'Organopathy.' "By Pharmacology," he says, "we mean a knowledge of the mode of action of drugs *upon the body generally*, and upon its various parts." Nor, in regard to the uses of drugs, has he banished the inadequate headings—Astringents, Purgatives, &c. Inadequate because the drugs classed under these headings must change places with each other, when their doses are changed. As long as the College of Physicians has in its Nomenclature "General Diseases," so long the Pharmacologists are bound to provide general remedies. It is to be hoped that the time is not distant when both will have passed into oblivion.

(6). The truth is that *the local action of drugs*, of which my critic boasts, as having been known from the days of Hippocrates, and of which he never heard any reasonable medical man doubt, has been all along, and is yet, so imperfectly known that much earnest and anxious labour must be continued for two or three generations, before a knowledge of it, in respect to any considerable number of drugs, will be acquired which can deserve the title of *definite*. Experiments on ourselves in health, with one drug at a time, and in different doses, is the method most likely to succeed.

(7). The use of strange technical words is to be avoided as much as possible, but this cannot always be done. The local action of drugs, and the use to be made of it by physicians, necessitated a *name*. Organopathy was given to it as the most obvious in its meaning. It embraces three subjects, 1st. The seat of disease, which, in all cases, is primarily local. 2nd. The action of drugs, which also, in respect to every drug, is primarily local. 3rd. The selection by the physician when prescribing, of a drug whose action is where the seat of disease is.

(8). Finally, it is evident that the knowledge of the local action of drugs possessed up to this time, does not meet the present demand. Something more definite must be discovered. Organopathy puts the question, and requires an answer:—In what organs or parts of the body do the actions of *each drug* take place? This local action when known will be found to be characteristic of each drug. And further, it is to be felt and acknowledged with sorrow, that the use of any drug as a medicine,

without this characteristic local action being known, is empirical, temporary, and unsatisfactory. Such knowledge of the action of drugs as is contemplated by Organopathy, so far from having been long and universally known, as hastily asserted by my University critic, is in reality only very imperfectly apprehended by a few physicians, and in respect to a small number of drugs.

If now we turn to the antagonism of the homœopathists, we shall be astonished at the change in the character of the attack. The contrast is ludicrous. We have seen, on the side of the old school, that the fact of the local action of drugs has been known from the days of Hippocrates, and my critic "never even heard of any reasonable medical man who doubted it." On the side of the new school the teaching of this local action under the name of Organopathy has been persistently denied, condemned, and rejected by homœopathists, ever since it was announced in 1867. The latest attack is by Dr. R. E. Dudgeon in a Paper in the 'Monthly Homœopathic Review' for last September (1887). What, in this Paper, refers to Organopathy may be noticed here; what belongs to Antipraxy in the next section of this Essay.

Dr. Dudgeon begins by asking a question:—"Is it possible to localise diseases?" The words are equivocal. Their grammatical meaning is—is it possible for us to put diseases into their seat? They cannot have been used in this sense. They may mean—is it possible for diseases to have a seat? Or they may mean—even if diseases are local, is it possible to discover their seat? We will take the words in this last sense, because if we find it possible to discover the seat of diseases, it is then certain that they are local.

Does my critic make any admission in regard to this? If so, the question he asks may, perhaps, be quickly answered. Let us hear:—"As regards many diseases their name indicates their locality, as peritonitis, pleuritis, pneumonia, meningitis, endo- and pericarditis, hepatitis, cystitis, phlebitis, keratitis, &c."

This admission so distinctly answers the question in the affirmative that it is scarcely necessary to add another word. Dr. Dudgeon replies to his own question that it is possible to discover that *many* diseases are local. The diseases he mentions are acknowledged by him—as they

are by every "reasonable" medical man—to be local diseases.

If there are so many diseases already known to be local, is it not easy to see that, by taking pains, their number may be increased more and more, till *all* diseases are "localised"? The mental process of scientific induction anticipates this conclusion, and is a great gain. An induction from a sufficient number of individual facts has given us this law-fact, that all diseases are local. This law-fact is expressed in the word Organopathy. Dr. Dudgeon has saved me from the necessity of relating the individual facts which led me to this conclusion. His own catalogue furnishes us with a sufficient number for such an induction.

That *diseases* are local is, therefore, satisfactorily admitted. We have next to ask about *drugs*. The question is thus put by Dr. Dudgeon:—"Can we specify the precise organ acted on by our medicines when we prove them on the healthy?" And this is followed by an equally satisfactory and sufficient admission. Dr. Dudgeon answers his question in these words:—"In some cases of poisoning the *post-mortem* examination shows one or several organs more or less altered from the normal state; and some of the objective symptoms of our provings sufficiently show irritation of internal organs and tissues." An answer very familiar to all students of Toxicology, and to all acquainted with the provings of drugs in health.

Thus the truth of Organopathy is established by evidences found in the Paper of its opponent.

The remarks which follow these admissions Lord Bacon would say are puerile. I will notice only two:—"But in how many cases are we not unable to connect the symptoms observed during life with any particular organ? . . . What then are we to do?" We are to do our duty, and deal with Organopathy just as Dr. Dudgeon deals with his "law of similars." He will allow that, even in his long practice, he has not yet discovered the remedy for every case. Does he give up his law on this account, and admit that it is a fallacy? Does he not try to diminish the number of his incurable cases as much as he can? We are to do the same. That a thing has not yet been done does not prove that it cannot be done. Neither does the difficulty of a thing prove it to be untrue.

He concludes with these words:—"This old and discredited *organopathy*, which is a revival of the futile speculations respecting the proximate cause of disease," &c. How can the *seat* of a disease be its *cause*? Apoplexy has its *seat* in the brain—is the brain the cause of the apoplexy? Moreover, there is no relation between the pathological hypotheses of the last century and the pathological facts of the present day. Organopathy is a scientific induction from a sufficient number of individual facts. It is neither old nor discredited. But it is opposed by homœopaths because they follow Hahnemann, and he rejected pathology. Dr. Dudgeon, in one of his 'Lectures,' says:—"It is greatly for the interest of homœopathy that it should have a scientific pathological basis, which, it must be confessed, *was not procured for it by Hahnemann.*" Yet, when an attempt of this kind is made it must be scouted, because it opposes Hahnemann!

Organopathy may be an erroneous induction, but Dr. Dudgeon must be reminded that negative evidence cannot prove that it is. To instance, as he does, diseases and drug-actions, the seat of which we do not at present know, is nothing to the purpose. He must adduce diseases and drug-actions *which have no seat*, if he would prove Organopathy to be an error.

V. *The contrary action of different Doses.—Antipraxy.*

"There is nothing for it but patience—and that conquers all things."

THOMAS CARLYLE.

It has again and again been insisted upon by scientific men that "to the idea of Science an express recognition and distinct apprehension of general laws *as such*, is essential." It must be evident, therefore, that Therapeutics is not yet a science. It is painfully evident also, that very few Medical men, even among the leaders of the Profession, have this apprehension of what is essential

to science. To bring into Medicine one or more general laws has been the earnest endeavour of these Essays, as those who are acquainted with them do not need to be reminded. The effort may fail, but it is better to have made it and to fail, than not to have made it.

It has been maintained that when a physician is consulted by a patient, his first duty is to observe carefully the symptoms, and next to discover the seat of the disease from which the symptoms proceed. This is a deduction from the law-fact that all diseases are local. His knowledge of drugs ought to enable him to fix upon those whose action is in the same seat—a deduction from the law-fact that the action of all drugs is local.

This is not sufficient to enable him to prescribe. Diseases in the same organ vary, and the drugs which act upon that organ also vary in their action. An enquiry has to be made as to the *kind* of morbid action going on. When we are competent to discover the kind of action of the disease, we may hope to find a drug whose action, in the smaller doses is of the *contrary* kind. In our present dearth of knowledge this cannot always be done, but it is the object to be aimed at. When we know a drug whose action is in the same seat as the disease, and the small doses of which act in the opposite manner, it may not be doubted that it is the best remedy for the patient which can be found, and if a cure by medicines is possible, he will be cured by it. The law of uniformity has two additional applications in this paragraph—to the kind of action of the disease, and to the kind of action of the drug.

Antipraxy, then, embraces three things—(1). The kind of action of the disease. (2). The kind of action of the drug. (3). The choice of a dose small enough to have an action contrary to that of the disease.

The same writer who says of Organopathy, that it has been held by many ever since the days of Hippocrates, goes on to say:—“Secondly, may I remark that the doctrine of Antipraxy is *opposed to all that we at present know* regarding the action of remedies, and that you do not bring any evidence that it is worth a moment’s consideration in support of it.” Everyone knows Organopathy, no one knows Antipraxy.

That Antipraxy is opposed to all that my critic knows is, doubtless, true of himself, but it is not true of others.

In the book which he is criticising, on page 140,* he might have read what one well acquainted with Pharmacology—Dr. Lauder Brunton—has said:—"There are *many* drugs, which, in small doses will produce an action the *contrary* of that which they produce in large ones." Many individual facts of the same kind have, of late, been noticed by other writers, but no inferences have been drawn from them.

Then it is said—"You do not bring any evidence that is worth a moment's consideration in support of it." I hoped that it was unnecessary to reproduce all the experiments recorded in previous Essays, for they have been numerous. The first of the Papers in the 'Practitioner,' (for May 1878), may also be referred to. But the following are given in the book criticised,* as examples:—

Opium, in its contrary action in larger and smaller doses, on the brain, and on the bowels.

Castor oil, in its contrary actions on the bowels.

Aconite, in its contrary actions on the heart.

Belladonna, in its contrary actions on the pupil.

It requires a good deal of assurance to say that none of these individual facts are "worth a moment's consideration." But "all is impossible" in some minds, said Lord Bacon, "that is not already found." Perhaps it was the absence of a moment's consideration which hurried the writer to say what he did, and, if so, he may charitably be forgiven, though he will scarcely so easily forgive himself.

But, possibly, others may wish to be reminded of more of my experiments in health with small doses, and more shall be given, with a sincere apology for the repetition, to those who already know them.

Digitalis, acts in a contrary manner on the heart in larger and smaller doses. And also on the kidneys. It has long been given in large doses to increase the quantity of urine. I have taken it in doses of the one hundredth part of a drop of the tincture, till there was *suppression*.

Phosphorus, in the larger doses quickens the heart's beats, in the smaller ones it makes them slower.

Physostigma, (*Calabar bean*), has an action on the iris the reverse of that of *Belladonna*.

Spigelia, (*S. anthelmia*), acts upon the heart; from larger doses the pulse is irregular, now slow, now quick,

* *Therapeutics*. Geo. Bell and Son. London, 1886.

falls from 72 to 54, with strong beatings of the heart; from smaller doses the pulse was raised from 77 to 84, with a preceding slight fall—the action of an intermediate dose.

Arsenic, larger doses acting on the stomach, destroy the appetite, smaller ones—the 2nd and 3rd dilutions—exaggerate it. Larger doses acting on the bowels cause diarrhœa, smaller ones cause constipation.

Chamomilla, (*Matricaria C.*) acts on the liver; larger doses diminish the secretion of bile, smaller doses increase it.

Myrica, (*M. cerifera*, *Bayberry*), acts on the liver; larger doses diminish the secretion of bile, smaller doses increase it.

Mercury, (*the triturated metal*), acts on the liver; larger doses diminish the secretion of bile, smaller doses increase it.

Podophyllum, acts on the liver; my note is this—“I think that the larger doses increase the secretion of bile, and that the smaller ones diminish it.”

Bryonia, (*B. alba or dioica*), acts on the bowels, larger doses constipate the bowels, smaller ones relax them.

Veratrum, (*V. album*), acts on the bowels, larger doses purge violently, smaller doses produce constipation.

Here are fifteen drugs, and they are not nearly all that I have experimented with, but they are sufficient to justify the inference drawn from them. It is my conviction that Antipraxy, and Organopathy, may safely be allowed to take their places among the known laws of nature, may become a practical basis of Therapeutics, and may thus elevate it into a Science. It must not be forgotten that “to the idea of Science an express recognition and distinct apprehension of general laws as such, is essential.”

It is necessary to repeat once more, that these are experiments *on healthy persons*, from which the induction or inference of Antipraxy was drawn.

And it seems necessary to repeat also once more, that the doses used in these experiments ranged from one grain of a solid and one drop of a liquid (as of the sap of a plant), to the millionth part of either. These are what is meant by “small doses.” The actual dose used in each experiment was carefully noted in the first reports.

Antipraxy was arrived at by these experiments in health; but it admits of being corroborated by experi-

ments with similarly small doses on the sick. I might refer to the experience of a large body of medical men, but their evidence is best given by themselves, and I will content myself with giving my own. I have had sixty years of hard practical work in my Profession. The first half was done in what are called "orthodox" methods, and in a way which lookers-on said was very successful; the second half was devoted to an independent investigation of Hahnemann's Homœopathy. During this latter period many thousands of patients, in almost every station of life and suffering from almost every kind of disease, were treated exclusively with "small doses" of drugs. Notes of every case were taken at the time; from these an Appendix to these Essays is in manuscript, containing upwards of thirteen hundred cases, as specimens of the successful and unsuccessful results of the whole practice. These are not to be published until the present temper of medical men is greatly altered. To print them now would be to expose them to be trampled under foot.

It is extremely difficult to persuade physicians to try these experiments, either on the healthy or on the sick, but occasionally this has been done. Dr. Lauder Brunton tells us he was persuaded to try what *Opium* would do in constipation, and whether any dose would give relief. He says:—"Not knowing what dose would be sufficient to produce this effect, I began with *one drop* of the 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, while *half a drop acted as a brisk purgative.*" These were cases of constipation accompanying "ovarian tenderness." Dr. Brunton adds this note:—"It is evident that *Opium* used in this way will *not act* as a purgative in cases of constipation depending upon general insensibility of the intestinal nerves."* How is this evident? Has he tried the experiment? A large experience justifies me in telling him that, in torpor of the bowels, nothing has succeeded so well with me as tincture of opium given night and morning in doses of the one hundredth part of a drop. Dr. Brunton says, in another place, *Opium* "in moderate doses lessens peristaltic action and causes constipation. In *very small doses* it generally increases peristalsis and acts as a purgative."†

* *Pharmacology, &c.*, 3rd edition, p. 386.

† *Ibid.*, p. 856.

Antipraxy has now been before the medical public for fourteen years, and it may be asked—What has been its reception? The answer is, opposition and criticism without experiment, and neglect amounting to a “conspiracy of silence.” Lately, a twilight before sun-rise has appeared. Among private letters this is an extract from an eminent Pharmacologist:—“I think your rule of Antipraxy comes nearer to the truth than any other yet formulated, but I do not think it includes the whole truth. I look upon it as a useful rule of practice, but one which one should take only for temporary guidance, while seeking for further knowledge.” And in another letter:—“The rule which you formulate under the title of Antipraxy . . . approaches as near to the truth as any general rule can in the present state of our knowledge. I regard it as a rule which may be of very great utility in guiding us in the choice of a medicine, but at the same time I have the same objections to it that I have to *other general rules*.” This last objection, if maintained, must for ever forbid Therapeutics to become a science.

All the laws of nature are limited, and Antipraxy is no exception, but it governs all drugs, and, therefore, is available as a guide in the use of all of them as remedies. There is truth of other kinds available also, but to discuss this now is unnecessary. Let this “rule of practice” be taken and acted upon. The admission now made demands a full practical trial.

A public acknowledgment has also appeared in the “Medical Press and Circular” for Sept. 7, 1887. The Editor says in his Notes to Correspondents—“Dr. Sharp. We are in accord with your view ‘that the contrary action of certain larger and certain smaller doses of each drug is a law-fact, and as such may be used as a guide in prescribing.’” The practical adoption of this rule would benefit the sick to an extent which is, at present, incredible.

This is the position of *Antipraxy* in the view of the old school. Since it was written I have read Dr. Dudgeon’s criticisms in the paper already noticed in the preceding section. They shall be briefly replied to in this place.

My two critics, representing the old and the new schools differed absolutely respecting organopathy, and each had to be answered. This time they as absolutely agree respecting Antipraxy, for both deny the existence

of the contrary action of certain larger and smaller doses of the same drug. The answer already given to the former may, therefore, stand as the answer to the latter. But it will be necessary to notice, as we did before with respect to organopathy, the *admissions* made by Dr. Dudgeon with respect to Antipraxy :—

“Dr. Sharp blames Hahnemann for not having indicated the doses of the drug that caused the different symptoms in his pathogeneses, and *I admit that it would have been more satisfactory had he done so.*” My readers know, not that I have blamed, but how often I have expressed my regret that this was not done, because it makes a re-proving of every drug a necessity ; the *kind of action*, whether in health or in sickness, being dependent, not upon the drug, but upon the dose.

Again—“Hahnemann, in his *Materia Medica*, gives us no means of discriminating which are the symptoms produced by small, which by large doses.” “Then our manuals of homœopathic *Materia Medica*, and our repertories mostly throw no light on the dosage of the provings.” Dr. Dudgeon is familiar with this question, and his testimony settles it beyond dispute, (notwithstanding the side taken by the Editors of the ‘*Monthly Homœopathic Review*’), that the different kinds of action of different doses of the same drug were entirely unnoticed by Hahnemann, and that they are equally unknown to the compilers of the manuals and repertories for the guidance of the homœopathic practice of the present day.

Of the many criticisms Dr. Dudgeon goes on to make in his Paper, I think only one requires a serious notice from me :—“Others who have repeated his experiments have not obtained the same results.” On reading this I immediately wrote to Dr. Dudgeon, telling him that I knew Dr. Nicholson of Bristol, and his experiment with Aconite, which was not tried under the same conditions as mine, but with the sphygmograph ; that I knew of no other experiment ; and requesting him to tell me who had repeated mine, and where the account of them might be found. He replied :—

“You ask me to point out to you any repetition of your experiments with small doses. You will find some experiments made under my direction with small doses of Digitalis in the ‘*British Journal of Homœopathy*,’ xxxix, p. 279, *which are purely negative.* I have the sphygmograms

of these experiments before me and I cannot detect the slightest difference in the character of the tracings at any stage of the experiment . . . I subsequently made some experiments with Aconite *with equally negative results*. I have not published them but have the sphygmograms which show no alteration in the features of the pulse." But "negative results" are no disproof of positive ones; my critic does not refer me to any other experiments besides his own. His statement seemed formidable, but this is all that it amounts to! *The positive results remain uncontradicted*. It is next said:—

"The fact is that a medicine produces its specific effects on the healthy human organism *in both large and small doses*." How did it happen in the test experiments with Digitalis and Aconite, when Antipraxy was put upon its trial, that there were only "negative" effects? Where were the specific effects? Certainly, the actions of Digitalis and Aconite upon the *heart* are the most conspicuous *specific* actions of these drugs.

"As regards *Antipraxy*, which is founded on [rather is the expression of] the 'fact' of the contrary action of large and small doses of medicine, Dr. Sharp nowhere tells us what are large and what are small doses of medicine. What can be more vague than the expressions 'large' and 'small'?"

Dr. Dudgeon was present at the Meeting at Leamington in 1873, when the opposite action of certain larger and smaller doses of each drug was first spoken of as a universal or law-fact. Experiments were recorded which had been made with Aconite, Digitalis, Phosphorus, Spigelia, Opium, Veratrum, Mercury, and Tartar emetic, and *the precise dose used in every experiment was carefully given* (Essay XXII). And in later Essays, as experiments with additional drugs were described, *the actual doses were always given!*

In the remainder of the Paper there are about fourteen paragraphs or sentences, which have been replied to in manuscript, but they do not deserve to occupy the time and attention of my readers. They are manifestations of hostile feelings, not presentments of evidence or argument.

Before quitting the subject of Antipraxy, it will be useful to add a paragraph on the *middle doses* between the larger and the smaller doses. In the Papers published in

the 'Practitioner' nine and ten years ago, the following is one sentence:—"The kind of action of drugs is determined by the dose. This varies in at least three ways. 1. A certain range of doses of each drug, called larger doses, acts upon the organs or parts in one direction. 2. A certain other range of doses, called smaller doses, acts upon the same organs or parts in the opposite direction. 3. Certain intermediate doses, between the large and the small, act in both directions; first in one, then in the contrary manner." ('Practitioner' for May, 1878.) In Essay XXXII, (1877) these middle doses were spoken of as a "connecting link." "It is not an abrupt division; but like many other operations of nature, very gradual. This connecting link includes all doses which have primary and secondary actions." In my reply to Dr. Bristowe, as to a "neutral dose," the same explanation is given. ('Therapeutics,' page 195, 1886). It must be borne in mind that this middle range of doses having double actions, or primary and secondary actions, in respect to some drugs, is a *large one*—so large that the mistake is made of taking it to include *all* the doses of such drugs. The mistake has arisen from the neglect to try experiments with really small doses.

In 1871, Dr. Bayes attempted to prove, in 'Specific Restorative Medicine,' that all drugs are first stimulants and then paralyzers. This was shown to be untenable in Essay XXIX, 1876, ('Explanatory Hypotheses').

Dr. Boardman Reed has a Paper in the 'Practitioner' for April and May, 1888, advocating somewhat similar views. He has found "no exceptions to the induction of Schultz and Peiper, that all paralyzing agents primarily stimulate." This is true of the *intermediate range* of doses of *some* drugs; but it is not true of all drugs, nor of all doses of any drug. It is, therefore, not a successful induction.

A review of *Therapeutics founded upon Organopathy and Antipraxy* in the Bibliothèque Homœopathique of Paris, has in it these words, with which I may conclude:—"les faits sur lesquels repose l'antipraxis sont incontestables."

VI. *Antipraxy without Homœopathy.*

“A man may be a heretic in the *Truth.*”
MILTON.

When this investigation of Hahnemann's Homœopathy was begun in 1850, the subject was in great confusion. Hahnemann's own writings were the main cause of this. There were two questions to be examined—the principle, and the small dose. The principle was taken first. In the introduction to “the British Journal of Homœopathy” edited by Drs. J. J. Drysdale, J. R. Russell, and Francis Black, in 1843, it is said:—“When Hahnemann, by a train of admirable inductive reasoning, had discovered the principle, *Similia similibus curantur*, he only laid the foundation on which a practical system was afterwards to be raised.” At that time it was believed to “differ totally from systems such as those of Brown and Broussais,” or other medical theories, and to be a law of nature. Hahnemann applied this law not only to the action of drugs as medicines, but also to the power which one disease exerts over another; to the influence of mental emotions; and to the action of such agents as light, heat, electricity, and magnetism. It was necessary to study each of these subjects separately. (See Essay VI.) Other medical men were at that time carrying this law of homœopathy into continually expanding limits. Dr. Geddes Scott, a very able writer, said:—“Let it be once thoroughly understood and heartily received . . . and let it be brought with honesty and intelligence into all the regions of morals, politics, and education, and, if I err not, it will appear that the very same ray which guides us in our dealings with the sick, will also guide us in our efforts to instruct the ignorant, to raise the fallen, to emancipate the oppressed, and to regulate the free.”* And this expanding energy was not confined to medical men. A clergyman published a book in which he applied homœopathy to the doctrines of the Christian religion.

To get rid of all these entanglements, and to show in

* *British Journal of Homœopathy*, July, 1856, p. 360.

the first instance that homœopathy is limited to drugs, was no easy task. It was in time accomplished; but there were many grumblings going on in the hearts of the homœopaths while this work was being done. In Essay VI (page 153, 10th Ed.), it is said:—"In the present state of our knowledge, this law is an ultimate fact. We are ignorant of its cause." This was written in 1854. Some years afterwards, (in 1872), the cause was discovered. The next year, (1873), the contrary action of certain larger and smaller doses of each drug was announced, to which the name of *Antipraxy* was given; this, while it contradicted homœopathy, explained the success of homœopathic practice. Originally *similia similibus curantur* was thought to be a law of nature; as investigated, without reference to the *dose*, it appeared to be so to me; since that reference has been made, *contraria contrariis curantur*, in a new sense, has taken its place. The old principle is now changed by homœopaths into *similia similibus curentur*, and is represented to be "a mere rule of practice." They do not seem to be sensible how great the downward step that they have taken has been. But Antipraxy must be rejected.

In Hahnemann's day *pathology* consisted much more of hypotheses than of facts, and it is not surprising that he rejected it, and substituted in its place the dynamic action of diseases and drugs on the vital principle, and the observation of symptoms only.

Since Hahnemann's day our knowledge of Anatomy, Physiology, and Pathology have become more real, and at the time I am referring to, his views appeared to me unsatisfactory. I thought that, at least, the facts of Pathology ought to be taken account of in Therapeutics. This led to a very careful and prolonged study of the *local actions* of diseases and drugs, and this study resulted in *local action* being insisted on as a true induction or generalisation, to which the name of *Organopathy* was given. (Essay XVII, 1867). This declares that all diseases are local; and that all drugs act locally. And the therapeutic rule inferred from it is, to give the drug which acts in the locality of the disease. This also is rejected by the disciples of Hahnemann.

The complaint that Pathology was rejected by Hahne-

mann, and is still rejected by his followers, is controverted by the assertion that pathological conditions are all included among *symptoms*. To this I have often replied by two remarks. First, to do so is to confound the meaning of words; to fail to distinguish between the sign and the thing signified; to confuse the seat and kind of morbid action of diseases with the signs, *i. e.* the symptoms or effects which arise from, or are produced by, these diseases, and which indicate their locality and kind of action. And, secondly, to this day pathology is practically rejected by homœopaths in reference to Therapeutics. Witness, Dr. Dyce Brown:—"Homœopaths are as well versed in pathology and physiology as their brethren of the old school, and cannot value them too highly, in their place, as essential to their knowledge of disease. They examine their patients by every known mode, and thus can say why the patient coughs, and can diagnose the disease present. *But when it comes to the treatment, they refuse to be guided by pathology.*"*

It will not escape notice that these two statements in support of homœopathy do not hold together. Pathology, they say, is included in *symptoms*, and the remedy is to be chosen according to the *symptoms*. And then they say, Pathology, though accepted for diagnosis, is rejected for treatment. There must, therefore, after all, be some difference between Pathology and symptoms.

The reason assigned by Dr. Dudgeon for the rejection of Pathology is thus expressed:—"This old and discredited *organopathy*, which is a revival of the futile speculations respecting the proximate cause of disease, and of the symptoms caused by the action of drugs, which have proved the *ignes fatui* of medical science, and have kept therapeutics at the low level of an empirical or a purely conjectural art." And by Dr. Dyce Brown in these words:—"Homœopaths are as well versed in pathology and physiology as their brethren of the old school But when it comes to treatment, they refuse to be guided by pathology, knowing well that the trust in pathology as a guide to treatment has been the bane of medicine from the earliest days, and has been the cause of the continually shifting practice of the old school."*

The reply is twofold. Pathology is not now made up,

* The 'Times' correspondence on the "*Oidium Medicum*," Jan. 8, 1888. Reprint, p. 38.

as it was in Hahnemann's time, of uncouth hypotheses; but, on the contrary, consists very much of well-ascertained facts. And, secondly, The acknowledged failure of medical treatment based upon pathology, was owing quite as much to hypothetical and erroneous therapeutics as it was to erroneous pathology. The objections to both, which existed formerly and were very real, do not now exist. To urge them still, as if no improvement had taken place, is a profound mistake. True, our knowledge of Pathology is imperfect, but it is foolish to say that we are not to use it in Therapeutics till it is perfect. All our knowledges are imperfect in this age of life.

The next task to be struggled with was the small dose. Hahnemann had involved this question also in great perplexity, and his followers had only made the perplexity greater. This is made very clear in several of the Essays. Finally, the contrary action of certain larger and smaller doses of each drug was recognised, (Essay XXII, 1873), and to this generalisation the name of Antipraxy was given. This law-fact makes known the cause of the so-called homœopathic cures, but at the same time it contradicts Hahnemann's axiom *similia similibus curantur*, and asserts the contrary. This is so plain that one cannot but marvel at its rejection. For example:—

Opium, in the larger doses, acts on the healthy brain, and causes apoplexy.

Opium, in the smaller doses, acts on the unhealthy brain, and cures apoplexy. In these two experiments the drug is the same, and the organ is the same. The changes are in the dose of the drug, and the condition of the organ. If the action of the different doses is the same, the different result must be owing to the changed condition of the organ—then it is “like curing like,” and Homœopathy is true. If the action of the different doses is not the same, but one is contrary to the other,—then it is not like curing like, but contrary curing contrary, and Homœopathy is not true, but Antipraxy is. This is a question which can be answered only by experiment. The larger doses of opium cause apoplexy in a healthy brain. What do the smaller doses cause in a healthy brain? If they act in the same manner as the larger ones, they may not cause apoplexy, but they will cause a weaker condition of the same kind, namely, drowsiness.

When the experiment is tried it is found that they cause wakefulness and excitement—then, the action of the smaller doses is contrary to that of the larger—then, the cure is by contrary not by similar action—then Antipraxy is true and not Homœopathy.

Antipraxy and Homœopathy as doctrines are the opposites of each other. If Antipraxy is a law-fact, then Homœopathy is not. Nothing can be clearer than this to any one who will give the matter the necessary thought. What is Antipraxy? It is the expression of the fact that each drug has an action in certain smaller doses on the living body of man, both in health and in disease, which is the contrary to the action of certain larger doses of the same drug; and that when these smaller doses are given as medicines, they should be given for diseases in which the morbid action, so far as we can understand it, is in the opposite direction to that of these smaller doses. They cure *because of* this contrary action. And what is Homœopathy? It is the doctrine that diseases are cured by medicines which have an action *like* that going on in the disease. This, then, contradicts Antipraxy so directly that it is not possible for both to be true.

That Homœopathy was accepted as true in the early Essays, arose from the subject being so large that it had to be divided, and one part taken at a time. The principle was taken first, without reference to the dose. As the Essays advanced and the part of the dose had been taken in its turn, it has appeared to me to be proved that cures are effected by a *contrary* action and not by a *similar* one.

For we have now seen that to prove that "like cures like" is true, the similar action should be found not only in the *drug*, but also in the *dose*. That other doses of the same drug have actions similar to those in the disease does not make the axiom *similia* true.

This change of view does not shake the faith, which innumerable experiments have given me, in the power of the small doses to cure disease. The facts proved by these experiments, through a long course of daily practice, remain as convincing as it is possible for facts to be. The discovery from experiments in health, that the action of these small doses, so far as it can be observed, is the same as it is in disease, gives an explanation of the cures

previously observed, which serves to settle the conviction not only as true but as eminently rational.

It is quite in vain to reply to this that the action of the small dose may be contrary, but still it is Homœopathy for all that. The very meaning of the word, the axiom *similia*, and its still being often called the homœopathic law, all flatly contradict such an assertion.

A modern writer on Pharmacology acknowledges with regret that the "regular physicians," as they unwisely call themselves, are at a disadvantage, as compared with homœopaths in this respect that they have no scientific principle on which to prescribe drugs for their patients, while the homœopaths have one.* All that the "regulars" have to do, to put themselves on a par with homœopaths, is to accept Antipraxy as their principle, and prescribe small doses for the reason that they will act contrary to the diseases for which they are given. Antipraxy being the opposite of Homœopathy, the only question between the two parties will be this:—Which is the true principle? This is a question which can be answered only by natural phenomena; the only resort in seeking this answer is *experiment*; and as every one is at liberty to try experiments, it ought not to be long during which suspense must be endured, or before a final answer is obtained.

"Why think? Why not try the experiment?" Nothing else can settle the dispute. Nothing else can give the answer. Argument, in such a case, is of no avail. Authority there is none. Hippocrates on the one side, and Hahnemann on the other, are equally without weight. Alas! how much men prefer the *words* of controversy to the *facts* of experiment.

This is the question to be decided between Antipraxy and Hahnemann's Homœopathy. At the same time it is necessary to notice how far the homœopaths of the present day have departed from this homœopathy. We have seen that forty-five years ago the 'British Journal of Homœopathy' contended that Hahnemann arrived at the law he called Homœopathy by *induction*. Now they prefer to speak of "a therapeutic rule" or a "rule of practice," and they have changed the motto to *curentur*. This amounts to relinquishing the guidance of a law of

* *Oidium Medicum*, in *Nature*, Jan. 26, 1888.

nature, and to admitting that they are content to follow, as physicians for thousands of years have followed, a rule derived from experience. I have given up Hahnemann's induction for a truer one, one which is capable of making therapeutics a science. They, by giving up a *law* and contenting themselves with a *rule*, have lost the chance of being scientific; they have forfeited the advantage, which the old school admitted with regret that they possessed, and so have lowered themselves to the old level.

As the latest refuge it is now said:—"Our law of similars does not express the law of cure, but the law of selection." "The selection is homœopathic, the *modus operandi* antipathic." This is not genuine, for it is contradicted by Dr. Dudgeon, who asserts that "no homœopathic practitioner would hesitate to prescribe a medicine whose symptoms corresponded with those of the disease, whether these symptoms were caused in the original prover *by large or by small doses.*" ('Hom. Rev.' Sept., 1887).

Perhaps a few words are necessary with reference to the congratulations offered to the homœopaths in my last paper, (Essay LI), on their having accepted Antipraxy. To that paper, (in the 'Hom. Rev.,' Aug. 1, 1887), the editors append some feeble remarks, among which is this:—"Antipraxy . . . for clinical purposes teaches nothing." From this and their other observations it is evident that I was mistaken in supposing that they understood what they were professing to accept.

The dates given in this Paper show that it is more than twenty years since the local action of the causes of disease, and the local action of drugs, under the name of Organopathy, was presented to the homœopaths—they have steadfastly rejected it.

It is fourteen years since the contrary action of certain larger and smaller doses of each drug, to which the name of Antipraxy was soon after given, was set before the same medical practitioners, and they have also rejected it. We have read Dr. Dudgeon's last utterance, (Sept., 1887):—"On the whole we prefer our old homœopathy, with its plain and intelligible *therapeutic rule, similia similibus curentur.*" It is no longer a law of nature, but a mere rule of practice.

Of course I have no right to raise an objection against the proceedings of two or three hundred Englishmen. They are entitled to follow their own devices in this matter, in Yorkshire phrase to "go their own gait." For myself, I think I may now, with a quiet mind, take my leave of Homœopathy.

To those who are able and willing to take up and continue this investigation I beg permission, with the greatest respect to lay it before them in two aspects, to suit two different classes of minds.

To the scientific let me say, that it will necessitate careful work in two lines of research quite independent of each other. The first line is in pure science, and involves the solution of such questions as these:—

(1). What is the local action of *each* drug in health? Or, what are the organs of the living body which appropriate the drug, and become the *seats* of its injurious action? This is Organopathy as a science.

(2). What are the actions of different *quantities* of each drug in health? And specially, what are the contrary actions of certain larger and smaller quantities of each drug? This is Antipraxy as a science.

(3). Can the subdivision of a drug be successfully carried to the millionth of a grain or drop? Are such quantities material and appreciable? And can they act upon living nerves? My individual experiments answer these questions in the affirmative.

The second line of research is in applied science, that is, in practical medicine, and involves such questions as these:—

(1). Are the actions of each drug the same in disease as in health?

(2). The actions being the same, or tending to be the same, is the adoption of the action of the smaller doses more successful in the treatment of the sick than the action of the larger doses? I cannot doubt that it is.

(3). If so, is it not the imperative duty of the Profession to adopt this method of treatment? This is Organopathy and Antipraxy in Therapeutics.

To the practical man let me, with all the courtesy possible, say:—Retain the old principle of *contraria*, but in a new sense. Do not scorn the small doses, but try them; and watch, for you have great powers of observa-

tion, what follows, without perplexing yourself with the question *propter hoc?* or *post hoc?* Prescribe these small doses for conditions opposite to those for which you have been accustomed to give the larger doses, *e. g.* give opium for constipation instead of diarrhœa, and for coma instead of wakefulness. And if you can persuade yourself to try some experiments upon yourself, you will attain a familiarity with drugs, and an interest in them, of a new and unique kind. This knowledge—if I may speak from personal experience—will cause you much real pleasure, and be of great benefit to your patients.

To all medical men I will say with real humility :— This Antipraxys is a new instrument put into our hands ; we have everything to learn concerning the manner of using it ; we have, therefore, to exert ourselves to acquire the power to handle it aright, in the serious work given us to do, which is to heal the sick in the best possible manner. It may be asked—How are we to carry on our work while we are learning to handle this new instrument? By using all the knowledge we already possess, whether acquired in the old school or in the new, with only this proviso, that we have the moral courage and the conscientiousness to acknowledge from what we have acquired it.

So long as the Medical Profession encounters Homœopathy with nothing better than ridicule and abuse in words, and with large doses in practice, Homœopathy will survive, and be “ a pricking briar and a grieving thorn ” in the sides of the Profession. If medical men will conquer their irritation, and calmly look at the facts of nature, as presented to them by observation and experiment, and as at present summed up in Organopathy and Antipraxys—and it is open to every man to make these observations, and to try these experiments himself—and if they will prescribe the smaller doses instead of the larger, and on the principle *contraria contrariis curantur*, the grieving thorn will be extracted, Homœopathy will pass away, the unity of the Profession will be restored, every man’s patients will be benefited, and God will be honoured.

HORTON HOUSE, RUGBY ;

May 26, 1888.

BY THE SAME AUTHOR.

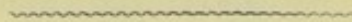


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