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London : George Bell ..., 1886.

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THERAPEUTICS

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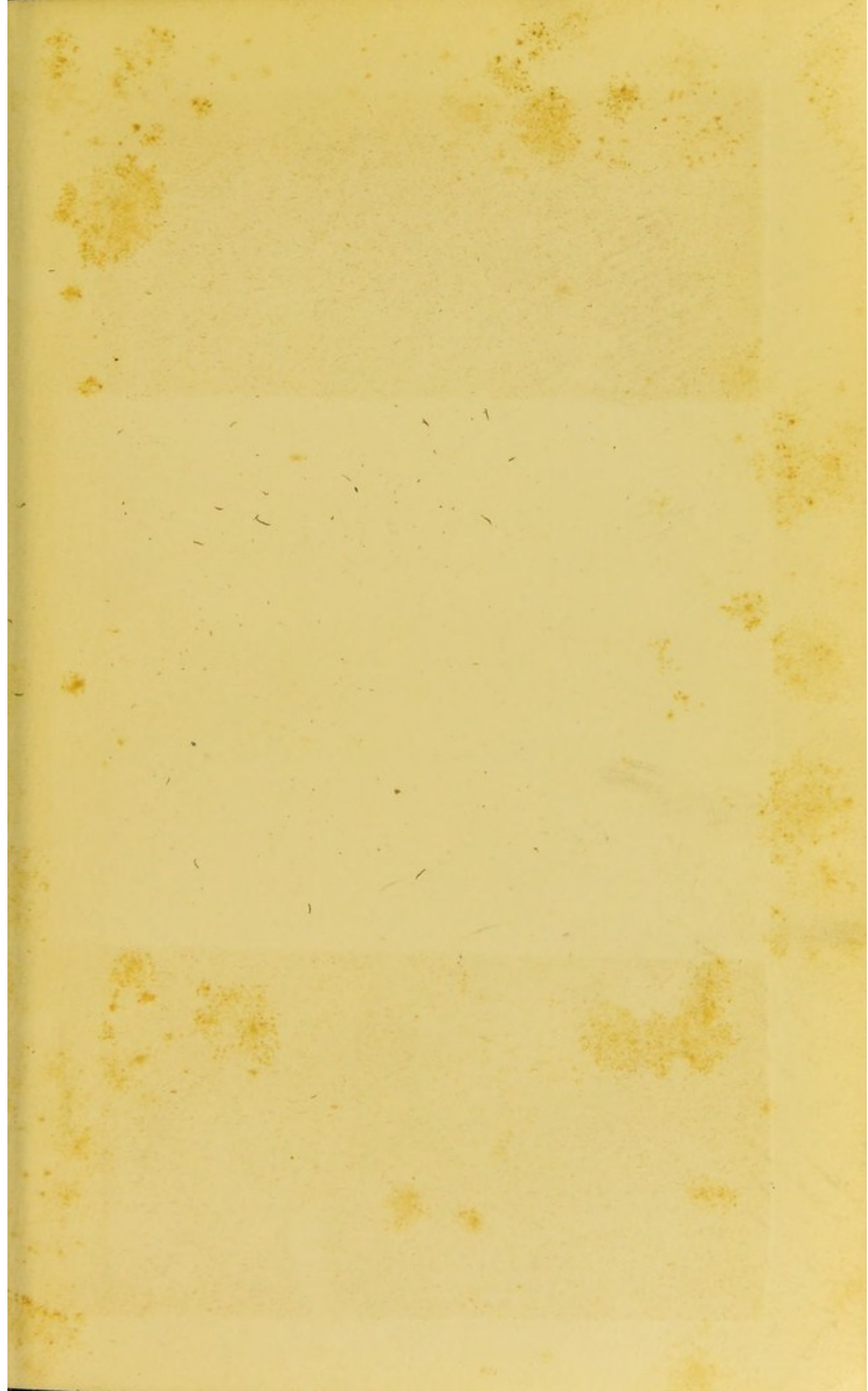
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THERAPEUTICS

FOUNDED UPON

ORGANOPATHY AND ANTIPRAXY.

RUGBY:

PRINTED BY A. J. LAWRENCE,

MARKET PLACE.

THERAPEUTICS

FOUNDED UPON

ORGANOPATHY AND ANTIPRAXY.

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

Licentiate of the Society of Apothecaries (1826); Member of the Royal College of Surgeons (1827); Fellow of the Royal Medical and Chirurgical Society (1840), &c. ; formerly Senior Surgeon of the Bradford Infirmary ; now Vice-President of the Rugby Hospital.

"The Physician should do good to his Patient, or at least he should do no harm."—HIPPOCRATES (*Epidemics, Book I. Sect. 2*).

LONDON:
GEORGE BELL AND SONS, YORK STREET, COVENT GARDEN.
RUGBY: A. J. LAWRENCE, MARKET PLACE.

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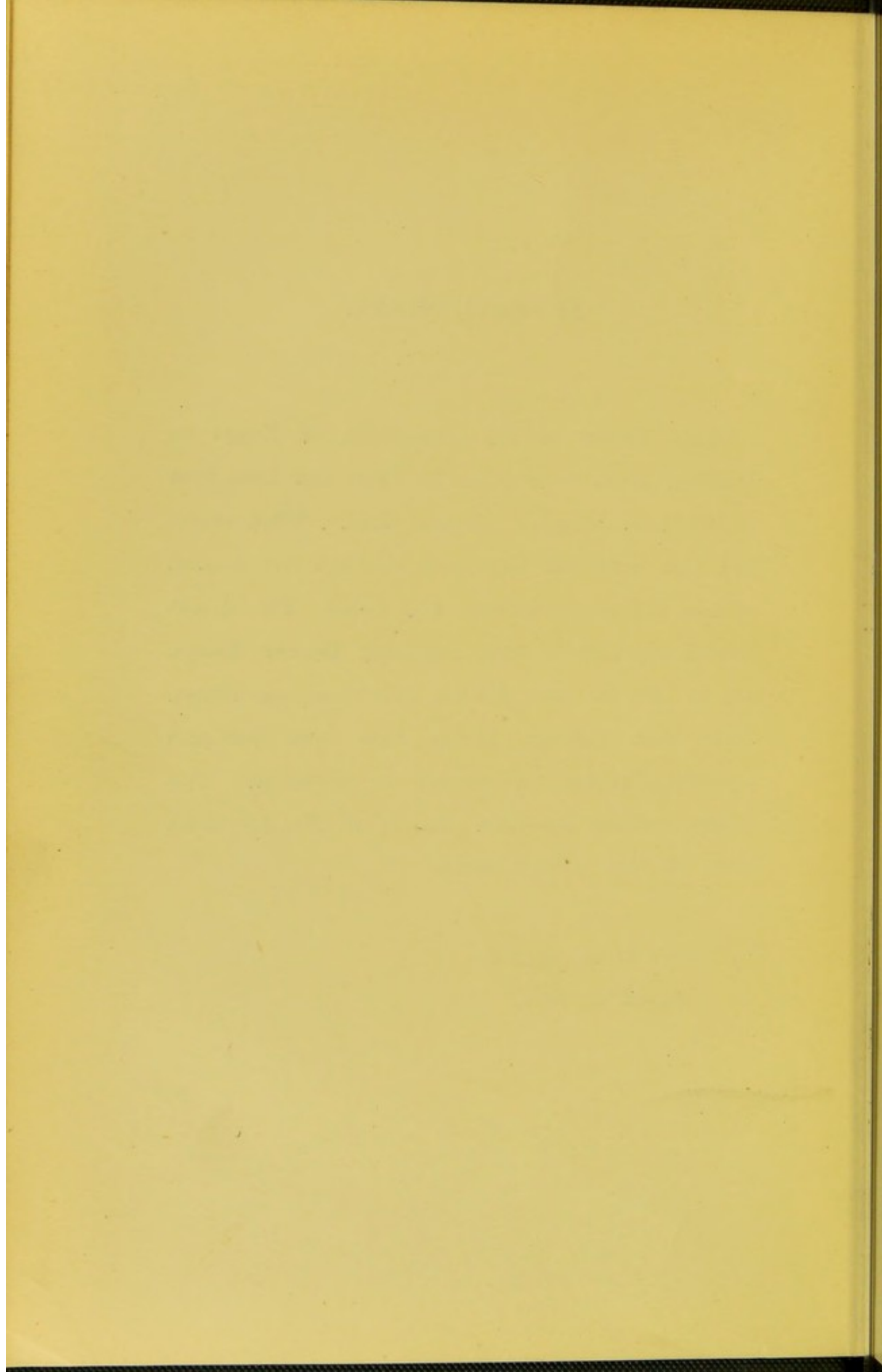
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ADVERTISEMENT.

THIS Volume is the continuation of Essays on Medicine which were begun in 1851, and have been continued at irregular intervals since. When twelve had been published they were collected into a small volume, and several Editions were issued. The seventh Edition appeared in 1856, containing thirteen Essays, and in 1874 the tenth Edition with twenty-six Essays. Since then eighteen Essays have been published separately, raising the number to forty-four. This volume contains six—from No. 45 to No. 50—which have not been printed before.

Horton House, Rugby,

August 21, 1886.



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INTRODUCTION.

THERAPEUTICS—the Art of Healing—has been based upon many foundations; all of them have failed to give satisfaction; the reason of this is that all these foundations have been theories, and as theories cannot be proved to be true, none can gain general assent. The Medical Profession has been divided into sects from the beginning, and is so still, as much as in former ages; the reason is the same, each sect has a theory. The consequence of this is that quackery of all kinds is the superior power, and that medical men do not occupy the position to which they are entitled by their calling. This has often been regretfully acknowledged. The last Harveian Oration (1885) by Dr. Quain laments it strongly.

Nothing can remedy these misfortunes but the adoption of a practice founded, not upon theory, but upon facts—facts which everyone may verify for himself. To desire this has, so far, seemed Utopian and impossible, because practice has always been founded upon theory.

All leading books on Medicine are said to be on "The Theory and Practice of Physic." Every theory fails, and is supplanted by another.

But there is another path—the path of searching for *facts*—first for individual facts, and then, by *induction* from these, for what have been called general, or universal, or, as I shall now call them, *law-facts*. They are often termed laws of nature. In pursuing this path in the field of Therapeutics, I have been so happy (as I believe) as to discover two of these law-facts—the local action of all drugs, which I have called *Organopathy*, and the contrary action of certain larger and smaller doses of each drug, to which I have given the name of *Antipraxy*.

Upon these two pillars a platform may be raised, on which men of all sides and diversities of opinion may meet and shake hands with each other. A union thus effected would bless the sick, the profession, and the world. Duty, patriotism, and self-interest would be a three-fold cord binding men together. In this way the true position of respect and confidence for the Medical Profession would be attained. The main support of quackery would melt away from underneath it, and that which, hitherto, has been triumphant would have to

occupy a much humbler place. Here is a golden opportunity placed before every medical practitioner. In saying this I am not afraid of being called presumptuous. I am confident that it would be a step in advance in the right direction, and if taken, may lead to other steps of further improvement; there can be no finality in knowledge.

It will be understood, therefore, that the result of the investigations carried on in these Essays, is the establishment (as I think) of Organopathy and Antipraxy, not as theories but as laws of nature or law-facts. They are commended to the Medical Profession for a practical trial, and if they should by this trial be confirmed and adopted, so that in future, practice may be ruled by them, they will do for Medicine what Sir Isaac Newton's law of gravitation has done for Astronomy, what Richter's and Dalton's law of combination in fixed proportions has done for Chemistry, and what William Smith's law of the superposition of strata has done for Geology.

With respect to Organopathy—the local action of drugs—this was so fully considered in the earlier Essays, especially in Essay XVII, (1867) it may be sufficient to say that in these later ones it is taken as established.

With respect to Antipraxy—the opposite action of

certain larger and certain smaller doses of the same drug—this was first announced in Essay XXII, (1873) and the following ones. It is further explained in these later ones. It may not be amiss to remind my readers here that this contrary action of different doses may be made use of in practice in two ways:—the *larger* doses may be prescribed to act contrary to diseases opposite to those produced by these larger doses when they are taken in health; as when Opium is given for sleeplessness, or for diarrhoea. This has been the common practice up to the present time. Or, the *smaller* doses may be given for the opposite condition caused by them in health; as when Opium is given for apoplexy, or for constipation. This last is the method which is very earnestly recommended to my professional brethren, as being greatly superior to all former methods.

What are the larger and smaller doses of a drug? There is a vagueness in these expressions which requires removal by everyone who uses them. In a book now in course of publication in America, called a *Handbook of Homœopathic Materia Medica*, by Dr. T. F. Allen, the action of Aconite is thus described:—"Aconite in small doses accelerates the heart's action and causes a rise of temperature," &c. In these Essays the contrary is

asserted, and Aconite in small doses is said to slow the heart's action. This contradiction arises from the "small doses" meant by the one not being the "small doses" given by the other. To obviate this uncertainty it will be found that the *actual doses* prescribed in the cases which I propose to give in an Appendix when these Essays are finished, are always very carefully written. To make Dr. Allen's remark of value the doses he is thinking of should be mentioned: this is a condition which applies to all experiments with drugs.

If fault should be found with the repetitions met with in this series of Essays, my reply by anticipation is, that the repetitions are intentional—of set purpose. The Essays have been written at intervals during the last thirty-five years, and they have been meant to be "line upon line." In no other way can a man hope to make himself heard. Repetitions are not always more than enough.

Facts against Theories have been the guide followed throughout this investigation of Medical Systems.



ESSAY XLV.

THE DEFECTS OF ORTHODOX MEDICINE.

Medical "sciences are delivered (in books and lectures) to be believed and accepted, not to be examined and further discovered; the succession is between master and disciple, not between inventor and continuer or advancer; and therefore, these sciences stand at a stay, and have done for many ages."

LORD BACON.

ANALYSIS.

INTRODUCTION.

- I. ORTHODOX MEDICINE IS GUIDED BY FALSE THEORIES.
- II. IT IS GIVEN UP TO IMPERFECT AND HASTY GENERALISATIONS.
- III. TRUE GENERALISATIONS ARE NOT SOUGHT.
- IV. ITS PRACTICE IS UNSETTLED.
- V. PRACTITIONERS ARE IN BONDAGE TO AUTHORITY.
- VI. THERE IS LITTLE PROGRESS.
- VII. WRONG PATHS ARE BEING PURSUED.

ESSAY XLV.*

THE DEFECTS OF ORTHODOX MEDICINE.

"The sick man dies of his Doctor."

SYDENHAM.

ARISTOTLE says, "Every science has its principles." These words, written more than two thousand years ago, are not only true still, but they are true in a much wider sense than that in which Aristotle understood them. When he said "every science has its principles," he meant the remark to apply to the mathematical sciences only. He says, "Science is a mode of judging which deals with universal and *necessary* truths." "A scientific truth is one that can be demonstrated." "Science implies demonstration; but things whose principles or causes are *variable*, do not admit of demonstration; for everything that depends upon these principles or causes is also variable."†

Aristotle looked upon the phenomena of nature as variable, and consequently incapable of demonstration. In his mind the faculty of demonstration was limited to things invariable and necessary, such as the properties of magnitudes and numbers. We are now able to see

* Written in 1882.

† *Aristotle's Ethics.* Book VI. 6. 1.

that natural phenomena are only *apparently* variable and irregular, and that they are as much under the government of fixed laws as are magnitudes and numbers, with this difference, namely, that the Divine Will absolutely controls them all, and the human will partially interferes with some of them. Aristotle's difficulty is removed, for these "variable" facts and events admit of being arranged under their governing laws. As these laws, one by one, are discovered, though they have not been demonstrated after the manner of mathematics, they become to us "principles," sufficiently proved to be true for all practical purposes. They are not *necessary* truths, but, within their own limits, they are universal, and they are found in practice to be fixed and stable, and to deserve the name of Science. To see proofs of this we have only to acquaint ourselves with the "principles" or laws of Chemistry, or some other branch of science, which was not known to Aristotle.

But even in this extended sense of the word, the Art of Healing is not yet a Science. It is not yet founded upon principles or laws proved by observation and experiment to be true. The *Principia Medica* is an unwritten book. It is beyond question that Medicine, in the meaning of Aristotle, is still a "variable" thing. It is without principles, and, therefore, it is without Science. This is lamentable.

The unconsciousness of this absence of true science, which pervades the minds of the bulk of the Medical Profession, is so surprising that it would be ludicrous if it were not serious. It is true that the words "science" and "scientific" are constantly uttered by writers in our

Medical Journals, but with a few noble exceptions, without perception of their true meaning. Nay, there is not only this unconsciousness of the absence of science; there is an actual want of appreciation of its value; so that, if there has been a discovery of laws or principles in Therapeutics, the announcement of them, instead of being hailed with pleasure, is treated with indifference if not with scorn.

Another initial difficulty is the absence of distinct meaning from many words, which are in common use, besides the word "science." So long as men's ideas are cloudy, their words must be cloudy also; they cannot but be wanting in clear illumination. And when this indefiniteness belongs to such words as "principles," "general laws," "inductions," "deductions," and other words of similar importance, it is impossible to argue usefully or successfully with such persons; and while this indistinctness of vision lasts, it is equally impossible for them to accept the statements of true science, however clearly they may be set before them.

The human mind has two methods of reasoning in science, by which it arrives at conclusions. By one it commences with universal facts, or what are believed to be such, and *descends* to a conclusion respecting a particular or individual fact. By the other it commences with particular facts, and by collecting all that are similar, it *ascends* to universal facts. The two processes are the opposites of each other. The first is called *deduction*; the second, *induction*.

The first, or deductive process, was that of Aristotle,

and of the succeeding ages; and for about eighteen hundred years reigned supreme.

The second was first fully set before men by Lord Bacon in 1620; so that, though individuals had worked with it before Bacon's time, it has not been generally known for much more than two hundred years.

The long previous reign of deductive reasoning has had this consequence—its method has acquired an intensely strong hold upon the human mind; so that even now, it is much better understood, and much more frequently had recourse to than the inductive. Indeed, it is surprising to find that powerful minds do not always clearly distinguish one method from the other. So lately as in Dr. Abercrombie's work on the "Intellectual Powers," which has had a high reputation, the words "deductive" and "inductive" are sometimes used indifferently as if they were synonymous. Yet they are methods of reasoning in directions opposite to each other, and it is by *induction* alone that any solid foundation for Therapeutics can be substituted for the quicksands of ever-fluctuating opinions and practice, which have till now prevailed among physicians.

It will be noticed that both these processes of reasoning—the deductive and the inductive—ought to have only *facts* to deal with. Whether the materials are mathematical or phenomenal, they ought to be *real*. There is no room for the admission of speculations. It has often been asserted in these Essays that the only use of hypothesis is to suggest further observation and experiment. Whenever it is dealt with as a mode of explanation it is the greatest known obstructive to the

progress of Science. There is nothing more surprising in medical literature than the mistaken value set upon hypothesis. Some clever men even think that the human mind *cannot* restrain itself from speculating about the causes of a phenomenon and how they bring it about; and there are other ingenious persons who cannot see the difference between a fact and a theory, between an induction and a speculation.

In this series of Essays it has been shewn that pains have been taken to find, by the inductive process, some of the principles or laws of Therapeutics. It is believed that progress has been made, and what has been accomplished may at least be used as stepping-stones, by treading on which others may advance further. It is quite true that all that has been done has been ridiculed or despised. The allopathic medical journals have exclaimed, "It is all noise and smoke." The homœopathist has echoed, "It is hair-splitting! hair-splitting!" Nevertheless, if truth prevails, will it not be the medicine of the future? Among the letters written to me on the appearance last year (1881) of the forty-fourth Essay, one was from a distinguished man who, for twenty-four years has not ceased to take a very true interest in this investigation, and with whom I have had many lively discussions. Until now he has always taken the opposite side, and if he had any convictions they were concealed by affectionate banter. In this letter he expresses himself in these remarkable and emphatic words:—"I read your forty-fourth Essay with very great interest. *I entirely*

believe that you there mark out the true line of study for making sound progress in Therapeutics."

It will strike many that this is an odd thing to say. They think that "the regular steady practice according to rule," which, so far as they know, has been going on from time immemorial, cannot be in such a crude and disorganized condition as to need to be started afresh on a new course. It would be difficult to convince such persons how little they know of medical practice. Others, who know rather more, have a suspicion that every thing in Therapeutics is not quite as it should be. Others again, whose knowledge is greater, are uneasy and dissatisfied; express their dissatisfaction; and eagerly look out for something more promising. A few know much more and are silent, but their silence is sad.

Some of my readers will muse pensively and sigh regretfully, as they take "their solitary way" through the desolation to which they are now sorrowfully led.

The principal Defects of Orthodox Medicine are:—

I. IT IS GUIDED BY FALSE THEORIES.

Speculation, hypothesis, theory — these words have their distinctive meanings, but they have also a meaning in common, and it is with this common meaning that we are at present concerned. It is the mind's thought about things previous to observation and experiment. In this sense speculation, hypothesis, theory, have a use, and have an abuse. Both the use and the abuse are common. The use is very good; the abuse is very bad. The use is to *suggest* observation or experiment; the abuse is to

explain what observation and experiment have left in the dark.

Let me give first an example of the use of hypothesis, and if I may be so bold, it shall be one which has happened to myself. While I was a pupil at Guy's Hospital, Sir Astley Cooper's lectures on dislocations came in due course, and among these the dislocations of the *thumb*. He told us that "these accidents are very difficult to reduce on account of the numerous strong muscles which are inserted into this part." He showed us the methods of making extension, very violent ones, which are described in his grand book on Dislocations, and admitted that even these sometimes fail. After the lecture I went to him and said, "Sir Astley, don't you think that if the thumb were *bent backwards*, the dislocated bone would slip into its place?" He smiled, and went away. Fifty years after this I had the first opportunity of putting this hypothesis to the test of experiment. I was asked to visit a young gentleman, a stout boy of seventeen, who had suffered this accident, I think, at football. The first phalanx was thrown back upon the metacarpal bone. Without loss of time I bent back the thumb with some force, and the bone *did* slip into its proper place. My hypothesis had vanished. It had been converted into a fact. After some further time a second case fell to me, with a like happy result.

On the other hand, the abuse of speculation in the invention of explanatory theories and hypotheses is, Alas! very common and very hurtful. In all ages it has been a great hindrance, I think the greatest, to the progress of medical knowledge. It may be useful to give two or

three examples. The first is so old that it was discussed in the days of Hippocrates, four centuries before Christ. In the book "On Ancient Medicine," allowed by M. Littre and Dr. Francis Adams to be genuine, "the method of those who prosecute their enquiries in the Art by hypotheses" is strongly condemned, and the instance cited in illustration is this:—"If hot or cold, or moist or dry, be that which proves injurious to man, and if the person who would treat him properly must apply cold to the hot, hot to the cold, moist to the dry, and dry to the moist," that is a theory; and Hippocrates enters upon the refutation of this theory with great vigour, and enforces arguments which ought to have been sufficient to put such a speculation out of men's minds for ever. He did not succeed, and afterwards, by the help of Galen, the theory reigned despotically over physicians till the sixteenth century after Christ. It is to be hoped that we have at length awoke from the oppression of this nightmare.

The profession had scarcely escaped from the bondage imposed upon it by Galen when it fell under the tyranny of other speculations. Among these a very prominent one has been the theory of sthenos, phlogosis, inflammation. The greatest name among physicians, in the middle of the last century, was Boerhaave, and his greatest disciple was Van Swieten; the latter says in his Commentary on the 30th Aphorism of the former:—"Almost the whole cure of acute diseases consists in bringing on a disposition towards a *dropsy*; that is, reducing the patient to a greater degree of weakness." And again, on Aphorism 105, when speaking of Small-Pox:—"All that Art can do is to *weaken life*, life

being that which makes poisons active." And so the unhappy patients were bled, and blistered, and purged, and starved to death for a century. But "it is the weakness of human nature, in every instance of folly, to run from one extreme to its opposite," so the next hypothesis was the contrary one, that all disease is debility; and from bleeding and barley-water the transition was made to beef-tea and brandy. Dr. Graves of Dublin wished it to be recorded of him, that he fed fevers. Dr. Todd of London carried the feeding and stimulating treatment further, until it has come to pass that this is practised almost without limit; and thus, on the hypothesis of debility suggesting as orthodox "a little and often," beef-tea is to be given every hour, with wine or brandy between times, and now death is hastened by food and stimulants. But coming events cast their shadow before them, and there are signs appearing of another swing of the pendulum; let me say, therefore, by anticipation, that the opposite suggestion of "much and seldom," or even of "little and seldom," will not be true, nor a safe guide to follow. Death will sometimes be hastened by a blind following of either method, however orthodox it may be at the time. Starving and over-feeding are the Charybdis and Scylla of food; and the unfortunate sick, after having been for a long time ingulfed in the one, are now carried across to be wrecked upon the other.

But it may be asked, What about medicines? It is replied that during the reign of the sthenic theory—the starving time—physicians had the highest confidence in medicines, and they "exhibited" them (that was the

phrase) in heroical quantities, and in what seemed to them most skilful combinations ; these were taught with great precision by Dr. Paris, a President of the Royal College of Physicians, in his *Pharmacologia*. Not unfrequently, however, physicians treated their patients, as unskilful sportsmen try to kill game, as one of them told Sir Astley Cooper,—“ I put into my gun plenty of shot, in the hope that they will scatter so much that some will hit. I aim at all the emunctuaries at once ! ”

And what about medicines during the reign of the debility theory—the feeding time ? The answer to this question shall be given by the *Practitioner*, a Journal justly looked upon as one of the best and most practical of the medical periodicals of the present day :—“ The system of our leading hospital physicians and surgeons . . . is physiological medicine, characterised by a strong belief in the sanative powers—the *vis medicatrix*—of nature, and very great scepticism as regards the utility of drugs : *it takes care of, supports, and amuses the patient*, while nature cures his disease ; [or until death comes] . . . it admits the existence of a few specifics, such as quinine for ague, [for which the profession is indebted to a Spanish Countess, not to physiology], which are given quite empirically, without any notion or theory of their *modus operandi*, of how or why they produce their effects.” This is said in regard to the choice of the medicine ; then, in regard to the choice of the dose it is said :—“ The truth is, there is no principle, or law, or mathematical certainty about the matter. *We* [the italics are the writer’s] frankly admit that we give drugs quite empirically ; we give $a, \frac{a}{10}$, etc., because experience

has taught us that such and such a drug, in such and such doses, produces certain effects; how or why we do not know, or pretend to know; nor do we profess to know of many drugs, what is the precise dose which will produce the desired effect;—from three to six grains, or from one to three, are exact enough for us!”*

Such is the desolation—the formless void—of the Therapeutics of the “physiological” school of this nineteenth century! And the writer of the article is enterprising enough to call this a “system based upon *scientific* knowledge, experience, and sound logic!” Can it be any breach of charity to enquire, What *science* is there in this system? Not the smallest. The writer himself repeatedly boasts of it as *empiricism*, which, if the word means anything, means the absence of science; and this, he tells us, is the knowledge of Brodie and of Bright; of Watson and of Jenner; of Paget, of Gull, and of Wilks. Alas! that this should be true, and that it should be the boasted but empty result of all the Physiology of this century. The world is instructed that such is the theory of “Physiological Medicine.” And such, we are told, is the practice of to-day—“We take care of, support, and *amuse* our patients, while nature cures their diseases”—or lets them die! “this is all.” Here, then, we have the *ne plus ultra* of the cure-by-nature theory.

These examples ought to be sufficient to prove the truth of the heading of this Section—that Orthodox Medicine has been, and still is, “guided by false theories.” They should, moreover, produce the conviction in the

* *A Paper in the Practitioner* for September, 1881.

minds of thoughtful readers, that hypothetical explanations either of the nature of diseases, or of the uses of drugs, not only do not help, but inevitably mislead and hinder us, in our search after a true method of healing.

Can it be denied, can it be questioned, can it be doubted, that the facts here reviewed are an overwhelming defect in Orthodox Medicine?

II. IT IS GIVEN UP TO HASTY GENERALISATIONS.

Professor Jevons has said that hasty generalisation is the greatest foe to the advancement of science. This remark does not apply to Medicine; because, in this pursuit speculative explanation occupies this unenviable position; and also because Medicine, as has already been explained, is not yet a science at all. Generalisations, as at present understood, have seldom been attempted by physicians.

A generalisation, that is, a universal, collective, or general fact, or as I now propose to call it, a law-fact, is an induction from a sufficient number of particular or individual facts. Such generalisations scarcely exist in Therapeutics. Their place has been filled up by inferences or deductions from theoretical axioms. These deductions have commonly taken the form of rules of practice; for instance, Galen's rules—give a hot remedy for a cold disease, &c.—have this character. So also in later times, the antiphlogistic treatment of the last, and early part of this, century, as well as the stimulating practice of the present day, are hasty, imperfect, and false generalisations; they are simply inferences from their respective theories.

The theories being false, the inferences from them cannot but be false and misleading rules.

When generalisations have been attempted by physicians, as the result of observation and experiment, they have been so hasty as to be mischievous. There is one which dates from the "Aphorisms" of Hippocrates, which has been superlatively mischievous; for under the shadow of his great reputation it has perpetuated a frightful cruelty in the Art of Healing almost to our times—through more than two thousand years. The Aphorism is as follows:—

"Those diseases which medicines do not cure, iron (the knife) cures; those which iron cannot cure, fire cures; and those which fire cannot cure, are to be reckoned wholly incurable."*

"It has been the practice in all ages," writes an author in 1750, "from the time of Hippocrates down to the beginning of this century, to cauterize the eschar. The memorable Aphorism he left behind him, relating to the efficacy of fire, brought the cautery into use upon almost every occasion. In mortifications they believed that the putrifying principle or venom was extracted with the juices that were dried up by the hot iron. They thought likewise, that the separation of the sloughs was exceedingly assisted by this process; and what was more important, they imagined that the life of the part was quickened by drawing the spirits to it and freeing it of all humidities.

* *Works of Hippocrates*, by Francis Adams. Vol. II. p. 774, Aph. vii. 87.

“I have here used the very language of all writers upon this subject, and we have hardly in Surgery a more extraordinary instance of human fallibility than this; for after an uninterrupted practice of above two thousand years, this celebrated remedy, whose virtues were supposed to be evident from both reason and experience, is at length fallen into disrepute, and never employed for stopping a gangrene. It has also met with the same fate in regard to many other distempers, for which it was formerly esteemed a kind of specific; but it has lost its ground very gradually. When it was abolished from among the remedies for a gangrene, it was still reserved for cancerous tumors and excrescences, from a persuasion that it would kill any lurking venom near the extirpated cancers. And now that it is no longer used for this disorder, it continues to be practised upon carious bones, in order to promote exfoliation; but I think upon no better grounds than in the other cases, so that in all probability it will by and by be universally discarded even from the exfoliation of bones. In England this is already done; but for the final removal of these prejudices we must allow more time.”* It is to be hoped that the medical mind will never become so insane as to practice afresh the cruelties of the “actual cautery.”

“Fire” then is discarded; but before fire was had recourse to, “iron” was to be tried. What is the present state of practice as to the use of this? Hippocrates was careful to say, “*When medicines fail*” the knife may be used. The hasty generalisations of our times are more

* *A Critical Enquiry into the Present State of Surgery*, by Samuel Sharp, F.R.S., Surgeon to Guy’s Hospital. 1750. page 253.

rash. Of how many diseases is it now asserted, without apparent regret, that medicines have failed, and, therefore, the time for cutting is come! The latest paragraph I have read on this point is the following:—"In the treatment of epithelioma of the tongue, all surgeons of experience are agreed as to the inutility of medication, whether topical or general, and the advisability of early and complete removal of the disease."* In other words, the removal of the half or the whole of the tongue. This is the usual mode of expression among surgeons; they say, "We are agreed as to the inutility of medication." And physicians often adopt similar language; they say, "There is no remedy." How much better would it be for their patients if medical men would exert themselves, as they might and ought to do, to *find out* remedies for these diseases which they so readily pronounce to be incurable! Fire is a hideous instrument in the hands of a "Healer." The knife is not much less so.

In every branch of Medicine imperfect generalisations are to be met with. It would be tedious to enter into the details, which thoughtful men will have no difficulty in discovering; and I will limit myself to two examples, one taken from Nosology, and one from Materia Medica. In Nosology, the classification of diseases is shown to be imperfect by two facts; one, the successive changes which it has always been, and still is, undergoing even in important particulars; and the other, the conviction in the minds of medical men of this imperfection manifested by the caution so often given by them to their juniors, never

* Christopher Heath, Professor of Clinical Surgery, University College, London. *British Medical Journal*, June 18, 1881.

to prescribe for a patient from the mere *name* of his complaints, as this would, not unfrequently, turn out to be a serious mistake. In *Materia Medica*, the classification of drugs is not only imperfect, but so imperfect as to be false. For instance, the drugs classed as "purgatives" may with equal justice be classed as "astringents;" and so of the other classes; each drug having actions in opposite directions in larger and smaller doses.

Here is sufficient evidence that Orthodox Medicine has been given up to imperfect or hasty generalisations; or rather to inferences from hypotheses, or at the best from partial observations, which inferences are not worthy to be called generalisations.

III. TRUE GENERALISATIONS ARE NOT SOUGHT.

Nothing has surprised me more, in my intercourse with medical men of all ranks, than their unconsciousness that scientific generalisations in medicine are needed. There is an absence of desire for such knowledge; and in many even an apparent incapacity for an intelligent understanding of the meaning of the phrase.

The common expression is, "as a rule" it is so and so. This means that there are exceptions to the rule, so that it has no pretention to science. As has already been pointed out, such rules are nothing more than inferences from theory. This assertion must be supported by proof. "In reference to the internal treatment of the disease (Erysipelas) we must recollect that mild cases get well spontaneously, and that more serious cases very soon

present symptoms indicative of great debility and of blood-poisoning." This is the theory, the inference follows;—"For these reasons it seems obvious that depletion can never be necessary; but that, *as a rule*, the strength of the patient should be sustained. . . . To support strength such nourishment as he can take should be administered frequently and in small quantities [little and often]; milk, eggs, beef-tea, arrow-root, sago, and the like, are most suitable for the purpose; to which, if the pulse be failing and the tongue dry, brandy, wine, or ale (if the patient prefer it), should be added."*

Feeding and stimulating are here, not *inductions* from observation and experiment, but *deductions* from hypothesis. It is imagined that the symptoms are "indicative of debility and blood-poisoning," and the inference is that "depletion can never be necessary." On the same hypothesis, Dr. Fordyce gave Peruvian bark in substance "in as great a quantity as the patient's stomach will bear, which is commonly to the quantity of *a dram every hour*." In Sir Astley Cooper's time this was changed for the Sulphate of Quinine. So that my friend Dr. Bristowe is in very good company. But Sir William Lawrence, no mean observer, following Sydenham, writes thus:—"I am quite at a loss to discover in this affection those marks of debility which some have so much insisted on. . . . As this affection resembles other inflammations in its causes, symptoms, and effects, so [for these reasons] it must be treated on the same principle; that on the antiphlogistic plan. Venesection, local bleeding, purging,

* *A Treatise on the Theory and Practice of Medicine*, by J. S. Bristowe, M.D., F.R.S., p. 302, 2nd Ed., 1878.

and *low diet*, are the first measures, to which saline and diaphoretic medicines may afterwards be added. The earlier these means are employed the better; vigorous treatment in the beginning will often cut the attack short, and prevent the disease from spreading beyond its original seat.* And my friend of olden time is also in good company.

Such is, and ever will be, the caprice of medical practice, so long as the unstable basis of theory is built upon, and until all such speculations are finally abandoned. Not till law-facts are discovered by observation and experiment will there be any solid foundation for practice, or any general agreement among practitioners. The Profession seems to be in the dark as to this first step in science! The practice of each generation is a blind following of a few successful competitors for the position of leaders, and these are followers of their own hypotheses! And so the wheel goes round.

The necessity for true generalisations, as the foundation of the only practice which can be wise and permanent, springs from the fact expressed in Lord Bacon's first Aphorism:—"Man, as the minister and interpreter of nature, understands and does as much as his *observations on the order of nature*, either in regard to things or the mind, permit him, and neither knows nor is capable of more—*non amplius scit aut potest*."

We cannot cure disease by tearing the body to pieces by "active treatment," nor by oppressing it with over-feeding. We cannot cure disease by "lowering life" on the one hand, nor by "stimulating" it on the other.

* *Medico-Chirurgical Transactions*. Vol. XIV., pp. 28 and 39.

All such methods, however conformable to a theory of disease, or however patronised by great names, can only open the way more widely for the approaches of death. They help disease to kill.

It is only as we may be permitted to discover the law-facts—the true generalisations belonging to the living body and to drugs—the laws of life, and the laws of the action of drugs—that we can hope to discover the laws of Therapeutics. And it is only by obeying these laws that we can hope to help the natural processes of an organ, which have become disordered, to return to their healthy condition.

IV. THE PRACTICE IS UNSETTLED.

This is a necessary consequence of the conditions briefly referred to in the two preceding paragraphs. In the Profession it is a fact so notorious that almost every medical man speaks of it and gives illustrations of it; and this not with much pain, but as a matter of course—an unavoidable state of things. This unsettledness is so great and thorough, that, were not the public so wonderfully unacquainted with professional affairs, their confidence in doctors and physic would have evaporated long ago. For my part, I wish the public would rise to the occasion, and would acquaint themselves with the broken reed on which they lean in the hour of their bodily distress. This might open the eyes of the Profession.

But the lay people will be at a loss how to set about

such an enquiry. I should like to suggest to them to begin by modestly asking their medical advisers to explain how it is that, while there is always a panacea or universal remedy in fashion, the remedy itself is always changing, so that it might have served Sir Isaac Newton to suggest to him the discovery of fluxions. In the course of one lifetime the rise and the fall of these favourites have been like those of Prime Ministers. I very well remember when the idol was *Rhubarb*; then gentlemen carried a piece of the root in their waistcoat pocket, and at all convenient opportunities took a bite of it. At that time *Rhubarb* was the panacea, as it had been, fifty years before, after Dr. Cadogan's pamphlet, a favourite remedy for gout. After a few years, the waistcoat pocket continuing still in demand, *Rhubarb* had disappeared from it, and now its contents were *White Mustard Seed*; then it was common to see two gentlemen talking together in the street, one taking a pinch of snuff, the other, from the before-mentioned waistcoat pocket, a pinch of *White Mustard Seed*. In due time this was followed by Mr. Abernethy's *Blue-pill*; when each patient was referred to a particular page of his book, for directions how to take it. This fashion struggled stoutly for a long reign; but it had to yield and retire, and be succeeded by the *Syrup of Sarsaparilla* (by which Mr. Hudson, of the Haymarket, must have made a fortune). This, again, was supplanted by *Cod-liver Oil*, which, abominably disagreeable as it is, has not yet been quite expelled in disgrace. Besides these we have had *Chlorate of Potash*, then *Iodide of Potassium* and *Bromide of Potassium* in luxuriant quantities; and now *Chloral*

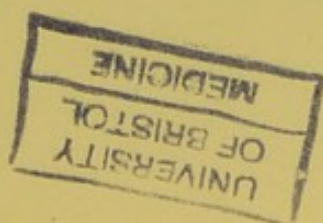
Hydrate. All these, be it remembered, are *within* the professional circle; no reference is made to the outside advertised nostrums. Now, let a clear and satisfactory reason be asked for, in explanation of this rise and fall of favourite remedies. It is a phenomenon worthy of investigation, and a true answer could not fail to be instructive. Moreover, the answer to this question will not fail to suggest other questions, and the answers to these, still further enquiries. What will be the convictions arrived at by this Socratic viaduct in the minds of intelligent patients? The very thought of these inevitable conclusions, and the anticipation of their inevitable consequences, one would think, must rouse to some energy the medical faculty.

Let any medical man, who is in earnest, satisfy himself upon this subject by fixing upon some common disorder; and, taking up the volumes of Braithwaite's *Retrospect of Practical Medicine* for the last forty years, let him make a catalogue of all the remedies which have been used, and on account of their success have been recommended, during that time, by practitioners more or less eminent. He will be surprised.

No one can doubt that Orthodox Practice is unsettled; and unsettled Practice cannot but be a grave defect in Orthodox Medicine.

V. PRACTITIONERS ARE IN BONDAGE TO AUTHORITY.

In Essay XI. the laws which governed the practice of Medicine among the Egyptians are referred to. These



laws will explain what I mean by the bondage in which medical men are enslaved in the present day. *Renouard*, in his "History of Medicine," thus describes them:—"The priests did not hesitate to seize upon the exclusive practice of the Art, and when they had collected a great mass of facts, they formed a Medical Code, the fruit of the experience of ages, which is called by Diodorus of Sicily the Sacred Book, from the directions of which they never were permitted to vary. If in following the rules there laid down, they could not save their patients, they were not held responsible; but according to Diodorus, they were punished with death, if, after departing from them, the result did not justify their course." "Unquestionably," adds M. Renouard, "this was an atrocious law, and must have arrested all further progress in the Healing Art."

The spirit of the Egyptian tyranny is the prevailing spirit of our times. If it is acknowledged that the patient has been treated according to the authorities of the day—though these may be in direct opposition to the authorities of a few years before—his death is accepted as satisfactory. While, if a medical man, having by observation and experiment become convinced that the prevalent theories are fatal errors, and that another method is more successful, shall, in the exercise of the right of private judgment, which is his duty, and in obedience to his conscience, which is also his duty, adopt this other method, notwithstanding that the result in the great majority of his cases is successful, if, in a few it is unfavourable, he is put to a social, if not to a physical death, by his professional brethren, who thus attempt to

"bear down conviction by mere force of names." Surely, it may be said, in the words of M. Renouard, this is an atrocious state of things, and as long as it is continued, must arrest all true progress in Medicine.

At the same time it is quite true that the opposite extreme would also be a terrible evil. If every medical man were to feel himself at liberty to follow his own devices in the treatment of his patients, the result would be a destructive anarchy. There is a large class of men in the Profession whose abilities are not greater than to allow them to be led; for all these to have a leader is their safest and happiest condition, and when their leader is a wise one their lot is to be envied. It is in medicine as in all other things, and conspicuously in all occupations where numbers are required, that the evil of one extreme does not rid the opposite extreme of its evil. In such cases the middle path is proverbially the safest. There should be liberty for those who can think, and their thoughts should be allowed freedom of expression and an attentive hearing. For those who must be led there should be leaders; but the leaders of this class ought not to be allowed to play the tyrant over the other class. At the present moment this tyranny is exercised in the profession of Medicine. Those who suffer most from this state of things are the *sick*. Would that the public could see this, and be convinced that it is their duty to themselves, and to their sick wives and children, to exert themselves to remove this oppression, and to open the gates of the prison in which thoughtful men are confined.

By unprejudiced minds this bondage to authority is

distinctly seen to be unjust and tyrannical, and, necessarily, is a great defect in Orthodox Medicine.

There is another tyranny in the opposite direction, from below upwards, in which the leaders are the victims, but I shrink from saying anything about this.

VI. THERE IS LITTLE PROGRESS.

My friend Dr. Lauder Brunton will be astonished at the heading of this paragraph. He closes his Lectures on "Pharmacology and Therapeutics" with these words: "If we compare our practice with that of the Egyptians and Greeks, we are discouraged; but if we glance at the advances made during a single lifetime, we cry, Slow has been the advance of medicine, because she went astray; now the path she follows is right, swift is her progress, and glorious will be her future." He had said a little before, that up to our own generation, "Pathology had hardly begun, and scientific Pharmacology did not exist." We are agreed in the fact, that during two or three thousand years there had been little progress; and in the reason, because medicine had all that time gone astray—the pursuit was made in a wrong path. But at this point, I am sorry to say, at present we differ. Dr. Brunton thinks that the chase is now in the right direction, and that progress is swift. I grieve to think that the course is still wrong, and the progress still slow. But I do not despair of a time when, not only Dr. Brunton, but many, if not all, will agree with me.

As to the fact of slowness—whoever can look calmly,

and free from the halo of enthusiasm which envelopes the heads of men engaged, as Dr. Brunton and his colleagues and contemporaries are, in exciting experiments in the physiological laboratory, will see plainly that the fancied "swift progress" is little better than a dream. Such an unprejudiced person will not be able to avoid seeing that there is as much hesitation, uncertainty, and contradictory opinion, on every important point of practice as there has been in former times—nay, this is understating the truth, for there never was a time in which the failure of practical medicine was so conspicuous as it is just now. This is one reason why faith in medicines never was at so low an ebb. What can be a stronger proof of this than the quotation already made in this Essay from the *Practitioner*?—"The system of our *leading* hospital physicians and surgeons is physiological medicine, characterised by a strong belief in the sanative powers—the *vis medicatrix*—of nature, and very great scepticism as regards the utility of drugs." Such, we are told, are the leaders, and by such I am not willing to be led. Is this unbecoming and unpardonable presumption?

Then, as to the reason of this slowness—it is, as Dr. Brunton says it was before, the wrongness of the path pursued. Medicine is still going astray. The proof which Dr. Brunton gives that the present path is "right," fails entirely; he says:—"Pharmacology is allying itself to Chemistry, and the rigid laws of the latter are beginning to extend to the former." The application of chemical analysis to drugs will, doubtless, sometimes be useful by making our knowledge of their composition more accurate;

but this does not help us in regard to their action as poisons, when taken by living men in health, or as remedies, when they are prescribed for sick people. Chemistry has been tried before our time as a guide in the study of disease and its treatment, but the attempt was a signal failure, as was also the mechanical theory which followed it. It will be said that Chemistry has made immense advances since the last century, and this is true; but it is still outside the province of Therapeutics. It has been repeatedly urged in these Essays, that the knowledges of life, of disease, and of cure, are studies of their own, and are not to be explained by chemistry, mechanics, electricity, or any other of the departments of science. Hunter's protest against this fundamental mistake was as vigorous as it could be made. "The stomach, gentlemen, is not a stew-pan, nor a mill, but a stomach, gentlemen, a stomach." The laws of Chemistry, so far as we know them at present, are not the laws of Therapeutics. To seek them there is to seek the living among the dead. The progress of Medicine cannot but be slow while such an error prevails.

Another error, which is an additional reason for the slowness of progress, is the trust which is put in experiments on the lower animals. This also is going astray; must end, like all journeys on wrong paths, in failure and disappointment. The action of drugs is *not* the same in different animal bodies, neither are the conditions of the experiments with animals sufficiently identical with the conditions of a prover or of a patient to allow reliable conclusions to be drawn from them. In an elaborate Paper by Dr. Joseph Coates "On the Relations of Minute Organ-

isms to Disease Processes," he says:—"It is remarkable that this bacillus, while it produces a *general* disease in mice, and is certainly *fatal* in a day or two, produces in rabbits a *local* disease, and is *not fatal*." And such "remarkable" confessions as this are being made all round.

In every age there is too high a value set upon the medical system prevailing at the time; and so much overconfidence is placed in it as to prevent the examination of other systems. And there is also so much deference paid to caste feelings, or professional etiquette as it is called, that suspicions, or even convictions, that there may be improvements in the hands of others, dare not be acknowledged.

VII. WRONG PATHS HAVE BEEN PURSUED.

From the facts briefly reported in the preceding Sections it is very evident that, up to the present time, the paths pursued by orthodox physicians have not led them to truth and success. It has been shown in former Essays that neither experiments on the sick, nor experiments on the lower animals, can teach us with sufficient exactness the real action of drugs, or the proper use of them as remedies, and that another path in the search for this knowledge ought to be tried. It has also been shown that the combining of several drugs in the same prescription, notwithstanding such elaborate works as the *Pharmacologia* of Dr. Paris, is a mistake quite intolerable in the future; and, further, that a similar

judgment must be arrived at on the large doses of nauseous and noxious drugs hitherto habitually prescribed. All these and other evil practices, such as the action on healthy parts, and the production in this way of artificial diseases to be added to those for the removal of which our aid is sought, have been, by plain and sufficient reasons, distinctly condemned. It is not necessary, therefore, to go into further details respecting any of these topics on this occasion. What the new path should be, it is believed will appear clearly before these Essays are concluded.

It ought not to be doubted that the aim of Medicine is to heal the sick. The gaining of this object, or failure in the attempt, is, therefore, necessarily the *test* to which every medical system must be submitted. The answer given by this test must be the final judgment upon it.

That up to our own time, the verdict given has always been unfavourable is not only admitted, but is strenuously asserted by every competent judge. It would be easy to fill an entire Essay with the testimony of distinguished practitioners in proof of this. What Molière and Addison have said in playful satire, physicians have again and again emphatically asserted in sober earnest. And this not in one country only, but in all. This confession of failure is summed up by Sir John Forbes at the conclusion of his book, "Of Nature and Art in the Cure of Disease," in these unmistakeable words:—"In only a very minute proportion of the numerous diseases presented to us in practice—and these few, for the most part, of slight importance—are we able to act positively or certainly, that is, directly or specifically on

the diseased part or on its morbid condition; while the whole huge remainder of diseases can, as we have seen, be only indirectly and slightly touched by our agents in any case—and in a large proportion of cases, cannot be touched at all.”

There are some, indeed, who, while they willingly allow that all the old systems have failed, flatter themselves that a brighter era has dawned, in which success will be achieved. Dr. Lauder Brunton, we have seen, is one of these. They think that physiological, microscopical, and chemical medicine is the true path for physicians to pursue, and be in no doubt of prospering. It is true that many interesting speculations may, in each of these ways, be put to the test of experiment; but not one of these, nor all of them put together, can teach a successful treatment of disease—a true science of Therapeutics; these hopes are doomed to disappointment. “Whatever cheering prospects,” says Sir James Forbes in the same book, “may thus be opened for the future, it must be confessed that, hitherto, practical medicine has derived but slight advantage from the new facts and views for which we are indebted to recent chemistry and microscopy,” and to which physiology may be added.

The continual *changes* of practice are incontrovertible evidence of ill success. No kind of treatment is settled or permanent. What is, for a while, thought successful, is soon found to fail and has to be discarded. What is next taken up is often the direct antagonist of the former, and, by and bye, it fails also. Once more, then, it is proved that the present, as well as all former, methods are wrong; and that the only quarter from which a ray

of hope comes to us, is when we are persuaded to walk on a new path.

The conclusion to which we are irresistibly led is this—that the Defects of Orthodox Medicine are fatal to its existence in its present form.

* * * * *

Nevertheless, in spite of these fatal defects, Orthodoxy in possession is so fascinating that it will probably maintain its ground for some time longer. It is respectable, aristocratic, safe, leads to promotion, wears a Court dress, and to fall out of its ranks is lowering and painful. “I think you are right, but you must not ask me to say so—it would get me into trouble!” Yet, though it now stands as square as an elephant, it will some day fall before the advancement of true knowledge, of which at present we have very little.

Jeremy Bentham laboured hard for many years in working out Dr. Priestley’s maxim—“The greatest happiness of the greatest number,” in the improvement of legislation. It will not, I hope, be thought presumptuous, if it is said that I have laboured hard for many years, to improve the work which Therapeutics have to do, in obeying the maxim—

THE BEST HEALTH FOR ALL!

ESSAY XLVI.

THE DEFECTS OF HAHNEMANN'S SYSTEM.

" ————— to believe that TRUTH is greater than authority, or fashion, or imagination, and will at last prevail."

DEAN STANLEY.

ANALYSIS.

INTRODUCTION.

- I. THE VAGUENESS OF THE PRINCIPLE.
- II. THE DEFECTS IN THE PROVING OF DRUGS IN HEALTH.
- III. HAHNEMANN'S INCONSISTENCIES.
- IV. HIS SPECULATIONS.
- V. THE CONFUSION CAUSED BY INFINITESIMAL DOSES.
- VI. OTHER IMPERFECTIONS.

ESSAY XLVI.*

THE DEFECTS OF HAHNEMANN'S SYSTEM.

"We can never get comfort by hiding any FACT."

F. D. MAURICE.

I AM not about to join the many to condemn Hahnemann's Homœopathy without examination; nor about to discard it after examination. Neither have I any feeling to express but one of thankfulness that five-and-thirty years of my life have been spent in a practical investigation of his system. But neither, on the other hand, can I shrink from pointing out its defects.

It is well known that I have never ranked myself among the admirers of Hahnemann. In the Preface to Dr. Dudgeon's translation of Hahnemann's chief work—the *Organon of Medicine*—in 1849, he says:—"The immortal work that contains the full exposition of that glorious and beneficent system of medicine, destined, probably at no very distant date, to supersede entirely all other systems and modes of treatment, where every page abounds in profound and original thought, and which in this the last Edition that underwent the Master's supervision, contains the principles of his doctrine in their most perfect and matured state, needs not the recommendation

* Written in 1882.

of overstrained eulogy, and requires no introductory Essay nor explanatory Notes. Perfect and complete in itself, it leaves no point of doctrine unexplained, no technical detail untouched, no adverse argument unanswered." My want of sympathy with this statement was strongly expressed in the Preface to the seventh Edition of these Essays in 1856; and in the thirteenth Essay of that Edition a vigorous repudiation of much of Hahnemann's system was delivered. In that Essay there is an account given of "those things in the system and teaching of Hahnemann which I reject." This was published in 1856.

The admiration of Hahnemann as a "Master" continues (1882); but his authority is now advocated in a different manner. Last year (1881) Dr. Hughes gave a Lecture in London, entitled "Hahnemann as a Philosopher—The Organon," in which he dwells upon the "considerable changes" which appear in the different Editions of that book; and instead of agreeing with Dr. Dudgeon to consider the latest as giving us the Master's matured thoughts, he takes pains to patch up a new Edition of the Organon out of all the five, and asks us to receive this as the true exposition of Homœopathy. This may be so or not, but it is not Hahnemann's "Organon." It may be the Organon of Dr. Hughes, in which he has profited by all that has been done and said during the thirty-eight years since the death of Hahnemann; but he must excuse us if we do not accept it as Hahnemann's.

No: the new system of Medicine did *not* appear perfected at once; nor can a perfect patchwork be made out of Hahnemann's various writings. His system is one more illustration of the truth of William Harvey's remark:—

"No great and perfect work is ever accomplished at a single effort, or receives its final polish from one instrument."

The conspicuous defects of Hahnemann's system are:—

I. THE VAGUENESS OF THE PRINCIPLE.

Hahnemann's principle is thus expressed:—" *Similia similibus curantur* "—"likes are cured by likes." In a certain sense it is the opposite of Galen's principle:—" *Contraria contrariis curantur* "—"contraries are cured by contraries." This latter, it is well known, governed medical men and their practice for thirteen hundred years; and which is still, though without much acknowledgment, obeyed to some extent.

Hahnemann has given, at different times, various accounts of what he understands of this principle; the following is taken from the last edition of the "Organon," which, according to Dr. Dudgeon, "contains the principles of his doctrine in their most perfect and matured state":—

"§. XXV. In all careful researches, pure experience, the only, the infallible oracle of Medicine, teaches us, that actually that medicine, which in its action on the healthy human body, has demonstrated its power of producing the greatest number of symptoms similar to those observable in the case of disease under treatment, does also, in doses of suitable potency and attenuation, rapidly, radically, and permanently remove the collective symptoms of this morbid state, that is, the whole disease present, and change it into health, and that all medicines

cure those diseases whose symptoms most nearly resemble their own without exception, and leave none of them uncured.

“§. XXVI. *This depends on the following homœopathic law of nature, some vague presentiment of which has indeed occasionally been entertained, but which until now, has not been acknowledged, and on which depends every real cure that has ever taken place:—A weaker dynamic affection is permanently extinguished in the living organism by a stronger one, if the latter (whilst differing in kind) is similar to the former in its manifestations.*

“§. XXVII. The curative power of medicines, therefore, depends on their symptoms, similar to the disease *but superior to it in strength*, so that each individual case of disease is most certainly, radically, rapidly, and permanently annihilated and destroyed, only by a medicine capable of exciting (in the health of a human being) in the most similar and complete manner, the *totality* of its symptoms, which at the same time *are stronger than the disease.*”*

From these paragraphs it is quite clear that Hahnemann's principle, as explained by himself, is a rule of practice *deduced* from a theory. For it is distinctly evident that his “homœopathic law” of *greater strength* is nothing more than a conjecture; yet he says the rule of *similia*, &c., depends upon this. A law or general fact requires to be proved by *induction* from a sufficient number of particular facts. Hahnemann does not even attempt this; he gives no proof whatever of this so-called “law of nature;” it necessarily sinks, therefore, to the

* *Hahnemann's Organon of Medicine*, translated by Dr. Dudgeon.

level of hypothesis. Just as the rules of the older practice were inferences or *deductions* from the various theories of disease, this rule is made an inference or *deduction* from a theory of cure.

It is necessary to call attention to the hypothetical basis of Hahnemann's system, as expounded by himself. He begins, indeed, as we have seen, with the remark that "pure experience is the only, the infallible oracle of Medicine," as if he were intending to bring forward facts; these facts are not forthcoming, but instead he gives us a theory. And afterwards, when treating of small doses, he throws overboard this "infallible oracle of experience" in the most energetic manner. In a note to §. CCXLVI. he says:—"It holds good, and will continue to hold good as a homœopathic therapeutic maxim, *not to be refuted by any experience in the world*, that the best dose of the properly selected remedy is always the very smallest one in one of the high dynamizations (the thirtieth), as well for chronic as for acute diseases."

The supremacy of theory over fact is as stoutly maintained by Hahnemann as by all his predecessors in the older or orthodox schools. Confidence in hypotheses and neglect of facts could not be carried further.

In the first of these Essays, written above thirty years ago, a different path was chosen; speculation, hypothesis, theory, were laid aside; and an attempt was made to look at facts. The view taken of the principle of "like curing like" was that it is a general fact, to be arrived at by induction; and afterwards, a large number of cases were given as individual facts, by which, as it seemed to me,

the general fact was proved. No effort was made to explain what this law "depends upon"; in other words, from what more general law it is a deduction. Whilst I was contending that, as a fact, Opium, which produces congestion of the brain when taken in health, would cure apoplexy from congestion, and similarly of other drugs, I was asked "Why?" by a very eminent man. I replied that I did not know, and could not tell. To me it was then—happily it is not now—an ultimate fact, "a principle or law of nature" claiming to be received as such, "unless it can be set aside by good evidence to the contrary." It was remarked that "it is not a speculative theory to be reasoned upon in the closet, but a fact to be observed at the bedside; it is not a metaphysical subject to be logically shown to be absurd; it is not a piece of presumption to be put down by authority—it is a fact to be examined, like the statement of any other fact, *upon evidence*. We are not called upon to sit down and imagine its possibility, or its impossibility, but we are urgently pressed to observe whether it be true or not."

This statement of the principle as an ultimate fact (the limit of our vision for the time), is still looked upon by many homœopathists as wholly true, and they habitually speak of the "homœopathic law," or the "law of similars," in this sense. It was right so to accept it till the discovery of *Antipraxy*—the contrary action of certain larger and smaller *doses* of drugs in health. By the new light thus thrown upon *similia similibus curantur*, what was before vague and difficult to apprehend, became clear and intelligible. The reason *why* is now seen. There is still a similarity between the disease to be cured

and the effects of drugs *in health*; but it is confined to the action of a certain series of larger doses; and the remedial action of the smaller doses is explained by its being, *in health*, in the opposite direction to that of the larger doses, and *therefore* in the opposite direction to that of the disease for which it is given. The action of the drug given in smaller doses is *contrary* and cures, because in larger doses it is *similar* to that of the disease. *Similia similibus* is limited to the effects of larger doses; the rule *contraria contrariis* is now clearly seen to belong to the effects of the smaller doses.

It follows from this discovery—the result of persevering experiment and enquiry into the truth of Homœopathy—that “like curing like” is not now an ultimate fact. So far as it remains true, it is changed into a formula or rule of practice deduced from what was to Hahnemann, and is still to homœopathists, an unknown law. The older systems of medicine had their formulas or rules; for example, Galen’s rule: give hot remedies for cold diseases, and *vice versâ*; this was a formula deduced from the doctrine that diseases are hot or the contrary, and that some drugs are hot and others cold. The rule to give antiphlogistics and the rule to give stimulants are, in the same manner, deductions from the supposed character of diseases. In what, then, does this new rule of practice differ from these older rules? In this most important particular—the older rules are deductions from hypotheses, and so was the homœopathic rule as enunciated by Hahnemann, but as now viewed, the new rule is a deduction from a general fact. The foundation of the former rules is an imaginary one, which may at any time be supplanted by

another fancy, when the deduction from it will perish. The foundation of the latter is firm, and may be expected to be permanent, because it is a *fact*.

And in what does this rule differ from Hahnemann's? Instead of being vague, it is distinctly intelligible; and instead of being made to "depend on" a theory, for the truth of which not the slightest proof is advanced, it is deduced from what is believed to be a universal or law-fact, proved by induction from a sufficient number of experiments. At the same time, it will be remembered that the rule of "like" as now maintained is restricted in its application to the action of certain larger doses of drugs, and admits the application of the rule of "contrary" to the action of the smaller doses.

It is a curious phenomenon of the present time, that there are some enterprising men who call Hahnemann Master, but reject his basis of homœopathy, and who also reject Antipraxy, and indeed, as far as appears, all other foundations, and yet speak of the "law of similars" as not a law but only a rule. Dr. Hughes, a Lecturer in the London School of Homœopathy, seems to be one of these. He says:—"Homœopathy as understood by all rational men among us, might be described thus—Homœopathy is a therapeutic method, formulated in the rule '*similia similibus curentur*'—'let likes be treated with likes.' The two elements of the comparison herein implied, are the effects of drugs on the healthy body, and the clinical features of disease." Dr. Hughes seems to have formulated a rule without either theory or fact for a foundation on which it may depend. He simply points to the rule as to something floating over our

heads like a cloud in the air—such clouds are apt to disappear.

May I be permitted to repeat that the real law is the general fact of the contrary action, *in health*, of larger and smaller doses of drugs, to which the name of Antipraxy has been given; and that the rule of practice, as understood by me now, is a deduction from this law-fact, and which embraces *both* of the quaint phrases so familiarly known—*similia similibus* and *contraria contrariis curantur*.

The vagueness of Hahnemann's principle or rule arises from several defects. One has been pointed out above; he has made it to depend on a mere hypothesis, for the truth of which he offers no proof. Another great defect is the indefiniteness which lies in the want of distinction, in the provings of drugs in health, between the effects of different doses of the same drug. Whatever symptoms are produced by a drug in all doses are put together, and all are to be used alike as "similars" to the symptoms of disease. A perplexing practical difficulty springs out of this defect, for in every extended proving opposite symptoms are found to belong to the same drug; excitement and depression, dilatation and contraction, excess and failure of the different secretions; in short, increase and diminution of the functional performances of every organ affected by the drug. How, under such circumstances, is a young practitioner to select the best remedy for his patient according to the rule, "let likes be treated with likes"? Opium excites the brain, and stupefies it. For which state is it the true remedy? Only Antipraxy can answer this question. It is the smaller dose which excites, it is the larger dose

which oppresses. The action of the larger dose is the "similar" pointing out the case to be treated by Opium; the action of the smaller dose is the remedy. To give Opium to quiet an excited brain may, from Hahnemann's provings, be called homœopathy; but it is wretchedly bad practice. Opium, as a remedy, in small doses, for an oppressed brain, is the best we know.

Another great defect in his rule is its indefiniteness as applied by Hahnemann. He not only applies it to drugs, but also to a miscellaneous assembly of causes of disease, which are of the most diverse character. Wild examples of some of these are given in a long note to §. XXVI. of the *Organon*:—"Thus are cured both physical affections and moral maladies. How is it that in the early dawn the brilliant Jupiter vanishes from the gaze of the beholder? By a stronger very similar power acting on his optic nerve, the brightness of approaching day! [This is not a *similar*, but a stronger degree of the *same* power.] In situations replete with fetid odours, wherewith is it usual to soothe effectually the offended olfactory nerves? With snuff, that affects the sense of smell in a similar but stronger manner," &c., &c.

Other defects might be mentioned, but these are more than sufficient to prove that the principle, as explained by Hahnemann, is vague, obscure, and undefined.

II. THE DEFECTS IN THE PROVINGS OF DRUGS IN HEALTH.

(1). *Their limitation to symptoms.*—Making inventories of symptoms only, whether in experimenting with

drugs in health, or in investigating a case of disease, is so universally felt to be degrading to a physician, that many homœopathists have done their best either to evade or to deny the fact that this is Hahnemann's method. The question has been repeatedly considered in former Essays, and I think it has been proved beyond doubt that, so far as Hahnemann had any fixed method at all, it was his determination to establish the practice of dealing with symptoms only, in the treatment of patients. Just now, however, we have to consider the subject with reference to the experiments with drugs on healthy persons, and no one who is at all familiar with the volumes of Hahnemann's *Materia Medica Pura*, in which the experiments made by himself and his friends are recorded, can doubt the fact that *nothing but symptoms appear in these provings*.

This being so, it should not be difficult to convince any educated person that such a limitation is a great defect. It will not be contended by anyone that the knowledge of the anatomy, physiology, and pathology of man's body can be of no use in the diagnosis and prognosis of diseases; and any knowledge which can help us in understanding the seat and nature and probable course of an ailment, assuredly ought to be of use also in conducting its treatment.

This proposition seems to me to be so obvious, that it does not require further evidence to prove it.

(2). *The distribution of symptoms in regions.*—In the *Materia Medica Pura*, the symptoms which followed the taking of a drug by a healthy person are not recorded chronologically, or so as to show any connection with each

other, as has universally been done in cases of disease; they are broken up into fragments, and scattered among the various regions of the body; they are assigned to the head, face, eyes, ears, nose, lips, chin, lower jaw, teeth, tongue, throat, pharynx, stomach, region of the liver, &c. This dispersion of the symptoms occurring during an experiment with a drug by a person in health, which might so easily have been recorded as is done in a case of illness from any other cause, renders it impossible to obtain a true picture of the action of the drug. The connection of the various symptoms with each other is broken and cannot be restored. This, surely, is an enormous defect.

But another consequence follows this artificial arrangement of the symptoms. The application of pathological knowledge to them is absolutely forbidden. It is impossible to assign the symptoms recorded to the internal organs in which they have originated. Some time ago I devoted the leisure of five years to this attempt, and found, in this practical manner, that it could not be done. If any further proof were wanting that Hahnemann was bent upon *permanently excluding* pathology from therapeutics, we have it here. The unsatisfactory condition of Pathology in Hahnemann's time is urged in his defence; but this artificial arrangement of symptoms not only excluded the pathology of his own time, it equally excludes the pathology of all future time.

This scattering of the symptoms in every proving, from their connections with each other and with their seat, is not only a great defect, but as regards all the provings of Hahnemann, it is irreparable. The damage can be removed only by new provings.

(3). *The combination of the symptoms of all the provers of a drug.*—This is another remarkable feature in Hahnemann's *Materia Medica*. Not only are the observations made during a proving confined to symptoms; not only are these symptoms, in the account given of them, broken up into a large number of divisions named after the external parts of the body; but the symptoms observed by five or six, or a dozen or twenty different experimenters with the same drug, are all collected together into one inventory, and almost always without any indication to which of the provers they belong. Take *Arnica* for an example, and we read:—"This drug has been proved by myself, and Drs. Franz, Gross, Hahnemann, Hornburg, Kummer, Langhammer, Wislicenus." And then we have under *Head* sixty-four symptoms, consisting of various kinds of giddiness, aches, and pains, which have been written down by these eight experimenters, but without the smallest distinction or reference to any one of them. Under *Belladonna* there are a hundred and seventy *Head* symptoms, inventoried in the same manner. And this accumulation from different sources and independent experiments is continued through all the other regions of the body. What kind of diagnosis could be made out of a dozen or a score of cases of typhoid fever, or other well-known disorder, with all the *Head* symptoms put together in a similar fashion? It could not be successfully done without a prompter. No more can the proving of any one of these several experimenters be taken out from the rest. Hahnemann insists much upon "individualizing" every case, and treating it as a special disease, often without

much heed being given to the genus to which it belongs. How perversely has he frustrated all attempts at individualizing the provings to which he sends us to find a similar! The result is that the "similar" found is commonly an artificial one, made up of symptoms which have never been experienced together by any single prover. It must follow also that the "similar" thus made up cannot often represent the pathological condition which the drug has power to produce, and which it always will produce, so far as the predisposition of each prover will permit. It should be remembered that it is this pathological condition only, which is the true "similar" to any natural disease, and that it must have been produced by the larger doses of the drug, if this drug, in its smaller doses, is to be the true and best remedy for the case in hand.

It must be evident, I think, that this putting together of the symptoms of all the provers of a drug is a great defect in Hahnemann's *Materia Medica Pura*.

(4). *The absence of the succession and connection of symptoms.*—The elements of *time* and *place* are often of the first importance in judging of the value of a symptom. Hence the necessity for knowing, with reference to the symptom in question, what other symptoms have preceded, accompanied, or followed it. This is so well known by practitioners that it would be useless to insist upon it. A single instance may be given as an example: *hiccough*, this may be an exceedingly slight symptom in one case, a mortal one in another; it may arise from the smallest amount of indigestion; or it may be the accom-

paniment of a strangulated hernia. Hahnemann attaches this symptom to *Pulsatilla* and to *Nux-vomica*. We may have learned from other sources that the hiccough of *Pulsatilla* is from indigestion, while that of *Nux-vomica* is preceded and accompanied by a much more serious condition. As remedies, *Pulsatilla* is for the stomach, and *Nux-vomica* also may be in some cases; but I have seen *Nux-vomica* save a patient in very critical circumstances, from the suffering and danger, and myself from the anxiety, of an operation for strangulated hernia. In the provings of Hahnemann, there is not only no proper history given of this succession and connection, but they are so effectually hidden by the dislocation of every symptom from its relative position in time and place, that it is impossible to recover them. This also is a great defect.

(5). *The absence of any distinction in the effects of different doses of the same drug.*—Hahnemann says truly concerning the proving of drugs in health, that they are “experiments which are the essential basis of our system, and require great caution, acuteness of sense, fine perceptive powers, severe discrimination in regard to one’s own sensations and observations, and a correct choice of expressions.” Being alive to all this, and especially to the fact that these experiments on healthy persons are *the basis of his system*, it is a great wonder, and has always been to me a subject of deep regret, that he should have presented his provings to us in the form we have them. In an undertaking so new as this it is marvellous that he did not give us, at least in the first

instance, the facts in their simplest form. We had had them given us before by Alexander of Edinburgh and by Crumpe in Ireland, perfectly; why should we not have had them given us in the same manner by Hahnemann? In the beginning of this investigation it was a severe disappointment to me to miss the original provings; and to have them only in their present condition has been a continual mortification. Among the many points felt to be sorely lacking in these provings, is the question of doses; assuredly, it ought to have been evident to him that the multitude of symptoms which he has attached to each drug could not be accepted without the addition of the doses which had caused them. The balance and the measure are as essential to Therapeutics, as they were in the hands of Cavendish to modern Chemistry.

Hahnemann's first experiment was with Cinchona, and he tells us he took half-an-ounce in two days. Why has he not told us in the same way the dose in every proving? He easily could have done so, as it must have been known to him at the time. But he has not done this, and the value of his provings is immeasurably reduced by the omission. This is another reason why it is necessary to repeat the experiments—a necessity greatly to be regretted, for the labour is a serious one; but we are not to be discouraged on this account, but rather stimulated to undertake it with greater zeal and perseverance. Seeing that such good results have followed from a work so imperfectly done, we can be quite sure that still better consequences will come of the same work when it is better done. The result of proving small doses in my hands is a sufficient earnest of this promise. It may, therefore, be concluded

that the omission of the doses in Hahnemann's provings is another great defect.

Here, then, are five distinct and serious defects in the *Materia Medica* of Hahnemann. It would be easy to point out others, but it is not necessary to do so; these are sufficient to show, not that the undertaking should be abandoned, but on the contrary, that fresh experiments with drugs in health should be zealously undertaken by a large number of healthy persons. The reward would be inexpressibly great. Are there not some who are willing to do it?

III. HAHNEMANN'S INCONSISTENCIES.

It is with no wish to damage the character of Hahnemann as a man that these are noticed—*de mortuis nil nisi bonum*; but in reference to his system and the reception of it by others, it is impossible to avoid the consideration of them. They are many and great, and of a very perplexing kind. One would have thought that to those who call him *Master* this perplexity would be overwhelming; especially when they remember his intolerance and dogmatism as expressed in this sentence:—"He who does not walk on exactly the same line with me, who diverges, if it be but the breadth of a straw, to the right hand or to the left, is an apostate and a traitor, and with him I will have nothing to do."* It is well for me that I never ran the risk of this terrible excommunication. In an early Essay it was said:—"Every feature of Hahne-

* Dr. Dudgeon's *Lectures on Homœopathy*, p. XLIII.

mann's exposition of his system has, I think, now been noticed; and there is not one which is to be admired or adopted *in the terms* in which they are propounded by him. As expressed in his writings, they all, without exception, excite in the mind a strong repugnance. Every detail is presented in so distasteful a manner as to produce a feeling of absolute dislike; so that, had I read Hahnemann's works *before* making a practical trial of the method, I never should have been induced to begin it. They are filled with unproved assertions, illogical arguments, fanciful speculations, and obvious contradictions. It is due to myself to have made these statements. Having adopted the practice of Homœopathy, I may be supposed to be a disciple of Hahnemann, and be held responsible for his follies. Such relationship and responsibility are altogether disclaimed.”*

Of Hahnemann's inconsistencies it would not be difficult to fill several pages with illustrative examples; but to do this would be wearisome both to myself and to my gentle reader. Many have necessarily been alluded to in the course of this investigation, and this Essay is not without instances of them. It will be sufficient to refer to the manner in which at one time he speaks in praise of *experience*, and at another how he slights it. We have seen that he speaks of “pure experience” as “the only, the infallible oracle of medicine,” and he called his system at first “The Medicine of experience.” In §. CCLXXXI. he exclaims:—“How unmeaning and ridiculous is mere *theoretical* scepticism in opposition to this unerring, infallible *experimental* proof!” And then

* Essay XIII. (1856).

we have seen that in another paragraph he tells us that his theoretical opinion about the very smallest being always the "best dose" is a maxim "*not to be refuted by any experience in the world.*" This arbitrary regulation, dogmatically laid down as true *in spite of experience*, may have been intended to guide his "disciples"; he certainly did not feel bound by it himself, for, to the end of his life, he continued to prescribe a great variety of doses. Hahnemann was one of those to whom it was easier to teach twenty men what to do, than to be one of the twenty to follow his own teaching. Such inconsistencies, and there are a great many, are a formidable defect in Hahnemann's system.

IV. HIS SPECULATIONS.

"How charming is divine" speculation! There is scarcely a greater fascination in the world. There are few who do not take more pleasure in endeavouring to account for a fact, especially if it is not *quite* true, than in the fact itself. How an egg stands on its end, why a fish can be put into a basin full of water without any of the water being spilt, and such-like innocent enquiries, will occupy many minds, rather than experiments to ascertain what statements are facts: so it was with Hahnemann. We have just now had a glimpse of his inconsistencies. He is eager to lay down his laws, and equally eager to excommunicate his followers, forgetting that he is excommunicating himself. As to followers such as he wishes to have, could he come amongst us now,

he would not find *one*. As to his speculations, they are transcendental and fanciful in the highest degree. We are reminded by them that there are some minds which rejoice in this unprofitable occupation, and which would far rather have a mystery with a fantastic explanation, than an intelligible story of matter-of-fact. Perhaps they may charitably be allowed to build their castles in the air for their own amusement, though it may well be doubted how far this is charity to them; but it is intolerable that they should think to lead others astray by such imaginations, and still more so that they should expect to compel others to obey their unproved maxims.

No one has used more severe language in the condemnation of theory than has Hahnemann. In his Introduction to the Organon of Medicine, and in his earlier writings, he is lavish in the strongest expressions of abuse when speaking of the "imaginary" and "maniacal principles" of former physicians. Yet no one has yielded more than he has done to the fascination of hypothesis. He supposes that he explains clearly the nature of disease by giving to it a "dynamic" origin, and then he says of former theories:—"These were all idle dreams, unfounded assumptions and hypotheses, cunningly devised for the convenience of Therapeutics. . . . But the essential nature of diseases and their cure will not adapt themselves to such fantasies, nor to the convenience of medical men; to humour such stupid, baseless hypotheses, diseases will not cease to be (spiritual) dynamic derangements of our spiritual vital principle in sensations and functions, that is, immaterial derangements of the state of health."* It

* *Organon*, pages 10, 16, 17.

is clear that this notion of "dynamic derangements" is as much hypothetical assumption as anything he has so unsparingly condemned. "Dynamic" (*δύναμις*) is "power"; but so applied who knows what it means? or what idea to attach to the word? He applies the same language to medicines. "§. CCLXIX. The homœopathic system of medicine develops for its use, to an unheard-of degree, the *spiritual* medicinal powers of crude substances by means of a process peculiar to it, and which has hitherto never been tried." "Homœopathic *dynamizations* are real awakenings of the medicinal properties that lie dormant in natural bodies during their crude state," &c. &c. Transcendental speculations like these, and like his "homœopathic law of nature" noticed in a preceding section, have been a powerful impediment to the calm study of homœopathy. They are a great defect in Hahnemann's system.

V. THE CONFUSION CAUSED BY INFINITESIMAL DOSES.

And what are infinitesimal doses? It is due to Hahnemann to accept his own account of them. "§. CCLXX. Two drops of the fresh vegetable juice mingled with equal parts of alcohol are diluted with ninety-eight drops of alcohol and potentized by means of two succussions, whereby the first development of power is formed; and this process is repeated through twenty-nine more phials, each of which is filled three-quarters full with ninety-nine drops of alcohol; and each succeeding phial is to be pro-

vided with one drop from the preceding phial (which has already been shaken twice), and is in its turn shaken; and in the same manner at last the thirtieth development of power (potentized decillionth dilution), which is the one most generally used."

Now, if it be without question that the decillionth dilution is entitled to be called infinitesimal, it is equally certain that the first dilution, which contains one drop of the vegetable sap in a hundred drops of the tincture, is *not* infinitesimal. Such a dose of Rhubarb, for instance, is very appreciable to three of our five senses; we can very easily see it, taste it, and smell it; and we have no doubt whatever that there is tincture of rhubarb in the phial.

Then, where is the line to be drawn between "appreciable" and infinitesimal doses? This is an example of a problem which meets us on every side. It has been thought, both in ancient and modern times, that the difficulty of drawing lines of separation between natural phenomena, is universal; it was expressed by the ancients in the axiom—*nihil per saltum*. It is common; but, so far as our knowledge carries us at present—and we are bound by the limits of our knowledge—it is not universal; it is not a law of nature. There is a gulf which has not been bridged by us between nothing and material things; there is another between the material of protoplasm and life; another between instinct and reason; and another between life and death; and there will be another between death and resurrection. These gaping chasms are solemn subjects for thought; so also is the progressive evolution and advancement manifest in all nature around us. This, indeed, is such that some old divisions, such as that

between vegetable and animal organization, are now abandoned. I think this will also happen to the division now drawn between "living" structureless protoplasm and the "non-living" structures which constitute the living body of man. So, to come back to our own subject, we can see no fixed line of division between doses which are appreciable by our senses and doses which are not, and to which the name of infinitesimal has been given. It is necessary, therefore, to draw an artificial line, and that which I have been accustomed to draw is between the third and the fourth centesimal dilution. The reason for doing so is this:—the division of a grain of a solid, or of a drop of a liquid medicine, may be carried to a million of parts, and yet be appreciable by one or another of our senses. Some drugs, for example, iron, can be detected, when thus divided, even by chemical analysis; some can be seen by the help of the microscope; a few, as musk, can be detected by the olfactory nerves. In these Essays drugs divided into a hundred, a thousand, or a million of parts, are called appreciable doses, and they are included within the meaning of doses with which experiments have been made by me both on the healthy and on the sick. Beyond these are the infinitesimal doses, and upon them I do not presume to offer an opinion. It appears to me that the time for their successful examination has not yet arrived. When it does come, it is quite certain that the first step to be taken must be very careful experiments with them *in health*. If these fail to give satisfactory proofs of action, it will be in vain to talk of using them in sickness according to the "homœopathic law."

It is well known that Hahnemann, and the great

majority of homœopathists, have had no hesitation in dealing with them and speaking of them as if our knowledge of their action, both in health and in disease, was clear, definite, and indisputable. And especially has it been taken for granted that the rule of *similia similibus curantur* as certainly applies to them as it does to doses of any magnitude whatever. This has not yet been proved; and until it is we must decline to admit any results, with reference to supposed cures by infinitesimal doses, however apparently true they may be, to be evidence in support of the principle of Homœopathy. They *may* be such, or they *may not* be such; proofs are wanting. In this state of the question the introduction of them into the already sufficiently difficult and obscure laws of Therapeutics, has brought in an element of confusion, which, to say the least, has been a terrible hindrance to the progress of the new method. It is evident, therefore, that this has been a great defect in Hahnemann's system.

VI. OTHER IMPERFECTIONS.

(1). All medical treatment, to be really as good as it ought to be, must, in the first instance, be dependent upon the extent of our knowledge of anatomy, physiology, and pathology. Thanks to the unwearied industry of our predecessors and contemporaries, this knowledge is now very considerable; still, it is not perfect; and so far as it is defective, it is a necessary bar to the perfectness of medical treatment of any kind. Though not owned as such by Hahnemann, this defective knowledge is a greater

hindrance to good treatment on the new method than on any of the older ones; we are, therefore, bound thankfully to avail ourselves of all that has been taught us already, and to encourage, as far as in us lies, further labours in each of these three departments of knowledge.

(2). The imperfection in the proving or testing of drugs by experiments with them *in health* has already been pointed out. At present it is a great one, and so urgent that I must be excused for calling attention to it again. When medical men are roused from their lethargy, and are become willing to give to experiments with drugs on the healthy the same amount of earnest work which they have given to anatomy, physiology, and pathology, this imperfection will be much diminished.

(3). What is called malignant disease forms a subject which calls for much more serious attention from the orthodox part of the profession than it has yet received. Ailments condemned by this bad character are hastily concluded to be incurable, and are at once handed over to Surgery. This is a frightful mistake. It was recognised by Lord Bacon. He says:—"The physician dischargeth the weakness of his Art upon supposed impossibilities." It is required that these hastily called incurable diseases be dealt with by the physician. The mistake exists in the new school, but not nearly to the same extent. If it is asked—How is the practitioner to deal with such cases? It is replied—As he has dealt with what he calls curable diseases. A physician practising on the new method has great confidence in the successful treatment of many cases—such as some inflammations, some fevers, and some neuralgias—in fact, of the ailments which are most

common. They can be imitated by drugs taken in health; the principal thing which remains to perfect their treatment is a more accurate knowledge of the action of *different doses* of the same drug. It is required that daily attempts be made to extend this comparatively perfect treatment of common ailments to the less common and less understood diseases. The imperfection which will operate most powerfully, and for the longest time, in the treatment of so-called malignant diseases is this: few will think themselves called upon to take drugs till similar diseases are produced by them. But though men may shrink from doing this intentionally, now and then such a result may accidentally happen, and this should be turned to the greatest account possible. Short of this a good deal may be learned from the organs on which a drug is known to act, and from the nature of its action, which may suggest and justify a trial of it in organic diseases of the same part. Again, though the method of learning *ex usu in morbis* hitherto followed, has again and again been condemned in these Essays, this does not hinder that, occasionally, our imperfect knowledge may compel us to have recourse to it. In another Essay something more may be said upon this subject; it is sufficient here to notice the fact that, at present, the treatment of organic diseases remains imperfect, chiefly because it is quickly laid aside, and surgical operations are substituted. Happily, some attempts are being made by both schools. For instance, in the old school, Professor Clay, of Birmingham, has been engaged for some time in very interesting experiments in the treatment of cancer, specially of uterine cancer, with genuine *Chian tur-*

pentine.* In these experiments it is much to be regretted that the Chian turpentine has not been given alone, so that the old uncertainty from polypharmacy might have been avoided. The new drug has been prescribed mixed with *Sulphur*; and during the treatment *Quinine*, *Ammoniated Copper*, *Perchloride of Iron*, *Arsenic*, *Iodide of Calcium*, and other drugs, have been had recourse to, either internally or externally, along with the Turpentine. For experiments like these, it is imperative that they be made with a single drug; and to render any conclusions from them satisfactory, they must be tried on the healthy as well as on the sick. This last duty has, hitherto, been undertaken only in the new school, and with some success, in reference to malignant disease, with a few drugs, such as *Hydrastis Canadensis* and *Phytolacca decandra*. But very much greater efforts still remain to be made in this direction, and every earnest and conscientious practitioner should take part in them.

We have now looked at many serious defects, both of orthodox systems and of Hahnemann's. What, in few words, is the difference between them? The defects of orthodoxy are fundamental; it has always been travelling on *false paths*. Hahnemann entered upon the *true path* when he began experiments with drugs upon the healthy, and with one drug at a time; but he pursued it in a mistaken manner, making many blunders. The defects of the old system are *fatal* to them; the systems must be given up altogether. The defects which have Hahnemann

* *On the Treatment of Cancer*, by John Clay. 1882. Churchill.

for their author *may be removed* by the labours of others, and the true path may be pursued with continually increasing success.

* * * * *

Some will censure me for having taken so much pains to expose the defects of Medicine, and especially the defects of Homœopathy. But "truth is truth to the end of reckoning"; and I have said these things because they are true, and because the welfare of the sick demands that the truth should be spoken.

Possibly, some may take advantage of what has been said, thinking that it may help them in their opposition to the treatment of diseases by small doses of drugs. They will not find that it does so, if what they take has not its true meaning altered by its separation from the context; and they will do well to remember that misrepresentation, sooner or later, recoils upon the head of those who have been guilty of it.

ESSAY XLVII.

THE MEDICINE OF THE FUTURE.

"Great works are performed, not by strength, but perseverance."

SAMUEL JOHNSON.

ANALYSIS.

- I. THE PATIENT.
- II. PREDISPOSITION.
- III. THE EXCITING CAUSE.
- IV. THE SEAT OF DISEASE.
- V. THE KIND OF DISEASE.
- VI. DRUGS ARE REMEDIES FOR DISEASE.
- VII. HOW DRUGS ARE TO BE PRESCRIBED.

ESSAY XLVII.*

THE MEDICINE OF THE FUTURE.

*"The miserable have no other medicine
But only HOPE."*

SHAKSPEARE.

"RASSELAS returned discouraged and dejected; but having now known the blessing of *hope*, resolved never to despair." It is with such feelings of discouragement and dejection that the two preceding Essays will have been read by thoughtful students. Nevertheless, there is even in the midst of this depression a gleam of hope, and a suggestion of better things. The hope must drive away despair, and the suggestion must be actively carried out. Let it be remembered that this Essay is written for medical students, and the object had in view is to encourage them to make a start in the new career. They will find themselves able to gather up much that will be permanently useful from both the older and the newer schools. For the sake of clearness the method of former Essays shall be followed, and the remarks to which their attention is invited shall be arranged under the following heads:—

* Written in 1882.

I. THE PATIENT.

After a conscientious devotion to his duty, the first qualification of a physician, if he is to be a successful one, is that he has *eyes*; the second, that he knows *how to use them*. He will thus often learn more from what he sees than from what he hears, though he will be expected to ask questions and to hear narratives sometimes tedious. While talking and listening he will be looking at his patient. And, if he follows my advice and example, he will write brief notes of every case he sees.

The things to be observed are, for the most part, obvious, and have been frequently insisted upon in medical books. So that, if they are here only briefly noticed, it is not on that account that they are to be thought trivial or commonplace, or that they may be neglected with impunity. They are chiefly—the Sex; the Age; the Constitution and Temperament; the Habits, Occupation, and Circumstances; the Nature and the Duration of the Illness and its past Treatment. Each of these carries with it a declaration of “urgency” in the mind of the patient; and it is incumbent upon the medical man to give it serious thought.

The Sex.—From a medical point of view the difference of sex involves considerations which cannot be overlooked or forgotten without detriment, perhaps damage, to the patient. Not unfrequently the greater part, sometimes the whole, of the character of the case belongs to this feature of it; and the treatment to be appropriate must be conformed to this character. Often even the same ailment cannot be best treated by the same remedy in the two

sexes. A few drugs seem to belong specially to men; more certainly do to women. There is often a more sensitive response to the action of drugs in women, and therefore smaller doses are sufficient for them. In a visit to a patient, or in a consultation by one, the practitioner must never be off his guard, never lose his presence of mind, never let his thoughts wander from his work, never fall into routine. It may sometimes be useful to him to remember that giving attention to what may seem small matters will be apt to irritate men, but will often conciliate and increase the confidence of women.

The Age.—Differences in this respect are also important; the child, the grown-up person, the elderly—each has its own character, whatever the disorder may be, requiring to be taken into account. This character will sometimes determine the remedy. There are many physiological, and even some anatomical conditions, which are peculiar to age; so that diseases which, unfortunately, may have the same name given to them, are not really the same in childhood, maturity, and old age; it follows that they cannot always be successfully treated by the same remedies. The wonderful activity and sensitiveness of both animal and organic life in children, has often excited admiration; the strength and comparative stability of middle age is a grateful and reassuring fact; the visible failure in strength and stability in the old, affords them a happy opportunity of calmly looking back upon the past with thankfulness, and forward to the future with hope—a hope full of immortality. These and many minor differences connected with age and affecting the

character and course of diseases, ought at all times to be visible to the eyes of the medical adviser.

The Constitution and Temperament.—The differences in these respects are often great and demand attention. Two persons may be in health, but one is vigorous, and the other feeble; one is sanguine, the other phlegmatic. When these persons become sick, their illnesses will necessarily be influenced by the conditions of their previous health. Such influences must needs be attended to when their illnesses are prescribed for. The physical treatment, including the action of drugs, should be adapted to the constitution; the mental and moral treatment should, in like manner, be suited to the temperament. Some time ago I went above a hundred miles to see a lady who had passed safely through her first confinement about a fortnight before, but who was exhausted by profuse sanguineous discharge; ordinary remedies had had no effect, she was taking frequently beef-tea and brandy, she thought herself dying, all her friends were alarmed, and her husband had given her up in despair. I found the windows closed, the blinds down, and the nurse and others in most assiduous and anxious attendance, three or four persons being about her bed day and night, and she speaking only in a whisper. At first I asked for light, that I might *see* my patient; the next request was that a window might be opened; then that some of the bed-clothing might be removed, and a smaller and much thinner binder might be substituted for the one then in use. After quietly effecting these and a few similar changes, I said to her, "Now you must be content with one attendant at a time, and with seeing your husband

only for a short time occasionally during the day, and you are not to send for him at all in the night; you are to take suitable food at proper intervals, and to have no more brandy." She burst into tears. I said, "That will do you good, and you have now nothing to do but to get well." At a subsequent visit, she thanked me for what I had said to her, and she rapidly recovered without any drawback. Her temperament was a nervous one.

The Habits, Occupation, and Circumstances.—These necessarily impress themselves upon the illness of any patient, and cannot safely be overlooked by his medical adviser. When one who has been much out-of-doors, taken abundant exercise, lived temperately, and from healthy fatigue slept through the night, has an attack of acute disease, he will be in a very different condition, and require very different treatment from another who has been sedentary, luxurious, and wakeful, and is suffering from a similar attack. A gardener in the sky of Surrey and a grinder in the smoke of Sheffield are visited by illness under circumstances so different that to give their illnesses the same name seems almost absurd. Such nomenclature can be no guide to treatment, and must necessarily play false with statistics. Many habits are conducive to health, and the having acquired them is a happiness: such as early rising; regularity and moderation in food and exercise; daily and not excessive or spasmodic employment; habitual cheerfulness; "a quiet mind and a thankful heart." While many have a contrary effect: such as excesses of all kinds. I have sometimes been able to tell patients that they were suffering from the effects of tobacco, without asking them if they smoked,

and to tell others that their skin disease was caused by salt, before learning from them how much they were in the habit of taking. The question of habits may not be overlooked in the treatment of patients; a perseverance in some will effectually prevent the success of the most judicious prescribing. Dr. Cadogan's pamphlet of the last century deserves to be remembered. He contends concerning gout that it is not hereditary; that it is not periodical; and that it is not incurable; that its causes are indolence, intemperance, and vexation; and that its remedies are the contraries of these—exercise, temperance, and peace of mind. And he thinks that what is true of gout is also true of the great majority of chronic diseases. Remarks similar to these on habits belong also to occupations and surroundings; witness the chimney-sweep's cancer, the knife-grinder's asthma, and the artilleryman's deafness.

The Nature and the Duration of the Illness, and its Past Treatment.—Questions of the greatest interest also hang upon the facts under this item of accounts. Sometimes a patient has to be seen who, up to an hour before, was in good health, but who is now dangerously ill, and nothing medical has yet been done for him. At another time a patient comes who has been ill for months or years of a chronic disease, for which many doctors have been consulted, much physic has been taken, and mountains and seas visited, in vain. We see at once how promptitude and presence of mind are called for in the one case, and how caution and circumspection are required in the other.

II. PREDISPOSITION.

Diseases are the result of the combination of a condition and a cause. These have commonly been spoken of as two causes—a predisposing and an exciting cause. I think it will clear away some confusion if the first is considered rather a *condition* than a *cause*; and we will call it Predisposition rather than Predisposing Cause. The second is the Exciting Cause.

Predisposition is a state of the body, or of parts of it, which does not exist in perfect health, and which produces a susceptibility to the action of an exciting cause. This predisposition may be brought about in a variety of ways. These have been considered in some detail in Essay XLIII. What we have now to notice is the condition itself, not the causes which produced it. Predisposition is often meant when such words as susceptibility, sensitiveness, aptitude, tendency, are used in reference to health or disease. It may belong to any part of the body, or to so many parts as often to be spoken of as “constitutional,” as if it belonged to the entire body. It may be illustrated, or its meaning be best explained, by a few examples:—Two persons may be exposed to a contagion: one will be affected by it; the other not. Here clearly the difference is not in the contagion, but in the persons. In one there was predisposition, in the other it was absent. A dose of *Aconite* or of *Digitalis*, which will affect one man’s heart with painful force, will have no effect at all upon the heart of another man. Here, as clearly, the difference is not in the drug, but in the sensitiveness of the organ. There is a remarkable note

in the "Nomenclature of Diseases," published on the authority of the Royal College of Physicians of London, and forwarded to every medical man "by authority of the Registrar General," which, while it shows plainly the futility of the attempt, so long persevered in by medical writers, and in this book sanctioned by the College of Physicians itself, to divide diseases into "general" and "local," is a good illustration of what should be understood by the word *predisposition*. Under "general diseases," which are really as "local" as those which afterwards follow under that title, we find on page 25 the word "Scrofula," with this "Note" attached to it:—"The constitutional tendency which has received the name of the Scrofulous Diathesis, *when unattended by local lesions*, is not to be returned as *a disease*."

Predisposition seems often to consist simply in a lowering of the vital force. Dr. Bristowe, in his admirable "Treatise on the Theory and Practice of Medicine," gives an illustration which makes the "predisposing cause" and the "exciting cause" appear to change places. A man exposed first to a depressing cold and then to a malarial atmosphere, will have an attack of ague. It is usual to call the cold the predisposing cause, and the malaria the exciting cause. Suppose the ague is apparently cured, and the man again exposed to cold, and he will have another attack of the marsh fever. *Now* the malaria is called the predisposing cause, and the cold the exciting cause. Dr. Bristowe adds, "Yet, notwithstanding, malaria is equally in both cases the specific cause of the disease, and acts (as we have no reason to doubt) in both cases in a precisely

similar manner" (page 8). The confusion, in fact, does not exist in nature, but in the obscurities of language. If we are content to speak of cold or privation as having produced a condition which did not exist previously in health, and to call this state *predisposition*, then the malaria is plainly the exciting cause of the ague, in both the first and the second attack. It is allowed that depressing cold, fatigue, or privation, produced the condition of susceptibility in the first attack; the recovery consisted in the removal of this depression of the vital force by good air, good food, and perhaps good medicines; and so the patient *seemed* to be cured. But when the depression was again produced by another exposure to its cause, the malarial agent, whatever it may consist in, being still in the body, again acted as the exciting cause in the second attack. Predisposition, then, is a condition into which a healthy organ may be brought, but which is not disease; the causes which have produced this condition are to be called the causes of predisposition, rather than the predisposing causes of disease.

The expression "Proximate Cause," so often met with in medical books, is a remnant of the scholastic period, which ought to be banished. It is a logical subtlety of no practical use.

The expression "Specific Cause" means that there is an *unknown* something in the cause, as in each of the various contagions. Of these specific causes we are so ignorant that we become aware of their existence only by the violent and often deadly effects which they produce. It is well, especially for students, to remember that many words very frequently used, mean simply "unknown."

Often, the harder the word the less we know about the thing it is applied to. In this way the weakness of human nature is, perhaps often unconsciously, taken advantage of —“*Omne ignotum pro magnifico.*”

III. THE EXCITING CAUSE.

As in man's moral nature there must be the inclination to evil, and also the temptation to it, before evil is committed; so in his physical nature there must be first the predisposition, and then the exciting cause before disease is produced. As in the moral atmosphere temptations are innumerable, so in the material world are the exciting causes of disease; we are surrounded and beset by them. To attempt, therefore, to give a catalogue of them would be in vain. The most practical thing to remember about these “exciting causes” is, so far as we know them, to use our best efforts to avoid them. Each of them acts locally, that is, on some part or parts of the body in preference to other parts. This has been considered in previous Essays.

It is hoped that the subject of predisposition and exciting cause is now clearly stated. In actual practice, however, many perplexities will still arise. It may be of service to give an instance which has happened recently to myself:—For many years I have been familiar with the action of *Digitalis* on my own heart, and especially with the strengthening effect of small doses. Last November (1881) I had, for the first time, an irregular and intermitting pulse; it arose from weakness in the heart's power; this weakness had been increased by a little walking on the

hills at Ilfracombe during September and October. I took *Digitalis*, as I had often taken it before, (the hundredth part of a drop of the sap of the plant for a dose three times a day), not doubting that I should again derive benefit from it. This did not follow. I went on with the medicine for a fortnight. No effect was produced on the heart, but on the kidneys the action was so powerful as to produce almost a suppression of their secretion. This being the case it was prudent to stop. By the advice of my friend Dr. Henry Ramsbotham, I next took *Arsenic* in the second dilution, a drop three times a day for a week, and had at first increase of appetite, then some nausea; after waiting a week, the third dilution was taken three times a day for a fortnight, and this ended in a smart attack of diarrhœa. All this time little or no effect upon the heart was felt. In the middle of December I went to Llandudno, in North Wales, and carefully confined myself to the level parade: still the intermitting and irregular pulse continued, and giddiness, especially on rising from bed, was added. I then thought of leaving off some articles of food, and began with *tea*, which I have taken with great pleasure all my life. Improvement speedily began, and soon after returning home on January 19 (1882), the troubles both of heart and head disappeared—to be brought on again at any time by taking *tea*. The solution of this perplexity appears to me to be this—the heart was depressed beyond its ordinarily low level by a little over exertion; here was the predisposition; the tea was then able, as an exciting cause, to act injuriously upon it, and had sufficient power to prevent the *Digitalis* acting in its own way upon the heart; here were two exciting causes

acting at the same time, and the stronger (the tea in the larger dose) prevailed over the weaker. On leaving off the tea I might have taken the *Digitalis* again, and probably with benefit; but it was more instructive to see first what the withdrawal of the tea should accomplish alone. The giddiness, no doubt, arose from the increasing inability of the heart to send the blood to the brain. This is not a singular instance of the poisonous effects of tea. Sir Thomas Watson relates a similar example, though I did not recollect this till afterwards.

There are other practical perplexities on this subject which a student will meet with, but they will be resolved by patient thought. For example, in cases of complicated disease, he may find it difficult to see an exciting cause for the whole case, as when abscess and caries of the jaw follow toothache. In such a case the exciting cause may be discovered to be a fungus growing from the extremity of the fang of the tooth, and the removal of the tooth will be the first step in the treatment. A remarkable case of this kind happened to me in 1842, in which *two-thirds* of the lower jaw were removed, in a state of necrosis, by a pair of forceps, leaving the entire set of excellent teeth (with the single exception of the one extracted, which had had the fungus attached to it), fast and in their proper places. The young woman's face recovered a perfectly natural appearance.* I have met with other cases of this kind, but none so remarkable.

* This case may be found reported, with an engraving of the bone removed, in the "*Medico-Chirurgical Transactions*," Vol. XXVII., for the year 1844.

IV. THE SEAT OF DISEASE.

What is Disease? Nothing tends more to produce misunderstandings and to prolong controversies than the vagueness which arises from unsettled definitions. It is, therefore, very desirable that these should be settled, if possible; or, if this cannot be, then at least each writer should be careful to give his own definitions, and having done this to adhere to them. Definitions are the meanings of words rather than of things. "A point is that which hath no parts, or which hath no magnitude." "A line is length without breadth." These are the meanings which in Euclid are attached to the words "point" and "line." We cannot form a conception of such *things*. What is wanted in Medicine, as a preliminary to successful progress, is such a definition of each important word as all may understand and acquiesce in. *Disease* is one of these words. Its meaning has not yet been settled. The definitions given of it have been various; the latest is that given us by Dr. Bristowe in the excellent work already alluded to. He says:—"Our conception of a disease is fulfilled only when the *cause* and its *results* are, so to speak, welded mentally into one common whole. And hence disease may be defined as a complex of some deleterious agency acting on the body, and of the phenomena (actual or potential) due to the operation of that agency." I think this definition will not be generally accepted. It is not simple enough. In support of it Dr. Bristowe instances "scabies," "tinea tonsurans," "scarlet fever," "acute rheumatism"; showing that in each of these examples, the cause forms part of

the idea. This is quite true: but it is part of the meaning of the words scabies, tinea tonsurans, scarlet fever, acute rheumatism; it is not necessarily a part of the meaning of the word "disease." The words used as the names of special diseases do sometimes involve in them the idea of their causes. "Scabies" must be defined as including in its meaning both the itching pimples or pustules, and the insect which produces them. "Scarlet fever" embraces in its meaning both the fever with its rash, and the contagion which caused and which accompanies it; so of many other names of special diseases; but the word "disease" must be kept free from this complexity. Other writers might define the word as involving not only the phenomena but also the *seat* of them. Many names of special diseases do involve this; *e.g.* "pleurisy," "pneumonia," "gastritis," "enteritis," "carditis," &c. All these words indicate the seat of the disease, but make no reference to its cause. Again, others might define the word "disease" as involving the character or *kind* of disease. Many names of special diseases do this; *e.g.* "inflammation," "ulceration," "congestion," &c. These words say something about the nature of the disease they refer to, but nothing about its cause. The word "disease" is so extensively used as to require a very simple definition. In order to this, let us first consider what the word is opposed to. This is "health." And what meaning do we attach to the word *health*? We understand by that word a condition of body and mind in which the functions of both can be performed with freedom and vigour, and without pain or suffering. There is, unhappily for mankind, a condition of mind and body which is the

opposite of this, in which the functions of one or the other or both cannot be performed without some impediment, or difficulty, or pain, or suffering. This opposite condition of health I venture to think should be the meaning of the word "disease." And this notwithstanding Dr. Bristowe's alarming denunciation that in doing so, "we simply shirk the difficulty which we pretend to solve." The fact is, that a word is wanted which in its meaning includes *all* ailments, but has not the meanings attached to the names of *any special* ailment; the word most commonly used for this purpose is "disease"; if Dr. Bristowe will deprive us of this word, he must kindly provide us with another.

It is practically useful to consider all diseases (in this wide acceptation of the meaning of the word), at least in their beginnings, as *local*—as having a *seat*. This question has been repeatedly examined in former Essays, and specially, and in much detail, in Essay XVII. The statement is a contradiction of the "Nomenclature of Diseases" of the Royal College of Physicians, and also of nearly all the teaching of the schools. These authorities divide disease into "General" and "Local." It appears to me that when nature is studied rather than books the contradiction will be found to be true. Let each disease classed as "General" in the College Nomenclature be taken separately; let the cases of it coming before the eyes of the practitioner be observed, and their history inquired into; and, I think, the conclusion will be inevitable, namely, that the beginning of every case is distinctly local; that is, some parts are affected, while others are not. The blood is to be reckoned as one of these parts.

Moreover, it has been shown that predisposition is local, and that the exciting causes of disease act locally; it follows as a necessary consequence, that all diseases are at first local. The practical value of this view can scarcely be over-estimated. It clears away an amount of vagueness and uncertainty which will be incredible to those who have not yet taken it, and substitutes for them distinctness and precision. This distinctness and precision will tend to the improvement alike of diagnosis, prognosis, and treatment.

Among the benefits which the sick will derive from the acknowledgment that diseases are local, will be the strong desire which will be excited in medical men's minds, to have a more certain acquaintance with the local action of drugs. If we know that in every case which we undertake to treat, there is an organ, or there are parts, of the body chiefly implicated; if we are convinced that the best treatment is that which is directed to this organ, or to these parts, so that the healthy parts shall not be molested by us; and if, further, we know that each drug has a local action of its own; then, we cannot but wish for such an acquaintance with drugs as shall enable us to choose one which will certainly act where the disease is seated, and not elsewhere. This acquaintance with drugs can be obtained by experiments with them upon ourselves in health.

This subject—the seat of disease—belongs to anatomy; it was considered at some length in Essay XVIII., on “The Anatomical Basis of Therapeutics,” as well as in Essay XVII., on “Organopathy.”

V. THE KIND OF DISEASE.

It has been the lament of our most able and most conscientious practitioners, hitherto, as expressed in the words of Dr. Latham, that "the knowledge of disease is not naturally suggestive of special remedies, which are always hit upon by chance." "We cannot construct aims and indications of practice out of hidden things. We shall perhaps know better some time; but no good will come from our pretending to know better now. Patience under imperfect knowledge is no proof of an unwise mind."*

The plan proposed in these Essays, if earnestly adopted, is capable of bringing about the better state of things so keenly desired by sober and far-seeing minds like Dr. Latham's. But it is a great change, and much time and labour must be devoted to it before its adoption can be general. This is only the seed-time, and those who engage in sowing seed must wait, like the husbandman, with long patience for the harvest. Their patience will have more ground for hope in it than Dr. Latham's had.

Now, it was contended, in the preceding Section, that every disease has a *seat* or local habitation which belongs to it; plainly, our ability to discover this seat in each case will depend upon the extent and accuracy of our knowledge of Anatomy. It is next to be stated that every disease has a *kind* of action peculiar to itself; and plainly, our knowledge of this action will depend upon the extent

* Dr. P. M. Latham's Works. New Sydenham Society. Vol. II., pp. 356, 364.

and accuracy of our acquaintance with Physiology. This statement will scarcely be questioned, but when another is added to it that there is attached to every physiological action the possibility of another action in a contrary direction, a tumult is occasioned, and many murmurs and objections become audible. Nevertheless, the statement is true, and upon its truth rests a possible future practice of medicine.

Each organ or part of the body has a possible double action in contrary directions. It will be granted that each organ or part of the body is subject to disease, that is, to a departure from health; it will have to be granted, by-and-bye, that each organ or part of the body is subject to disease in either of the two opposite directions of its physiological action. The statement has been advanced before in these Essays, but it has always met with opposition. The objections were answered on each occasion. The difficulty in medical men's minds with respect to it is expressed thus:—That blood-vessels may dilate and contract, that glands may secrete too much or too little; that there may be heat or cold, dryness or perspiration, excitement or coma, diarrhœa or constipation; in short, that wherever a *plus* or *minus* state is possible, there may be two diseases of opposite characters cannot be doubted; but how can there be a “contrary” to many other common diseases? What is the opposite of *gout*? and, finally, what is the opposite of *pain*? Let me reply once more to all these objections. Diseases with names like “gout” are compound conditions, which require to be analysed; then it will be seen that each element has an action which necessarily admits of an opposite action. What are the

elements of gout? A first analysis resolves them into swelling, stiffness, redness, heat, and pain, of the big toe; with indigestion, fever, sleeplessness, restlessness, irritability, and what not? If now my objectors will patiently examine (they must not attempt to do this while they are in a fit of the gout)—if they will patiently examine these conditions, they will find that each of them may be analysed and reduced into still simpler elements, and they will be compelled to own that these ultimate elements do admit of contrary conditions or actions. They will find that what they call “gout” is like a bundle of sticks tied together by the name, and that when untied each stick may be bent in contrary directions. But the hardest knot given me to untie is thought to be the question: What is the opposite of *pain*? Well, what is pain? Is it a physiological process or action? No; it is a *result* of an action; it is not the action itself, and it is with the *action* that we have to do; and, plainly, this may be in contrary directions—in one direction it may cause pain, in the opposite direction it may or may not do so. A muscle may be contracted as in spasm, and great pain may result from this, or it may be relaxed, and there may be no pain; a vessel may be distended and there may be pain, or it may be contracted and there may be no pain; a nerve may be compressed and pain result, or it may be stretched and still greater pain may be the consequence. It will be understood that we are engaged in the study of medicine, not of surgery; and I think it must be admitted that for every morbid action a contrary action is possible.

VI. DRUGS ARE REMEDIES FOR DISEASE.

The character of this age, according to an earnest writer, may best be expressed by the word "faithless." Certainly, as regards the medical profession, there is a wonderful amount of scepticism (as well as some credulity) in their thoughts and convictions. Notwithstanding this scepticism, it is not denied that sick people do sometimes recover. The fact is visible in our daily experience. But it is said that these more or less perfect recoveries are brought about by the *vis medicatrix naturæ*, or, in plain English, that they get well of themselves; and that the part which the physician has to play is that of a looker-on, while nature cures his patient.

For the sake of sick humanity, I am thankful to say that the contention in this Essay will be in support of the efficacy of drugs, though not of the present method of using them. It is owned with deep sorrow that, as commonly used, so far from acting as remedies, they often aggravate the sufferings of the sick, they often delay a recovery which is at length effected in spite of them, and they are not seldom the unintentional instrument of death. If drugs are remedies for disease, it may well be asked, How is all this mischief brought about? *By medical men being unacquainted with the true method of using them.* Medicines have been provided for the sick, but, Alas! physicians have not yet learned how to give their patients the benefit designed for them. Hence at the present time a very wide scepticism prevails as to the efficacy of medicines.

For many ages there was an exalted notion of this

efficacy, and, had a true method of prescribing drugs been known, that high notion might have been justly maintained till now; but an excessive use, guided by a mistaken method, has brought them into discredit. One of the first to contend against this hurtful practice was Dr. William Cadogan. In 1771 he published a "Dissertation on the Gout and all Chronic Diseases," in which he labours with great vehemence to destroy the faith in drugs as then used. He condemns medicines in these words:—"The Art of physic has now been practised, more or less regularly, above two thousand years; and most assuredly there is not yet discovered any one certain remedy for any disease. Ought not this," he asks, "*to make us suspect that there is no such thing?*" He closes his book with these words:—"Thus have I endeavoured to set forth the real causes of chronic diseases, and the true principles of convalescence, health, and longevity. If I have hazarded anything new, or contrary to received opinions, it has been from a thorough conviction of its truth, however dangerous to fame and fortune; both which I know are more easily acquired by complying with the world, than attempting to reform it: but it must be somebody equally indifferent to both, as I am, who will venture to tell such truths as are more likely to recoil and hurt the author, than to convince and conciliate the bulk of mankind." This treatise went rapidly through many editions, and the tide on which confidence in drugs had floated for so many ages, began to ebb. When nearly half a century had elapsed, this ebb was precipitated by the blasts of more distinguished men. In 1818, Bichat, in his "General Anatomy,"

said:—"We have yet had in our *Materia Medica* no general system; but this science until now has been successively influenced by those who were leaders in the profession, and each of these has forced upon it his own views. Hence the vagueness and uncertainty which it presents to us to-day. An incoherent assemblage of incoherent opinions, it is, perhaps, of all the physiological sciences, the one which shows plainest the contradictions and wanderings of the human mind. In fact, it is no science at all, but a shapeless conglomerate of inexact ideas, of observations often puerile, of illusory remedies, and of formulas as oddly conceived as fastidiously arranged. It is said that the practice of Medicine is repulsive; I say more than this; it is, in respect to its principles taken from our *Materia Medica*, *impracticable for a sensible man.*"

This condemnation was echoed by many voices until, in 1857, Sir John Forbes, after a practice of fifty years, published his book, "*Of Nature and Art in the Cure of Disease.*" His object is to show that the faith put in drugs is still very much exaggerated; that when patients recover of their diseases it is Nature that cures them, and that often in spite of the damaging interference of Art. The inadequacy or inefficiency of medicines as hitherto given, he points out in a great variety of fervent and eloquent sentences, a few of which are as follows:—"Physicians have prescribed their remedies, for the most part, not because they deemed them capable of fulfilling some rational indication, but on the empirical ground that they were the best they knew for the particular disease under treatment." Again:—"The views given

(in this book) if they do not quite extinguish these notions (of the use of medicines) as altogether visionary, at least reduce them to a very humble and insignificant position amid the rational doctrines of physic. In only a very minute proportion of the numerous diseases presented to us in practice—and these few for the most part of slight importance—are we able to act positively or certainly, that is, directly or specifically, on the diseased part or on its morbid condition; while the whole huge remainder of diseases can, as we have seen, be only indirectly, and distantly, and slightly touched by our agents in any case—and in a large proportion of cases, cannot be touched at all.” And once more:—“In the vast majority of diseases, the medical Art, even when exerting its powers most successfully, can, in strict language, *hardly be said to cure diseases at all.*”

The climax of this scepticism was attained in the article quoted from the *Practitioner* for September, 1881, in Essay XLV. (page 18). From this it appears that the Medical Act is a sham; it stands by and “takes care of, supports, and amuses the patient, while nature cures his disease!” One is tempted to ask, When do sick people send for a doctor to amuse them? Nor can one wonder at the indignation at the condition of the profession described by these authors, expressed by Renouard. “I know,” he says, “of no more revolting position for a conscientious man, nor a more ridiculous one, than that of a physician who has no confidence in the means he employs. Such a man cannot possibly pursue the study and the practice of his Art with the zeal, application, and assiduity which alone can secure him real, honest success.

For in order to study and practice Medicine well, says a wise man of our time (Cabanis), we must give it great importance; and to be able to do this, we must believe in it."

Here, then, is a great dilemma. Until a century ago drugs were thought to have a power little short of omnipotence; now they are thought to have no power at all. So that the conclusion arrived at by Dr. Cadogan in 1771, that we ought to suspect that there is no such thing as any one certain remedy for any disease, seems now to have been fully acquiesced in. Is this a true conclusion? For if it is, it cannot be right to prescribe any medicines. I will steadfastly answer the question with a negative. *It is not a true conclusion.* It is a fact that drugs have failed, in the hands of physicians, to fulfil their expectations; but it has been contended throughout these Essays, that the true conclusion to be drawn from this fact is that *they have pursued wrong paths* in their methods of studying the properties of drugs, and of prescribing them as medicines. *It is a new path which is wanted.* When a better knowledge of the properties of drugs has been learned, a better use may be made of them, and better success may be obtained; in this manner our faith in them will be revived, and a conscientious man may again pursue his calling with confidence and honour. This new method has been sought for and found in these Essays, and a summary of it was given in Essay XLIV. Some further account of it shall be presented in the next Section; but let a few words on the necessity of studying the action of drugs *in health* be premised.

That drugs can cure the sick has come to be doubted,

and even denied ; that they can kill the healthy has not yet been disputed. If drugs have power to destroy life, it cannot be doubted that they have power to injure it in all lesser degrees. For a long time it has been an earnest pursuit of talented men to trace these various injuries with the accuracy required by Courts of Law, in order to throw light upon the many dark crimes which have been committed by the help of drugs. Toxicology has, in some sense, become a science in the hands of men like Orfila, Christison, and Taylor. But with regard to their use as medicines, to the bulk of the medical profession the action of drugs on healthy persons is a new subject. It is, however, an absolute and pressing necessity that attention should be given to it without delay. Until the knowledge to be gained in this way, and which cannot be gained in any other way, has been acquired, the articles in the *Materia Medica* cannot be safely, cannot any longer be honestly, employed as medicines. On hearing this some men will not be ashamed to exclaim :—"Physic for the sick ; I am not going to make myself ill with drugs !" Would that these men could see how selfish such arguments are ! They have undertaken the duty of caring for the sick ; this duty involves the duty of learning how best to help such of the sick as seek help from them ; and now it has been made clear that only by experiments with drugs on healthy persons can we learn how to use them safely and with benefit on the sick. Let it be remembered that it is in the path of duty alone that conscience can be satisfied, that good can be done to our fellow-creatures, and that happiness can be secured for ourselves. Every sick-bed is at this moment uttering a loud cry for better

help; would that every medical man could hear it! Would that he could see that he *must* learn more about the action of drugs in health before he prescribes them as remedies in disease! This duty, though neglected by the majority, is not new; it has been recognised and acted upon by some conscientious and wise spirits in former times. Several of these have been noticed with honour in previous Essays;* one more shall be added. In §. MCCCCXCIII. of Van Swieten's Commentaries on Boerhaave's Aphorisms we have a fine example. Van Swieten is occupied with the treatment of rheumatism with cauteries, and recommending instead of them something milder in the shape of *moxa*, "which does not burn the skin to an eschar." But before he would prescribe it himself, he says:—"I applied a pretty large cone of it to my own thigh, which I lighted at the top, and found that I could easily bear the pain that it caused. I was not disordered, but was willing to *try the experiment on myself before I made it on my patients.*" Doctor, go and do likewise!

VII. HOW DRUGS ARE TO BE PRESCRIBED.

Something was said in the preceding Section on the extravagant and hurtful use of drugs as medicines for many ages until a century ago. But it is scarcely possible for us now to understand the horrible amount of evil which, for so long a period, the prevailing polypharmacy was guilty of. By one part of the profession

* Specially Stöerck (Essay VII.); William Alexander (Essay XXVI.); Samuel Crumpe (Essay XXXII.)

drugs were lavishly prescribed to suit the current theories of disease; and by another part they were given empirically without regard to any theory. As to the theories, nothing has been too absurd to be laid hold of and professed as the true guide to prescribing enormous quantities of medicines; and as to empirical prescriptions, there is scarcely anything left in nature, however repulsive and disgusting, which has not been given as a remedy. Everything has not only been tried, but boasted of as a panacea, and that in all possible quantities, and in all possible combinations. One might be very vocal in lively illustrations of each of these statements, but they are too notoriously true to require either illustration or proof. Only one remark, and this unwelcome subject shall be closed. Casual, arbitrary, and reckless as these prescriptions seem to us to be now, they were thought far otherwise by those who made use of them. This is certain, though to us it may appear impossible. So lately as 1820, about the time when I entered the medical profession, a very learned book was published by Dr. Paris, afterwards President of the Royal College of Physicians, "On the Theory and Art of *Medicinal Combination*," and this "with a view to establish the Art of Prescribing upon Fixed and Scientific Principles." How easily do we impose upon ourselves and others by such words as *fixed and scientific principles*! The extreme of the swing in the direction of a crude and mischievous polypharmacy was then fully reached.

The swing in the opposite direction has now also touched its extreme point. More was said upon this in the last Section. Even Dr. Bristowe, in his very able

book on the "Theory and Practice of Medicine," fully confirms what was said on the loss of faith in medicines now prevalent. He says:—"The great aim of medical art is the cure of disease. Unfortunately, however, a direct cure—at all events a direct cure by means of drugs—in the great majority of cases *is totally impossible.*" "Neither by mechanical measures, nor by specific drugs, nor by the restoration to the dietary of matters in which it has been wanting, can we cure the infectious fevers, internal inflammations, carcinoma, degenerative changes, or many of the functional or other disturbances to which the organism is liable. Most of these affections take a course peculiar to themselves, tending in some cases to ultimate recovery, in some to chronic ill-health, in some to speedy death. We can do little, often nothing, to arrest them in their progress, or to put limits to their duration." Such is the despairing result of all the former and of all the present practice of medicine!

Men have had recourse to medicines ever since they began to suffer from disease; to help them in the use of these medicines there has been in Europe for more than two thousand years a medical profession; and the conclusion this profession has arrived at is that "in the great majority of cases of sickness a direct cure by means of drugs is totally impossible!" Nevertheless, men will continue to have recourse to medicines, when they are sick, in all time to come; and this in spite of the conclusion arrived at by physicians. It is worth while, therefore, to enquire whether a better method of prescribing them may be found out.

The following is believed to be a better method than

any hitherto adopted. Each of the parts of it has been so thoroughly investigated and explained in former Essays, that it would be tedious to do more now than give a summary of them. They may be summed up as follows:—

(1.) The drug must be known; be readily described and recognised; and admit of being obtained in a uniform and stable condition.

(2.) The actions of the drug *on the healthy*, in different doses of it, must be known as accurately as it is possible to learn them.

(3.) The actions of the drug chosen as a remedy must be on the same organs or parts affected by the disease for which it is to be prescribed.

(4.) The action of the larger doses *on the healthy* of the drug chosen must be of the same kind as the action of the disease.

(5.) In the smaller doses the action *on the healthy* must be contrary to that of the disease.

(6.) The dose to be prescribed must be the smaller one.

The practice, then, will be an antipathic one, but such an antipathic one as has never been practised before. The medicines given will always be given in small doses, because these small doses have a tendency to act in the contrary direction to that going on in the diseased part, so far as this is known.

I will now content myself with a few words on the two latest difficulties which have come to my knowledge.

The first was expressed in this question: "Why may not a drug with a known contrary action in its larger doses be given as a remedy in those larger doses?" The answer is a practical one. For many hundreds of years medical men have been working on this plan, and we have seen it, from first to last, an utter failure. Assuredly, it is time that this plan was abandoned, and that too with a determination on the part of all honest men, never to take part in reviving it. To use the contrary action of the smaller doses is quite another plan. There is not only the contrast of doses, but there is commonly also a reversal of the drug. For example: *Opium*, which has hitherto been supposed to be a remedy for excitement and wakefulness, becomes a wonderful remedy for coma, and even for some forms of apoplexy; it will also probably act on the bowels; while the purgatives, commonly given in such cases, will be used in their smaller doses in various forms of diarrhoea. *Belladonna* and *Physostigma* as remedies will change places for their action on the pupil. And while *Aconite* will be prescribed, instead of bleeding, to lower the heart's action, *Digitalis* will be used, in small doses, to strengthen it. And so of the rest. It is so complete a change that it will take a long time for the majority of medical men to become familiar with it. But it is a change eminently for the advantage of the sick, and as such its adoption becomes, not a scientific adventure, but a moral duty. The change involves a preparation of drugs in their simplest, most uniform, and most permanent form; the use of each drug by itself both in experiments on the healthy and on the sick; and for the carrying out of the above method perfectly, it will be

necessary to learn by careful trials with each drug, the range of larger doses which act in one direction, and the range of smaller doses which act in the contrary direction.

It must be understood that among the smaller doses, nothing smaller than the third centesimal dilution or trituration is included. I have not experimented in health with any doses smaller than these. Doses now commonly called "infinitesimal" I have not had time to investigate; respecting them, therefore, I have no opinion to offer.

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The first of these is the fact that the
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ESSAY XLVIII.

WHAT IS ANTIPRAXY?

"Every science is seeking after a Foundation."

PLATO.

ANALYSIS.

INTRODUCTION.

- I. WHAT WE KNOW ABOUT DRUGS.
- II. WHAT WE DO *NOT* KNOW ABOUT THEM.
- III. WHAT ANTIPRAXY IS *NOT*.
- IV. WHAT ANTIPRAXY IS.

ESSAY XLVIII.*

WHAT IS ANTIPRAXY?

"I have no time for any thing that has not a PRACTICAL bearing."
MISS BIRD (*Japan*).

THE first impression intended to be produced by this Paper is that the subject is treated *practically*—without intentional or conscious speculation or hypothesis. Patients are ill, and patients are dying; their cry for help cannot be silenced by speculations; their illnesses cannot be cured, nor their lives prolonged by hypotheses. Theories without truth, and technical language without meaning, are a mockery. What can we *do*? This is the question ringing in our ears. It calls for attention. Listen, then, to this practical Paper.

The next impression which it is desired to make upon my readers is this:—We know nothing *beforehand* of the treatment of diseases by medicines. Our whole knowledge must be got by observation and experiment; and we must take the knowledge which observations and experiments (made, of course, with sufficient care) give us, without hesitation and without cavil—such knowledge is *fact*; and facts may be relied upon. Opinions may vary, but they cannot alter facts.

* Written in 1885.

Another impression, which should grow out of the one just noticed, belongs to feelings and behaviour. It is often a preliminary remark by a Lecturer on Chemistry, that we have no *à priori* knowledge of what will be the result of a new chemical experiment. When Sir Humphry Davy subjected *Potash* to the force of the large galvanic battery he had succeeded in constructing in the Royal Institution, he had no idea what would happen to it. When he saw, for the first time, a new silvery-looking *metal*, lighter than water, appear, it was an intense surprise and delight to him. When he saw this new metal, on being put into water, or laid upon a piece of ice, instantly burst into flame, his surprise and delight were at their height. So is it with experiments with drugs on living man. We cannot even form a conjecture what will happen, when a dose of a new drug is taken in health. Generally, something happens which was quite unexpected. Whatever does happen it is of value to know. Notwithstanding this, when experiments have been made, and results are related, which are unfamiliar and unexpected—new and strange—the feeling in the minds of hearers which often prevails at first, is one of dislike and resistance. The first duty with regard to all such feelings of opposition, is to quell them indignantly; they are base feelings and must be subdued. A more sober feeling will follow, which is to be encouraged: it is the feeling that the statements made are novel, that they are unlike previously received opinions and beliefs; but inasmuch as they are given as the results, not of thinking, but of experimenting, the only way of dealing with them is to repeat the experiments.

Now, let my readers begin with attention, with a consciousness of want of knowledge, and with a spirit of fairness, and what follows may prove worth the time and labour given to the study of it.

To throw as much light as possible upon the subject of this Essay, it is proposed to consider, very briefly, the four following topics:—

- I. What we know about Drugs.
- II. What we do *not* know about them.
- III. What Antipraxy is *not*.
- IV. What Antipraxy is.

I. WHAT WE KNOW ABOUT DRUGS.

Definition of Drugs.—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.*

We know something, though it is time that we knew much more, about the action of drugs; or in other words, of *what* they do. We know that

(1) *They act injuriously in health.*—They are not food; they do not make blood; they disturb the functions, or even change the structure of some organs or parts of the

* The distinction drawn in these Essays between food, stimulants, and drugs, is of great practical utility; though it is true that, in the present state of our knowledge, the line which separates them from each other cannot always be clearly drawn.

body, which were previously in a healthy condition. They are enemies to a healthy man; they hurt him more or less; he should naturally avoid them, as he would avoid any other enemy. Plato dreaded them so much that, even as medicines, he doubted if they should ever be had recourse to. He says, "*in a case of extreme necessity* they may be of use; but in any other they will be adopted by no man of sense." (The "*Timæus*"). Eminent men in our own time, Sir Astley Cooper amongst them, have said as much. Nevertheless, we also know that, sometimes,

(2) *They act curatively in disease.*—That they have power to cure disease is, I think, proved beyond dispute by the cases which have been given in former Essays, and will be given in a future Appendix. That they did not cure in Plato's time, that they do not more frequently cure in our time, arises, not from their want of power to cure, but from *our want of knowledge how to use them aright*. It is this want of knowledge which made Sydenham exclaim:—"There is no proportion between the means of cure and the multitude of diseases!" Sir John Forbes and others, in our day, have in the strongest manner reiterated this.

(3) *Their tendency is to act in the same manner on the sick as on the healthy.*—This, in a rough way, has always been known. The White Hellebore of Hippocrates would purge a healthy man as drastically as it did his patients. Ipecacuanha or Sulphate of Zinc, in certain doses, will make almost any man, whether well or ill, vomit violently. Indeed, it is hard to conceive how it can be otherwise, seeing that drugs are not intelligent beings, or in any way able to distinguish between the

healthy and the sick. That they are endued with some faculty of this kind seems to be almost implied in Hahnemann's axiom, "*Similia similibus curantur*"; and this has been no small difficulty in thoughtful minds. That drugs should produce disorders in health, and cure similar disorders arising from other causes, has been hard to believe. Why they should do so could not be conjectured.

(4) *Drugs act locally.*—Each drug acts on one or more organs or parts of the body in preference to others. It is not possible, I think, for any observant experimenter to doubt this. Oh, that every medical man would try some experiments with drugs upon himself! How much he would soon learn! The difficulty would then be how to think that he had experimented enough. The Profession should not rest till every organ of the body has a list of drugs which it appropriates, and which produce characteristic actions upon it. Opium, Belladonna, Stramonium, and some others, are known to act upon the brain. Aconite, Digitalis, Bovista, and some others, on the heart. Bichromate of Potash works a hole through the septum of the nose. So every part of the body should have its complement of drugs; and every drug should have attached to it the localities of its action.

(5) *Each drug has its own kind of action.*—As every drug has its *seat* of action, so has it also its characteristic *manner* of acting. Only by experiments on the healthy can this knowledge be satisfactorily obtained; but with these experiments, it may be. We want to know, not only in what parts every drug produces its effects, but also what those effects are. We know that Belladonna and

Physostigma both act upon the iris; and that Belladonna, in certain doses, contracts it, and so dilates the pupil; and that Calabar bean does the reverse, dilating the iris and so contracting the pupil. We know that Aconite and Digitalis both act upon the heart; and that, in certain doses of each, their actions are very different—Aconite slowing the beats of the heart; Digitalis quickening them. We want to know of every drug, both its *seat* of action and its *kind* of action.

(6) *Each drug acts differently in different doses.*—With respect to a few drugs we avail ourselves of this knowledge every day. Mercury has, by a great abuse, been given, in one dose, to produce salivation; in other doses, to act upon the liver and bowels, as a purgative. Tartarized Antimony has been given, in one dose, to produce perspiration; in another, to purge; in another, to vomit. But the amount of our knowledge on this subject is very small indeed: so small that we ought to feel ashamed. We ought to know the exact action of every dose of every important drug. This knowledge can be acquired only by careful experiments made in health.

(7) *Some doses of a drug act in the contrary manner to other doses of the same drug.*—It is only very recently that attention has been drawn to this remarkable fact. A few writers in the old school have casually noticed it in regard to a few drugs, but have not drawn from it any practical inference; nor has the enquiry been pursued to discover how many drugs possess the property of acting in this manner. In the new school, it was first distinctly advanced as a universal or law-fact, that is, not only as

belonging to a few drugs, but as being a characteristic property of all of them, in 1873. The name of *Antipraxy* was given to this property. The statement then made has not yet been accepted by either school of medicine, and it is the object of this Essay to invite to it, again, the attention of the Profession.

(8) *The action of a drug is governed by the dose.*—This important fact springs out of the two last stated. Such and such actions cannot be predicated of any drug without reference to the dose. To say that Opium causes coma or constipation is to say only half the truth. The other half is that, in other doses, it excites the brain, and relaxes the bowels. And this, I venture to assert, will be found to be true of every drug. The grounds for this assertion will be found in this and in previous Essays.

(9) *Some drugs do not seem always to act on the same organs in all doses.*—This is the evidence of experiments up to the present time. For example, Tartarized Antimony acts on the skin in one dose; on the bowels in a larger; on the stomach in a still larger; and on the lungs in the largest. But this knowledge must be considered tentative and preliminary, to be confirmed or corrected by further experiments. It is not yet seen to apply to all drugs; it is, therefore, indefinite, and “that which is indefinite cannot be reduced to science.”

(10) Lastly, we now know *why the small doses of the homœopathsists have been so successful.* That they have been successful, I think no unprejudiced person will deny. The provings of drugs in health have been made with all doses, large and small. They have

been given as remedies in the small doses only. When these small doses have cured, it is because they have been given for disorders similar to those produced by the larger doses of the same drug, and because they have acted contrary to the disordered action going on. This is the only "Why?" which has yet been answered, in respect to the operation of drugs on living man.

II. WHAT WE DO NOT KNOW ABOUT DRUGS.

It has been remarked that the important matter is not, to know what is untrue, but to know what is true; and it is a remark which cannot be too well remembered. But in respect to our present subject, it is important to know our ignorance as well as our knowledge. Books and Lectures on Medicine are so much occupied with teachings about drugs which are fanciful and untrue, that it is necessary to sweep away these worse than useless encumbrances, so as to clear the path and make room for true and useful knowledge. We have no knowledge respecting the following questions, and cannot answer them:—

(1) *Why do drugs act injuriously in health?*

A few grains of Opium will give us an apoplexy; a few grains of Colocynth will writhe us with colic; if we eat Aconite root at dinner instead of Horse-radish root, we are killed outright; a few grains of Ipecacuanha will make us sick; the same quantity of Arsenic will slay us. These facts are full of wonder; they are known to all; but of the *why* we know nothing.

(2) *How do drugs act?*

We know nothing of the manner of action of drugs. Drugs can do any kind, and any amount of mischief; they can, and often do destroy life. *How* they do this we are entirely ignorant. "Dr. Sharp! you are quite wrong in saying this. Don't we know that Opium causes apoplexy by congesting the veins of the brain? Don't we know that Arsenic kills by causing gastritis and enteritis? We know a great deal about the *modus operandi* of drugs." Alas! how sad it is to be blinded by knowledge, so that we cannot see that this knowledge, however useful in its way, only alters the *form* of the question. That, *in certain doses*, Opium does produce congestion, and Arsenic inflammation, we are thankful to know, and we can turn this knowledge to most practical use—even to the saving of life: but *how* does Opium cause congestion? *how* does Arsenic cause inflammation? Of this we are profoundly ignorant. The question remains in all its force, and has not been answered by this shifting of its form. *How* does Ipecacuanha vomit? or Senna purge? *How* does Belladonna dilate the pupil? or Cantharides inflame the bladder?

(3) *Why does a drug act upon one part of the body, and not upon another?*

That drugs act locally has been shown. *Why* they do so we have not the smallest notion. It is a wonderful fact; practically, of the greatest use; but entirely without explanation by any knowledge we possess at present. *Why* does Opium act on the brain and bowels? or Arsenic on the stomach, small intestines, and rectum, passing by the colon? *Why* do Aconite and Digitalis act on

the heart? or Bryony on the muscles? or Nux Vomica on the spinal cord? or Chamomilla on the liver? or Rhus on the spleen? or Aloes on the rectum? or Symphitum on the bones? There is no reply.

(4) *Why does one drug act upon one part of the body, and other drugs act upon other parts?*

This is another wonderful fact, and one also of the greatest practical use. The examples just given of local action are also examples of this difference of selection. But we know no facts from which to learn an explanation. And to conjecture, or invent hypothetical explanations, is to raise the greatest barrier against progress in knowledge.

(5) *What determines the kind of action of drugs?*

We have seen that each drug is distinguished by having a seat or locality for its action; and by having a kind of action of its own. These are facts of the highest practical value, and lie at the foundation of scientific medicine. How this is brought about, what fixes the seat, and what determines the kinds of action of drugs, we do not know. We cannot even form a working hypothesis to help us to discover such subtleties of nature as these.

(6) *When several drugs act upon the same part, why has one of them one kind of action, and the rest have other kinds?*

That each drug has its own kind of action is well known. Bovista, Spigelia, Lachesis, Cactus, as well as Aconite and Digitalis, act directly upon the heart, but each in its own way, and that way differs from the way in which the others act. Why? We cannot tell.

(7) *Why do some drugs act so much more powerfully than others, as to compel great differences in the doses?*

The fact here noticed has been a difficulty and a reproach in medical treatment to the present hour. Posological tables have many times been compiled, but to little purpose. Some have given up the subject in despair, and have said, "Let me know the remedy, and I will not care about the dose." There could scarcely be a greater mistake. The dose is as important as the drug. Why the dose of one drug should be ten times, or a hundred times, larger than the dose of another drug, we are unable to say.

(8) *Why is the action of drugs governed by the dose?*

Though we know nothing of the manner in which the action of drugs is brought about, we every day see that, to a great extent, it is governed by the dose. Why it should be, we do not know.

(9) *Why does the same drug act in one way in certain larger doses, and in the opposite way in smaller ones?*

This also is a marvellous fact; and what an advantage in the treatment of the sick to know it! Nothing half so precious or useful was known about drugs before. That Aloes and Ignatia act on the hæmorrhoidal vessels of the rectum cannot reasonably be doubted; that large doses of either of them cause hæmorrhoids, and that small doses cure them, is beyond question. A short time ago I had, for the first time in my life, a pile. How it came, or what it was caused by, I do not

know; but it was very painful, excessively tender to the touch, and about the size of a large pea. There had been no previous irregularity of the bowels, and I had taken nothing likely to cause it. I took *one* drop of the first dilution of Ignatia, (that is, the hundredth part of a drop of the tincture). This was speedily followed by relief of the pain and tenderness; in three days the tumour was shrivelled up, and all trouble from it gone, and did not return. I must believe this as *fact*; but I cannot imagine, much less indicate, any explanation. This is *Antipraxy*, and we are now prepared to consider the answer to the question at the head of this Paper. It will help us if we first ascertain what it is *not*.

III. WHAT ANTIPRAXY IS NOT.

1. *Antipraxy is not a revival of any former rule of contraries.*—The first rule of *contraries* is so old that it was criticised and condemned by Hippocrates in his “Treatise on Ancient Medicine.” It is the hot and cold, and the dry and wet, which, five centuries afterwards, was so strenuously and successfully advocated by Galen, that it reigned over European medicine till modern times. It has been so fully considered in Essay XIX. that nothing more need be said of it now. During the last two centuries the axiom *contraria contrariis curantur* has assumed several other forms. I excuse myself from entering into these here because they have been

sufficiently dealt with in previous Essays, specially in XIX. and XXV. Antipraxy is distinct from all these.

2. *Antipraxy is not Homœopathy.*—Hahnemann did much in the way of experiments with drugs, on himself and many friends, in a state of health. He collected all the symptoms produced by each drug, in all the experiments tried with it in very different doses, large and small, and arranged them under the different parts of the body. These symptoms were, all alike, to be guides in prescribing the drugs for diseases in which the symptoms were similar. In this collection of symptoms there is no distinction of the effects produced by different doses. Whether the symptoms followed the taking of a large dose or of a small one, they were equally to be viewed as “similar” to any patient’s suffering.

My first experiment to test Hahnemann’s axiom on the curative action of drugs was in the case of a little girl, whom I was asked to see in 1850. For a week she had been vomiting after every thing given to her, either as food or medicine, and, as might be expected, was in a very prostrate condition; she lay helpless in bed. I thought of *Ipecacuanha*, which I was certain would make such a child as this, if given to her in health, violently sick. I put one drop of a tincture of it into a tumbler half full of water, and gave her a teaspoonful, telling the mother to give her a similar dose every time she vomited. The next day, to my surprise, I found the little girl dressed and down stairs, and on asking how often she had been sick since my first visit, her mother told me that she had not been sick again, had had no more medicine, had asked for her breakfast and had

taken it. The child was well without further treatment. Here we see Ipecacuanha, which makes a healthy person vomit, curing this child of vomiting. This is what Hahnemann meant by Homœopathy—"like curing like."

In these experiments—a drug given in health and in sickness—there are two sides to be looked at. If we look at the side of *the persons experimented on*, we see a healthy girl made to vomit by Ipecacuanha, and a sick girl cured of her vomiting by the same drug. The drug is not changed, but the condition of the person experimented on is changed. The same drug cures a disease like that it causes in health; this is the meaning of the axiom, "likes are cured by likes."

If we now look at the side of *the drug* with which the experiments are made, we see that though the drug is not changed, there is a great change in the dose of it; so that the comparison between the two experiments, which we have just now made, is broken. To prove the axiom of *similia* it should not only be the same drug but the same dose. Half a drachm or more of Ipecacuanha wine may be required to make a child sick, whereas a child already sick is cured by a fraction of a drop. Here we cannot but notice the contrary action of large and small doses of the same drug, and the axiom from the experiments becomes reversed; it is now "contraries are cured by contraries." The similarity is limited to the action of the drug in its larger doses, and these larger doses would not cure, but kill.

3. Antipraxy is not hypothesis.—The truth of this statement depends upon the meaning of words. The

unsettled and capricious use of words, by which their meaning is changed, is one of the many troubles and difficulties which a writer has who wishes to be understood. The word *hypothesis* has been so widely extended in its meaning by some recent authors, that it has been applied to all the known laws of nature outside pure mathematics—to gravitation, and even to the law of uniformity itself. All these are now spoken of as being nothing more than “working hypotheses.” This liberty introduces a confusion into the discussions of natural science much to be regretted.

Hypothesis—“a supposition; a system formed upon some principle not proved.” From *ὑποτίθημι*, “to presuppose.” This was the meaning formerly attached to the word, and to me, it expresses a form of *conjecture without proof*; so that when proof comes, it ceases to be hypothesis and becomes fact. These proofs ought not to be confined to mathematical demonstration; there are other forms of certainty; and an induction from individual facts, often called a law of nature and in these Essays a law-fact, when made with the necessary care, I take to be a sufficiently proved fact, and accept such facts as true. They are inductions from phenomena; they are not imaginations of the mind.

Of Antipraxy I have been asked this question: “Is this known by experiment with *every drug*, in which case it is the short-hand statement, 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 is not an *enumeratio simplex*; but it is experimentally

known of so many drugs that it is, I think, safely concluded of them all. I call this not a hypothesis, but a fact. If some will persist in calling it hypothesis, I can only say I am sorry; at the same time they may be reminded that if they call it a "working hypothesis," seeing that the interests of the sick are so deeply concerned, it is their duty to "work" with it for the benefit of the sick.

It has also been asked: "If a drug did not conform, would it drop out as useless?" If the substance, whatever it is, does not conform to the *definition*, it is *not a drug*. If it does conform to that it need not be doubted that it will also conform to Antipraxy. The story about Castor Oil, given in Essay XXXI., is a case in point.

I venture to take my stand besides Sir Isaac Newton, who closes his *Principia* with these words:—"Hitherto I have not been able to discover the cause of these properties of gravity from phænomena, and I frame no hypotheses; for *whatever is not deduced from the phænomena is to be called an hypothesis*; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy *particular* propositions are *inferred from the phænomena*, and afterwards *rendered general by induction*. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered. And to us it is enough that gravity does really exist, and act according to the laws which we have explained."

Antipraxy has been "inferred from the phænomena" exhibited by individual drugs, and "afterwards rendered general by induction." It is enough that the action of

drugs on the living body "does really exist;" and that, among the laws which govern it, Antipraxy is one. No doubt, there is a difference between a mathematical verity and a physical one; *demonstration* is the word for the first, *hypothesis* is *not* the word for the second. There are intellectual certainties which are not mathematical.

Plato, in his "Dialogue of Timæus," has a prayer to God begging Him to rescue them (himself and the friends to whom Timæus is speaking), from their strange and unwonted enquiry into the Creation, and to bring them to *probability*. For phenomena, he says, are to be reasoned out by the *method of probabilities*. His reverence for God forbids him to go further than observation and thinking. He says: "He who should attempt to *verify all this by experiment* would forget the difference of the human mind and the Divine nature." Hence this Dialogue is a surprising combination of the power of thought with the absence of knowledge.

Lord Bacon, on the other hand, with not less reverence for God, encourages us *to experiment*. For without this "verification," hypothesis must usurp the place of knowledge. His advice has been followed till, I think, it has in some directions (as in "vivisections") been carried beyond rightful limits, but we are indebted to it for the greater part of our present knowledge.

Of late this knowledge has been attacked in a new direction. Some eminent men would bring us back to Plato's probabilities, and would have us think that, even with experiments, we cannot get beyond it. They argue that, as mathematical demonstration cannot be applied to physical science, there can be no certainty

in it. I venture to differ from them, and to believe that certainty may be arrived at by observation and experiment.

To bring these thoughts home to our present subject, it may be remarked that a large number of experiments with drugs, both on healthy and on sick persons, have been made, and some inductions have been drawn from these experiments which may confidently be relied upon as universal or law-facts. If the certainty which attaches to these inductions is, nevertheless, to be theoretically questioned, it must be so; but it is, at any rate, a certainty *sufficient for all practical purposes*, and the sick ought to have the benefit which can be got from it for them. This is a responsibility which physicians cannot now lawfully shrink from. We know that it has not been arrived at in the same way that mathematical conclusions are obtained, but it cannot, on that account, be reduced to mere probability. It is something very different both from Plato's probability, and from the legitimate meaning of that word in modern thought.

4. *Antipraxy is not Allopathy.*—It has scarcely a feature in common with "orthodox" practice. This orthodox practice is very chameleon-like. It is always changing, and not unfrequently passes from one extreme to its opposite. When I was young I was taught that bleedings, or drugs, or both, were essential to a recovery from any illness. At that time the vulgar doggerel of Dr. Lettsom was not thought barbarous:—

"Is people sick, they comes to me;
I physics, bleeds, and sweats 'em."

Now that I am old, I am instructed by one of the most

popular teachers, that "The great aim of medical art is the cure of disease. Unfortunately, however, a direct cure—at all events a direct cure *by means of drugs*—in the great majority of cases is totally impossible. . . . Neither by mechanical measures, nor by specific drugs, nor by the restoration to the dietary of matters in which it has been wanting, can we cure the infectious fevers, internal inflammations, carcinoma, degenerative changes, or many of the functional and other disturbances to which the organism is liable. Most of these affections [and it will be seen that they form the great bulk of diseases] take a course peculiar to themselves, tending in some cases to ultimate recovery, in some to chronic ill-health, in some to speedy death. *We can do little, often nothing*, to arrest them in their progress, or to put limits to their duration. And frequently all that remains to us is, by maintaining the patient's strength, by relieving symptoms, and by taking precautions against the supervention of complications or accidents, to enable him to pass with comparative safety or comfort through his malady—hastening convalescence if the disease be one that does not necessarily end fatally, postponing the final issue if the disease *be in the nature of things mortal*."* An echo of Sir John Forbes.

I venture to think that this is not merely, like the former, a mistaken extreme, but that both are dangerous errors; and am doing all in my power to call the attention of my Profession to a method, which differs greatly from all former methods. The difference is not

* Dr. Bristowe's "*Treatise on the Theory and Practice of Medicine*," p. 121 (2nd Edition).

in the outward mode of acting, but in the inward mode of existence. It is the essential difference between the *σχῆμα* and the *μορφή* of the Greeks—the outward “fashion” and the inward “form.” The change advocated in these Essays is not a change of fashion, but of foundation and principle. The prospect held out by this change is a success in the treatment of the sick, which has hitherto been unattainable.

There is a rival candidate for this distinction. “Never,” said Professor Tyndall, in a lecture at the Royal Institution (on January 16, 1885), on “The Germ Theory of Disease,” as illustrated by the researches of M. Pasteur—“never in the history of medicine was there so bright a dawn for the healing Art, as the present one, if scientific men are not hampered by mischievous legislation.”

A much brighter forecast might be entertained, from the point of view of these Essays, were there not more formidable antagonistic forces at work against it, than “mischievous legislation.” What these opposing forces are will be discussed, if we may come to “The Difficulties of Antipraxy.” A breach has been made in the bulwarks of orthodoxy; it has to be entered by the forlorn hope; and the issue left to God.

5. *Antipraxy is not necessarily connected with disease.*—It will help to clear the subject if this aspect of it is considered. Let it be remembered that the question is—What is the action of drugs upon a living man?—The answer can be learned only by experiments on man in a state of health. Some experiments have been made in health, and the contrary action of certain larger and

smaller doses of drugs has *in this way* been discovered. This being so, it is evident that disease has not yet put in an appearance. It is not one of the elements in the problem. It follows that any remarks in the form of criticism or opposition, which are not limited in the same manner, to experiments on the healthy, are not in the least relevant; they are impertinent and out of place.

6. *Antipraxy is not necessarily connected with the use of drugs as medicines.*—This statement is complementary to the last; it is a necessary consequence of it; nevertheless, to call attention to it is imperative. It is surprising how habitually it is overlooked by those who undertake the office of critic. It would be a happiness if all former uses of drugs could be forgotten—could be obliterated from the minds of physicians, and patients, so that a new start could be made. It would greatly facilitate the investigation, and hasten the adoption of the true use of medicines. Alas! this is impossible, and order must be got out of chaos as best it may. Viewed in the light of Antipraxy all drugs are poisons, and are to be experimented with as such.

7. *Antipraxy does not include infinitesimal doses.*—Another statement which rests upon the meaning of a word, and calls for a definition. What is *infinitesimal*? In these Essays it means something less than the millionth part of a grain or drop. Appreciable doses are *not* infinitesimal; and it has been shown in former Papers, that the first, second, and third dilutions or triturations—that is, divisions of a grain or drop into a hundred parts in the first, into ten thousand parts in

the second, and into a million parts in the third of these preparations—are appreciable, can be recognised by one or more of our senses. This may not as yet be possible of every drug, but if it can be done with some, the fact of the presence of the remaining ones need not be doubted.

A division of this kind, between appreciable and infinitesimal doses, must, unavoidably, be more or less arbitrary; but, at any rate, here is one reason given for the line of separation. Another may also be mentioned, namely, that my experiments with small doses in health have not been carried further than to the third dilution or trituration. Antipraxy, therefore, as at present known, does not include infinitesimal doses.

8. *Antipraxy does not include all appreciable doses.*—As there is a limit to the descending scale towards infinitesimals, so there is also a limit to the ascending scale towards large quantities. The boundary line, in both cases, is the extent to which experiments in health have been carried. It would be quite contrary to the principles of this enquiry to predicate any thing of doses which are beyond the extent of the experiments hitherto made. They are left as work for future research. It is hoped that it will be clearly understood that the present limits of Antipraxy, expressed by the words “certain larger and smaller doses of the same drug,” are not necessarily the natural limits; they are only the present limits of the experiments. It is quite possible that the law may extend much further; and it is not only possible but probable, that itself forms a part of a much larger law, to be discovered, some day, by a wider generalisation.

IV. WHAT ANTIPRAXY IS.

9. Antipraxy expresses the fact that *certain larger and smaller doses of the same drug act in contrary directions*.—It has been shown that this contrary action of larger and smaller doses of the same drug is independent of disease. Neither has it any necessary connection with Therapeutics. A healthy man takes a grain of Opium; he is put into an apoplectic sleep, and his bowels are confined. At another time he takes a few doses of the hundredth part of a drop of Laudanum, or perhaps one dose of half a drop; his brain is excited, and his bowels are slightly relaxed. Another healthy man takes half an ounce of Castor Oil; he is purged. At another time he takes a few doses of the fiftieth part of a drop, or perhaps, one dose of a drop; constipation follows. I have taken when in health, one drop of a strong tincture of Aconite; and the beats of my heart have been made much more frequent. At another time I have taken the hundredth part of one of these drops; and the beating has been made slower. At one time I have applied a strong tincture of Belladonna to the eyebrow; and the pupil has been dilated. At another time, a very weak tincture; and the pupil has been slightly but sensibly contracted. This is Antipraxy. The word has been chosen to express all these and a multitude of similarly contrary actions of different doses of the same drugs.

Antipraxy is an induction, according to the law of uniformity now acknowledged by all students of physical science, from a large number of such individual facts as

these, all being the results of experiments with drugs made in known doses on healthy persons.

We have seen that this induction has been questioned, and has been declared to be a hypothesis only. I think it is worthy of a title of more certainty. Truth has reference to very different subjects, and has to be reached for each of these subjects in its own way. The truths of magnitude and number are reached by mathematics; historical truths are taught us by testimony; revealed truths are understood and accepted by faith; scientific truths are arrived at partly by the evidence of our bodily senses, and partly by an operation of the mind. When the evidences are clear, and have been distinctly placed side by side by the mind, these scientific truths may be accepted as certain. This has been contended for sufficiently, and will now be taken for granted.

Antipraxy is one of these truths of science. Experiments with drugs in different but known doses in health, give the evidence; the mental process called induction makes known to us what we call a general or universal law-fact.

All these different kinds of truth, when supported by their own appropriate evidence, are beyond mere probabilities. For all the practical purposes of life they may be accepted and relied upon as certainties. They differ greatly from hypotheses.

A question will arise in the mind of every one who thinks about Antipraxy, which requires an answer. What about the dividing line—the line which separates the larger doses which have their action, from the smaller ones which have the contrary action? Is the division a

sharp one, and between two doses? Or, is there a central region, where the doses have no action at all? This question presented itself to me on the first glimpse of these contrary actions. I said the question cannot be answered by speculations, only by experiments. These were undertaken, and the result was that there is a middle region—not of no action at all—but consisting of a group of doses which have *both* actions in succession. These doses act first in one direction, and then in the opposite one. In Essay XXVIII., on “the Limits of Antipraxy,” the following illustration of this double action is given:—

“Aconite, in its first dilution (when taken by myself) first, for a moment quickens the pulse, then, for a much longer time depresses it. Here is a primary and a secondary action, or as some call it, action and reaction.”

“Aconite, in a larger dose (a few drops of the strong tincture), depresses the pulse for a moment, then quickens it for a much longer time. Here also is a primary and a secondary action, or, action and reaction.”

10. *Antipraxy is a practical basis for Therapeutics.*—The value of a law-fact is its pregnancy; this is seen in the grand extent of its practical application. Look at the practical application of such law-facts as the law of gravitation, or the law of chemical combination, or the laws of electricity, or of mechanics. The use of the balance in experiments with drugs, first on the healthy, and afterwards on the sick, has led to the discovery of a fact so suggestive, that it promises to be the one rule of Therapeutics, as pregnant of practical consequences, as the law of gravitation, or that of chemical combination has proved to be.

It is likely that this practical rule will, for a long time, be neglected, or even ridiculed, by the great multitude of medical men, who dare not tread one step out of the conventional path. But we are assured that, sooner or later, truth will prevail. The great impediments to the acceptance of the rule are, unwillingness to take the trouble to investigate a new claim; the governing power of self-interest; the absence of moral courage; the prevalence of jealousy; inability to *think*; and, specially, the slavish submission to human authority and fashion. This last is nearly as universal, and quite as contemptible in this nineteenth century, as it was in Egypt in the days of the Pharaohs. It is well known that then, if the priest treated a sick person according to the prescribed rules, and the patient died, as must commonly have happened, the priest was free from all blame—the death was orthodox, it was the event to be expected; but if he ventured to think for himself, and so be led to use some other method, no matter how many sick were recovered by it, if one died, he was held responsible for the murder, and suffered accordingly.

So it is still. This very argument was urged upon me by an eminent divine, although he had experienced in his own family, the great superiority of the new treatment, in order to show me what I must expect by perseverance in leaving the beaten tracks of orthodoxy and authority.

Nevertheless: through a long series of years, the practice has been persevered in, and though a large amount of the obloquy threatened me has had to be borne, success has been attained. Abundant evidence from the

action of small doses on the sick has been found, confirming the experiments on the healthy, and proving, in an independent manner, the truth of the fact expressed by Antipraxy. For the greater part of this time the practice was guided by the vague principle of Homœopathy: but the results are not, on that account, less decisive evidence in favour of Antipraxy.

A good many years ago, it was shown in much detail (Essay XVII., on Organopathy) that every drug has a locality in the living body of man, in which, by preference, its action takes place. This law-fact was recommended as the rule *for the choice of the drug*. The action of the drug prescribed should be, as nearly as can be ascertained, in the organ or part of the body where the disease is going on. The forcible words which a distinguished patient once used to me, should be true of any medicine given—"it went bang to the spot."

The place of action is the first consideration. The next is the kind of action. For a drug to be the true medicine, it must act contrary to the kind of action going on in the diseased part. Not only the drug, but the dose also, must be found which will do this. The only way to discover these is by experiments in health, not only with different drugs, but with each drug in different doses. This having been done, and the contrary action of larger and smaller doses discovered, Antipraxy becomes the rule *for the choice of the dose*.

It is obvious that, with this knowledge, a physician may prescribe the larger doses, so as to utilize *their* contrary action to the disease; for example, he may give the larger doses of Opium, to send an excited man

to sleep; or he may give them to another patient, to restrain a diarrhœa; and in both cases have a world of trouble for his pains. Or, he may give the smaller doses of Opium to an apoplectic man, or to one suffering from constipation, and be rewarded with a success which will make him happy.

It is the contrary action of the *smaller doses*, which is advocated here; and in the Appendix, when published, will be found a large number of representative Cases, which have been treated in this manner.

11. Antipraxy *explains the success of Homœopathy*.—"Every true thought contains a deeper meaning, and involves more important consequences than can be seen at once."* Homœopathy was a true thought; but it was vague and indefinite, and therefore readily despised; but being true, it contained a deeper meaning. This deeper meaning is made manifest in Antipraxy. The contrary action of larger and smaller doses of the same drug is a clear and definite expression. It is the vision of twilight now seen in sunshine. It is the effect seen in its cause. As often happens in such a change of circumstances, the dogma is reversed; *similia similibus* has to be exchanged for *contraria contrariis curantur*. Still, it must be remembered that *this contrary* is not revived in any ancient sense, but has a meaning which it never had before, and which, henceforth, it must exclusively retain.

The practical success of Homœopathic treatment, during the last thirty years, has been so conspicuous

* W. Robertson Smith.

that no one will venture to deny it. This success has been accounted for in various ways, which do not deserve to be recounted now. Antipraxy is the true and satisfactory explanation. The symptoms given in the records of the proving of drugs on the healthy, and which the homœopathic practitioner takes for his guide in the selection of his remedies, are, for the greater part, the symptoms produced by the larger doses. The drug is chosen because the symptoms which the patient is suffering from, are found in its provings; it is given in the smaller doses; the patient commonly recovers quickly—sometimes marvellously; the action of the drug so given is believed to be similar to that of the disease going on. There is no consciousness in the mind of the prescriber of any difference of action between the larger and smaller doses; the effects of the smaller doses, when these have been taken by provers, are inserted along with the effects of larger doses; and no notice has hitherto been taken of any reason why they should not be so mixed together.

It will now be seen, I think, very clearly, that when a cure has been accomplished by this practice, the reason has been that the small doses have cured by their action being *contrary* to that of the disorder. Medical men *think* the action is *similar*, because they find the similar symptoms in the provings, not having observed that these have been the symptoms produced by the larger doses. Their *thinking* this has been no impediment to the cure, because natural actions go on in their own way, whatever we may be thinking about them.

That Antipraxy explains the success of Homœopathy is, surely, now so evident, that no further elucidation of

it can be necessary. It also explains some of the failures which occasionally occur. If the symptoms, which have led to the choice of the remedy, have been produced by the *same* doses as the doses prescribed, their tendency would be to aggravate, not to cure, the patient who takes them.

12. *Antipraxy is an intelligible rule for all.*—The present conviction of eminent medical men is this:—that drugs, as hitherto given, have done more harm than good.

Everyone knows that they have been given universally in the large doses. This mode of giving them, therefore, ought to be abandoned; attention should be given to the small doses, and their effects be learned by a practical trial.

We have seen, in this Essay, that whatever effects *any dose* of a drug produces, when taken by a person in health, it will produce, or, in the face of hindrances, will attempt to produce, when taken by a patient. This, to start with, is a very intelligible fact.

It is also, I think, certain that there are smaller doses which always act in health, and always act, or tend to act, in disorder, in the opposite direction of the action of larger doses of the same drug. This too is a very intelligible fact, however unable we may be to explain it.

Guided by these two facts, Antipraxy becomes, to any instructed physician, a very intelligible rule for prescribing. It is not like theories of disease; these, however plausible, may be easily misunderstood, especially in their application to individual cases; directions for the use of medicines dictated by such theories, when they

come to be interpreted at the bedside, are unavoidably obscure and uncertain; hence practice, even in able hands, is almost always lame and impotent for good, and often does a great deal of harm.

When the two facts just stated are kept in mind, and studiously made the guide of practice, there may frequently be a wish for more distinct and detailed knowledge of the actions of individual drugs, but there cannot be either obscurity or doubt as to the use to be made of the knowledge of these drugs, which we actually possess. Nor will any previous opinions or practice interfere with this clearness; whatever these may have been, Antipraxy, and how to take it as a Rule of prescribing, cannot but be intelligible to all.

Some time ago, the movements of magnetism gave us the mariner's compass; how navigation and commerce have since been developed, every one knows. Very recently, the movements in contrary directions of electro-magnetism gave us the galvanometer; what has the electric telegraph done for us since?

Truly, these are only analogies; but they are instructive and mightily encouraging. Here is another motion in contrary directions; it is put into the hands of physicians; it is very intelligible; it is not more difficult to carry out in practice than either of the other two. Why may not great and most beneficent results to mankind be anticipated and realised from its general adoption?

13. *Antipraxy is yet imperfectly known.*—That is, in its details and practical application. That this must be

so it is sufficient to say that, up to the present moment, so far as I am informed, the development of Antipraxy, and its practical application, have been confined to my own hands. It is enough for one man to initiate a new movement; it is not possible for him to perfect it. Twenty thousand medical men may work at it, in this country alone, and every year may carry on the improvement of its details; and every one who contributes something will deserve "honourable mention." Even after this, though, doubtless, these improvements will be very great, perfection will not have been attained. Other nations will help, and further progress will be made. As with every other branch of physical science, there will always be room for improvement.

14. *Antipraxy is already very successful.*—Notwithstanding what has just now been said, much success has been achieved. This success is not personal boasting. I may be the only one who has seen and recognised the working of this contrary action of small doses; but whether others have seen it or not, Antipraxy really claims as its own all the success of genuine Homœopathy. This has been explained before, and the explanation does not need repetition.

15. *Antipraxy may be adopted by those who have rejected Homœopathy.*—Inventions are commonly found in one of two ways; either by accident, as James Hargreaves, more than a century ago, got the Spinning Jenny (called after his wife); or by hard labour and indomitable perseverance, as Bernard Palissy, more than

three centuries ago, succeeded in finding his method of producing the whitest enamel. Discoveries in science are commonly made in the latter of these ways—by industry and perseverance. Many examples of this might be given, perhaps no greater one than that of Kepler. Sir John Herschel (there can be no better judge), speaking of *inductions*, when verified by a *deductive* process, turning out to be rigorous laws of nature, says:—“The finest instances of this kind are the great laws deduced by Kepler, entirely from a comparison of observations with each other, *with no assistance from theory.*”

These laws of Kepler were stepping-stones for Newton, himself another eminent example of industry and perseverance. It is in this way that men enter into the labours of others. It is in this way that medical men, whenever they are willing, may enter into mine. For thirty years and more, I have travelled laboriously through the dark tunnel of Homœopathy, and have come out into daylight with Antipraxy in my hand. This can now, be, not only understood, but be taken hold of, and worked practically, by the entire medical profession, without obliging one of them to follow my toilsome journey through the before-mentioned tunnel. This is no idle boast; it is the plainest fact, the simplest common sense. An octogenarian will not live to see it done, but he cannot doubt that, sooner or later, it will be done. The wants and the sufferings of the sick cry aloud for better help than doctors have yet been able to give them.

16. Antipraxy is true Antipathy.—It is only by precept upon precept, that any impression can be made;

it must, therefore, again be repeated that all the former meanings and uses of the famous dogma of "contraries" are to be rejected; and further, that Hahnemann's "similars" no longer need to be contended for; and that a new meaning is now attached to *contraria contrariis*, and that this meaning is true science, and the foundation of successful practice.

It will be exclaimed, then Homœopathy is given up. Every honest mind will at once reply:—this is not because Homœopathy has failed, for it has succeeded; but because a practical study of it through many years, has led to the discovery of something still better.

All former therapeutical doctrines and dogmas have rested on theory. The allopathic on theories of disease, the homœopathic on a theory of cure. It is high time that we have done with theories, and that our principles and rules should rest upon facts. As I have often said before, the treatment of the suffering and the dying is too grave a duty for speculation: it ought to be guided, if such a thing is possible, only by facts.

17. *Antipraxy is a guide to the choice of the dose.*—It will be granted, I think, by everyone, that the only object of giving drugs as medicines is, if possible, to counteract the morbid process going on in the patient, and so to restore him to health. It will, I hope, also be granted that this ought to be done, if it can be, without doing harm in other ways.

It is evident that any drug, whose hurtful action on the healthy body, and the varieties of this hurtful action belonging to its different doses, are known, may be given

in any of these doses to counteract a disease whose morbid action is, in some sense, also known to us. Any dose, large or small, may be so prescribed.

But it is known to us by centuries of experiments on the sick, that when the large doses are given, even if an arrest of the morbid action is obtained, there are other effects of the dose which are hurtful. It is now known by many experiments during the last half century that, when the small doses are given, in cases the opposite of those for which the large doses have been hitherto given, the morbid action is commonly arrested much more effectually than before, and no hurtful effects accompany this beneficial one.

These are the facts: it follows that it has become a duty to abandon the large doses, and to prescribe the small ones. To help us to adopt this great change of practice, we have a perfect guide in Antipraxy—it is *the law of the dose*. It is in the details only that more knowledge is necessary. Every medical man should feel it *his* duty to labour to supply this knowledge of details.

18. The reception of Antipraxy *will imply the reception of several facts*. Such as the following:—

(1) All drugs act injuriously on healthy persons, when taken in any dose. Hence Alcohol, according to the definition, is not a drug.

(2) Each drug has local actions characteristic of itself. (*Organopathy*).

(3) The actions of each drug vary with the dose.

(4) Certain groups of larger and smaller doses of

the same drug, have actions which are in contrary directions.

(5) The known examples of this last statement are sufficiently numerous to make it safe to infer from them that the same is true of all drugs. (*Antipraxy*).

Each of these facts rests upon its own independent evidence, and this evidence has been discussed again and again in former Essays.

19. The reception of Antipraxy is *not indispensable to the commencement of the new practice*.—It is not reasonable to expect that all these law-facts should be received at once, or even quickly. Their reception cannot be brought about by argument. They rest for their truth on evidence, and this is experimental; and to produce conviction and reception, a certain number of experiments are required to be repeated by each person, until the time arrives for its reception by general consent. In the meantime, everyone, if he will, may turn to practical account the knowledge which he has of individual facts.

At the close of a Paper of mine in the *Practitioner* for June, 1879, the Editor—Dr. Lauder Brunton—admits some of these individual facts; he says:—"There are many drugs which in small doses will produce an action the contrary of that which they produce in large ones." Here is sufficient justification for him, and for all who agree with him on this point, to put Antipraxy to a practical verification. It is, I think, their duty to do this. The inference which the Editor himself draws from this admission is:—"that homœopathy and antipathy are one and the same

thing as regards drugs, and differ only in dose." He will forgive me, I hope, when I say that this is unintelligible.

20. *Antipraxy brings Therapeutics into harmony with the Sciences governed by Number.*—Weight and Measure are put into requisition, and the result is a series of Constants. Men with scientific minds—minds accustomed to think accurately—will understand how great an advance this is upon all former foundations of the treatment of diseases by drugs, and they will appreciate its value.

21. Much of what is said from §. 18 to §. 26, in Essay I., on "What is Homœopathy?" remains as true and as valuable for Antipraxy.

And now, what shall I say in conclusion? This is my legacy to the Profession which I have loved, and in which I have laboured, for upwards of sixty years. I heartily wish that it may be accepted frankly. I have passed painfully through Homœopathy, to get at this Antipraxy. Others may now get at it directly, without meddling with Homœopathy at all. "I have always aimed to keep my mind from the two extremes; on the one hand, of giving up my own opinion, from a kind of false humility and deference for men, without being previously convinced that I had been mistaken; and on the other hand, of assuming my opinion to be truth, so as to exclude light, especially if it came from an opponent, or a person not entitled to much deference. So that I have always aimed to be open to conviction, to bring every man's probable opinion to the touchstone and to *give it a fair trial.*" (THOS. SCOTT.)



ESSAY XLIX.

THE DIFFICULTIES OF ANTIPRAXY.

"Our medical knowledge is incomplete. But such as it is, we must use it. And the first condition of using it safely or profitably, is to know that it is incomplete."

PETER MERE LATHAM.

ANALYSIS.

- I. THE DIFFICULTIES WHICH PREVENT ANTIPRAXY BEING LAID
AS THE BASIS OF THERAPEUTICS.
- II. HOW THESE DIFFICULTIES MAY BE OVERCOME.
- III. HOW FAR THEY HAVE BEEN OVERCOME.
- IV. WHAT REMAINS TO BE DONE.

ESSAY XLIX.*

THE DIFFICULTIES OF ANTIPRAXY.

"No great and perfect work is ever accomplished at a single effort, or receives its final polish from one instrument."

WILLIAM HARVEY.

It must have become painfully evident to every reader of this series of Essays, if he did not know it before, that Therapeutics—the treatment of diseases by medicines—is not yet a Science; and that, as an Art, it has scarcely advanced since the days of Hippocrates, in the fifth century before Christ.

To attempt to lay the foundation of it as a Science, and so to show the direction in which it may be improved as an Art, cannot therefore be said to be presumptuous, or premature, or unnecessary; but it may safely be spoken of as a difficult undertaking.

It may be useful to inquire what the difficulties are, which such an attempt has to face; how these difficulties may be overcome; how far, in the case of Antipraxy, they have been overcome; and what remains to be done. We have, therefore, the following topics to discuss in this Essay:—

* Written in 1885.

- I. The difficulties which are preventing this basis of Therapeutics being laid.
- II. How these difficulties may be overcome.
- III. How far they have been overcome.
- IV. What remains to be done.

I. THE DIFFICULTIES WHICH ARE PREVENTING THIS BASIS BEING LAID.

(1) The greatest difficulty in this affair *is its magnitude*.—Monographs on single diseases, or on the doubtful distinction between one disease and another, as, for instance, between Typhoid and Typhus fever; or on single drugs, and their use in particular diseases, as the use of Salicylic acid in Rheumatism, which is now in fashion, as I remember Colchicum was in the same malady, sixty years ago; monographs, I say, are read with attention and interest by the medical profession. So also are books or papers of a speculative or hypothetical character; in forgetfulness that “the truths which are most essential for us to know cannot be discerned by speculative arguments. Chemistry cannot tell us why some food is wholesome and other food is poisonous.”*

But the subject now before us is widely different from all these; it differs in many ways from all professional writings which are popular, but especially in its overwhelming magnitude. This is so great that it involves nothing less than an overthrow of all previous therapeutics. It aims at nothing short of laying a scientific foundation, upon which future Therapeutics may safely rest.

* Froude's *Bunyan*, p. 180.

This magnitude alone is sufficient to make the multitude shrink from any book in which it is grappled with, as being far too obscurely vast to be agreeable reading; it is much too thorough a reformation to be undertaken; it is, therefore, scouted as absurd or impossible, or at the best, it is courteously declined.

There may be a *few* who will have the moral courage to own their responsibilities, and who also have the mental capacity, the industry and the perseverance to face the giant. To these an appeal is made; and though there may not be more than two or three, the appeal will not, I hope, be made in vain.

(2) The next greatest difficulty *is the amount of labour required*.—What is this labour? To test every drug, in many different doses—each a determinate and known quantity—first on the healthy, and afterwards on the sick; to register, without adaptation to any previous notion or conjecture, the effects produced by each dose, as a distinct investigation. Each drug must be studied by itself, and each dose by itself. This separate examination is essential, and so each drug will furnish employment for one experimenter for a considerable time. I once saw a very little boy take up a saw, and begin to cut in two a large log of wood. I smiled; he looked at me, and said, “I shall get it done in my life-time!” This is the sort of pluck which is required for the work to be undertaken.

(3) A third great difficulty is the loud call which this enterprise will make on *the unselfishness of men*—an unselfishness which amounts to a certain measure of

self-sacrifice. For experiments with drugs *in health* must be made chiefly by medical men upon themselves. This ought not to be a hopeless difficulty, though it is a great one. Independent observers have frequently given the medical profession high praise for the exhibition of the grace of self-denial; there have been many examples of it in time past, and others may be hoped for in the future.

(4) Another very great difficulty is *misplaced respect for authority*.—This mistake is not only common, it seems to be inveterate. No doubt, *true authority* not only deserves our respect and submission, but it is criminal in any one not to render it. The mistake is in rendering submission to what ought not to be looked upon as authority. In the matters we are now engaged upon, all true authority is centred in *facts*; whereas it is commonly claimed by *opinions*. Instead of searching diligently for the evidence of *facts*, with the freedom and conscientiousness inspired by the conviction that it is both their right and their duty to do so, men are perpetually making themselves slaves to the *opinions* put forward by those whom they are willing to consider their leaders.

I have so often, on former occasions, protested against this error, that I will here add only a few words on the responsibility of what is called "private judgment" which rests upon every man.

Private judgment is commonly spoken of as being, in religion the privilege of Protestants, and in medicine as belonging to independent inquirers. In truth, it is the privilege of every man, which he can and must

exercise on every subject. It is a responsibility as well as a privilege, inseparable from his nature and condition. He cannot avoid the exercise of it. A religious man joining the Church of Rome, is said to resign his right of private judgment, when he surrenders himself to the guidance of the Priest; a Physician is supposed to resign his, when he determines to be led by the orthodox opinions of his time. Both do this under the modest plea of incompetence. They say:—"I am afraid to trust my own opinion and judgment; learned men must know what is true better than I can." Now, these persons are really exercising their right of private judgment *just as much* as those who are labouring to discover truth by their own efforts. The difference between them lies, not in one exercising his private judgment, and the other modestly declining to do so, but in this—one trusts his fellow men, and follows their lead; the other thinks this an unsafe course, and so he rests upon such a measure of truth as he is able to learn. One leans upon opinion; the other stands upon evidence; but both are following their "private judgment."

These four difficulties are great enough to be deterring; and as regards the bulk of the profession, I do not doubt that they will continue to be so. They are a sufficient and permanent block to prevent such a subject as this being entertained at all by the great majority of medical practitioners.

By the *few* these difficulties will also be keenly felt; but underneath this feeling, a struggle will go on, which will forbid them joining in the peremptory rejection

acquiesced in by the many. For such as cannot be thus pacified, there are other difficulties, which they will find hard to be surmounted, and which shall now be noticed.

(5) *There is a disbelief in the existence of any natural law governing the action of drugs on the living body.*—This disbelief seems always to have prevailed. Five-and-thirty years ago, it was strongly expressed by Sir J. Y. Simpson, who, nevertheless, added that “if such a law could be found, it would be *the greatest discovery possible in Medicine.*” The present generation is as much under the influence of this scepticism as any former one. Indeed, it has been a rare thing to meet with the expression of a belief in the existence even of laws governing diseases. SYDENHAM was one who went at least as far as this. He says:—“I am convinced that Nature here, as elsewhere, moves in a regular and orderly manner.”* I am content to be in a small minority with Sydenham, and feel bound to go even further than his language seems to go. To me, it is certain that the movements of life, whether in health or in disease, whether under the influence of food or medicines, are as much under the government of laws, as are the planets, or any other part of inanimate nature.

We know that in every department of nature, in which laws have been sought for, they have been found. This of itself is a great presumption that laws also govern those departments in which, from the prevalence of this popular scepticism, they have not been sought for.

“Laws of Nature” are, in truth, nothing but the

* Works of Thomas Sydenham, Vol. I. p. 72.

expression of the will of God, who created all things, and who rules over all things by these laws, which are at all times under His control, and may have their movements altered or stopped, much more easily than a clock-maker can regulate the speed of his clock, or stop it.

(6) Again, there is *the complexity of the inquiry*.—It is a problem with many elements; none will feel this difficulty so much as the men best qualified to overcome it. All experiments with living beings are thus complicated. It will be found on trial, that experiments with small doses of drugs, as well as with large ones, have difficulties of their own. At the same time, my own experience has convinced me that all these may be either avoided or overcome by a reasonable amount of skill, caution, and perseverance.

(7) There is *the difficulty of prejudice or prepossession*.—This will assert itself even in noble minds. Such thoughts as the following are continually suggesting themselves:—"After all these many centuries of earnest medical labour, how can it be necessary to start afresh?" "It is humiliating to confess that our knowledge of Therapeutics in reference to medicines, has scarcely advanced since the days of Hippocrates." "If nothing better has been done in the times past, it is useless to expect anything better in the times to come." "Many minds, more able than ours, have been devoted to the study of medicines before us, how can we expect to accomplish what they have failed to achieve?" These arguments, and others like them, are never wanting to discourage and deter even strong minds from undertaking

such a work as this. They are to be dismissed with resolute firmness, as unfit to be entertained. A man must put his foot down upon every one of them. The result will then show that they have defeated their own end, and have ministered to the contrary one; that they have only strengthened the resolution to engage in, and with all the more vigour to push forward an enterprise so necessary for the relief of our suffering fellow-creatures.

(8) There is *the fear of getting into trouble*.—This was the difficulty of a very eminent physician in London. For twenty-five years he was always kind and glad to see me, and would talk with me on my subject for any length of time. There was no one whom I enjoyed visiting so much; he was the one man who understood me; who had the power of overcoming his old notions and convictions; who could see, and could admit, that, to a great extent, I was right. But, alas! he could not face the trouble, which he feared that he should fall into, if he did what I often asked him to do—go boldly to the College of Physicians and tell his belief there.

This fear operates upon minds which ought to be superior to its influence. I sent a Paper to the International Medical Congress, held in London in 1881; having been encouraged to do so by one of its Vice-Presidents. But the President of the Section in which it was to be read, had not the courage to give his consent. "It would have been," said my informant, "as much as his place was worth." Men ought to be ashamed of such cowardice as this.

(9) And not only the fear of trouble, which moral

courage ought to extinguish; there is also *the exceedingly hard work of changing the current of thought*, this current of thought being the necessary result of all previous education. This is no slight difficulty, and it is not a discreditable one like the last. All authority, all teaching, all study, all practice, even all professional associations, have been flowing, like a strong tide, upwards in one channel. All these must ebb, and flow downwards, never to rise again. Who will wrestle with all this? Who?

(10) Lastly, there is a difficulty which can never be removed, namely, *the Will*—Divine and human. When every care has been taken to render the experiments as simple as they can be made; and every pains taken to estimate at its true value each of the elements of the problem which is under the dominion of the laws of nature; there always remains the element of *Will*, which has power—the Divine Will absolutely, the human Will within narrow limits—to counteract or nullify experiments. This independent and uncontrollable element is not peculiar to experiments with drugs; it is not peculiar to experiments with living beings; it exists in every department of science. In all it is met sufficiently, when it is recognised and allowed for.

This is a slight sketch of very serious difficulties; nevertheless, as I have contended in these Essays for five-and-thirty years, *difficulties are things to be overcome*. What, if there be both a lion and a bear in the way? The lion and the bear must be faced. There was once a stripling, who could say:—By the help of God, he slew both the lion and the bear. So let it be again.

II. HOW THESE DIFFICULTIES MAY BE OVERCOME.

We will now venture to suppose that such of these difficulties as are moral or intellectual, have been overcome by moral and intellectual considerations; and that the resolution has been taken, as we trust it will be, to engage in the work. There are difficulties remaining, which belong to the work itself, and the object of the following paragraphs is to point out the best course to be pursued, and to show how these unavoidable difficulties may be diminished or overcome.

(1) *By experiments with drugs on the healthy.*—It has just now been remarked that “every care should be taken to render the experiments as *simple* as they can be made.” This can be best done by eliminating as many elements as possible, so as to reduce those that remain to the lowest number. Experiments with drugs on the sick are not thus simplified. It ought to be so plain as to require no proof that the presence of disease is a complication; one sufficient to prevent accurate knowledge of the action of drugs on the living body of man, being obtained by any number of experiments. Nothing short of a condition of *health* can deliver the investigation from this complication, and so simplify the experiments as to make the results satisfactory. This has been so often urged in former Essays, that it would be tedious to say more upon it now.

(2) *By the use of the Balance.*—This is the method which has converted our knowledge of other departments of nature from chaos into science; the application of this

method to the testing of drugs is not only possible but peremptory. Quantity must be put before quality. When this has been done, it will be seen that quantity is the ruling power. This may be called a retrograde movement, a revival of the doctrine of numbers of Pythagoras, but it is none the less true for that. The accepted teaching—the teaching, under changes of form and expression, which has so long prevailed—as to the properties of medicines is not true. To say that certain drugs are purgatives, and certain others are astringents; that those are depressants, and these are stimulants; are statements so far short of the truth as to be little better than blind guides leading us into dangerous errors in practice. They are statements which, though made for thousands of years, only cover the properties of drugs with the mantle of theory, and put a veil before our eyes which hides from us our ignorance, and prevents us going in search of true knowledge.

But to say that such quantities by weight of a drug, when taken in health, have such and such properties, or powers of action; while such other quantities of the same drug, have other properties, or powers of action, when taken under the same conditions; is to state facts which admit of proof, and which can safely be relied upon as guides for the use of these doses as medicines. For, when a known quantity or dose of any drug has been tested in health, and has been found to have an action which can be recognised, it may be assumed without fear that the same dose will act, or tend to act, in the same manner upon other persons who are not in health.

Let it be remembered, therefore, that in all experi-

ments with drugs, whether on the healthy or on the sick, the *balance* is the first requisite—that *quantity* is the ruling principle. It follows from this that, in a scientific sense, it is untrue to say of any drug, that such and such are its properties. Before ascribing any action to any drug, it is necessary to fix the quantity or dose to which a property or action may, with truth, be said to belong. And it also follows that, in reference to their scientific value, all experiments made with indefinite quantities—that is, with unknown doses—are imperfect.

(3) *By testing each drug by itself.*—This rule has been persistently urged upon the Profession for a century. Its truth, and even its necessity, one would have thought, but for daily experience to the contrary, were only the dictates of common sense. Alas! experience often contradicts the most obvious suggestions both of reason and common sense; and never more strikingly than in the complicated prescriptions of the medical practitioners of past and present times. Drugs are still compounded, that they may “fight together in the dark” in the interiors of the victims to whom they are administered. The value of this rule is now beginning, though reluctantly, to be acknowledged. Sir Thomas Watson allowed to me a good many years ago, that one drug should be tested alone, and be given alone as a medicine. It seems as if it would take another century before there will be courage enough for it to be allowed all round.

This opportunity may be seized to remark that the drug to be experimented with should be in a known state of purity; and also that it is better that it should be, as nearly as is practicable, in its natural condition, and

before it has been submitted to *chemical* disintegration: *e.g.* that it should be the bark of the Cinchona tree, rather than Quinine; that it should be Nux Vomica, rather than Strychnine; Opium, rather than Morphia; and so of other drugs.

(4) *By testing each dose by itself.*—Not only does each drug require to be experimented with by itself, but each *dose* must be dealt with in the same manner. An experimenter is not to take a fraction of a grain or drop to-day, and a whole grain or drop to-morrow. The same dose may be repeated at sufficient intervals, but the experiments must be confined to that dose during the whole of one investigation; and when the dose is repeated the repetition must be recorded. Many other details have been given in former Essays; they need not be repeated here; at least it is hoped that they need not. In this hope only two more observations shall be added.

(5) *By discovering the seat of action.*—In these experiments success will necessarily depend upon careful and correct observation, as it does in mathematics on accuracy of calculation; and in order to render this observation perfect, there must be an untiring ability to take trouble. Then, the first endeavour must be directed towards the discovery of the organ or organs of the body, which appropriate the drug, and where, consequently, its action will take place.

(6) *By discovering the kind of action.*—After the locality or localities of the action have been ascertained as correctly as is possible, the next step to be taken is the discovery of the kind of action which is going on in the part where the drug has been taken possession of.

A faithful and persevering observance of these rules, will conduct every competent medical man, who engages in this research, to an assured victory over the formidable difficulties which have been honestly pourtrayed.

III. HOW FAR THIS WORK HAS BEEN DONE.

Having surveyed the great difficulties which obstruct the path that ought to lead to the successful laying of a scientific foundation for Therapeutics; and having also examined the steps by which these difficulties may be overcome; we are now prepared to look at the progress which has already been made in this arduous work. For, notwithstanding the discouragement which these difficulties present, it is certain that, to some extent already, they have been overcome.

(1) *Many drugs have been proved or tested in health.*—In former Essays details have been given of the work done in this stage of the inquiry, by several men whose names are recorded as being well deserving of remembrance. These details, I hope, need not be given again. The result is a very large collection—in many volumes—of provings of all our best known drugs, and of many which had not been heard of before.

Many of these provings have been made with great care and precision as to the drug, if not as to the dose, and with no small amount of persevering self-sacrifice; and with the result of determining with much exactness, the characteristic actions of these drugs. They are a durable monument to the anxious and unwearied doings

and sufferings of men, who are justly entitled to the grateful memory of their successors in the same field of labour; and not only of these, but of all to whom their work becomes known.

It has been asked, And what have *you* done? It may be answered that I have done what I could. In the proving of new drugs, one has been attempted—the metal Titanium—which nearly killed me, but which has since enabled me to save more lives than one, by affording me a remedy which we did not possess before. No doubt, surprise will be felt at my using such strong expressions as these, but I believe they are true. The report given of this proving in Essay XIV. was brief, and in one respect imperfect. The experiments were made in 1856—twenty-nine years ago—and were recorded then, as far as I could rely upon their truthfulness; since then I have had reason to believe that another very serious effect was produced by the metal, which was not recorded. Besides the state of ill-health described in the report then given, and attributed, I think, correctly to a morbid condition produced in the blood, there was a very copious discharge of albumen from the kidneys. This continued so long, and was accompanied with so much exhaustion and weakness, that I was compelled to suspend work and go to the sea-side. Dr. Sutherland, whose patient I then was, spoke to my wife in alarming terms as to the probable issue. The albuminuria continued for two years; and though I was a good deal strengthened by repeated visits to the sea-side, it was not until I took *smaller doses of Titanium itself*, that a cure was effected. My usual health then quickly returned. This is now

twenty-seven years ago, and I have never had the slightest return of the complaint.* Moreover, more cases than one of albuminuria have been cured in my hands by this same Titanium.

Provings have also been made with the smaller doses of many drugs. Several of these have been reported in the Essays written at the time. They were undertaken with the object of learning, if possible, whether the different doses of the same drug had any characteristic difference of properties or action, and whether any glimpse could be got of the law which must govern the dose, and the knowledge of which would become a rule for the choice of it.

(2) It is now known that *each drug has a seat of action*.—From the great mass of provings of drugs in our possession, a first universal fact or law of nature has become a legitimate induction: namely, every drug, in whatever doses given, acts by preference on some part or parts of the body, rather than on others. This general or *law-fact* was examined in much detail in Essay XVII., and there received the name of Organopathy. It has since been contended that this was a plagiarism from the Germans; that the fact must have been known before, because the *name* had been used before. It has been shown, in reply, that though the name had been invented before, of which I was not aware, the fact itself as now stated was not known.

(3) And that *each dose has a kind of action of its own*.—This is a much newer subject than the last, and

* 1886, July 19. As far as concerns the kidneys I am thankful to say that I continue in perfect health.

until now, has received very little attention. It will be acknowledged that to discover the seat of action of each drug, is a study of great value: to discover the various kinds of action of the different doses of the same drug is, at least, of quite equal importance—at the present moment it is of greater importance; it has become a question surpassing every other, in pressing and serious interest. The reason is this—the Profession is now engaged in adopting nearly all the so-called homœopathic remedies: they are heartily congratulated for this, but they are using these remedies in such a mistaken manner, that the results are unsatisfactory. There is the most imminent danger that the undertaking will suffer shipwreck, and the rocks on which this will happen are the varying effects of *different doses* of the same drug.

So many credible reports have been published of cures of recognisable diseases, which have been treated by these medicines, in the hands of qualified medical men, that the temptation to try them by the orthodox has become irresistible. They are now being tried. Occasionally they succeed, but they more commonly fail. How is this? Simply because the new prescribers, who are willing to give the same drugs as the homœopathists, are *not* willing to give the same doses. They say they have no faith in the small doses; they give larger ones; and they get results not at all resembling those reported by the homœopathists. Thus they are driven to draw false conclusions; and the new departure is threatened with failure. How is this to be prevented? Only by the investigation—patiently and honestly carried

on—of the question of the characteristic diversity of dose-action. It is a real work; it is an urgent work; it is a work, which if not done, will make the present using of good remedies a grievous injury to the sick, because it cannot succeed; which if done, will lead to the most beneficial reform of Therapeutics that has ever yet been possible.

Let it be repeated once more—each *dose* of a drug has its own action; and this action ought to be understood *before the dose is given to a patient*.

(4) And that *certain larger and smaller doses of the same drug have contrary actions*.—As it is with food, so it is with medicine. Food must not only be wholesome in quality, it must be moderate in quantity; so a medicine to do good, must not only be rightly chosen as to its quality of action, but it must be given in the suitable dose. Too much food may kill; so may large doses of drugs. Every drug has two sets of doses; the one killing, the other curing. This is strong language, but if I am to be heeded, it must be used.

During the last few years several medical men have noticed and acknowledged the existence of this contrary action of large and small doses as belonging to a few drugs. Dr. Lauder Brunton has gone so far as to say that it belongs to “many drugs.” But no one has joined me in the endeavour to prove that it belongs to *all* drugs—that it is a natural law. And what is perhaps of still more importance, none have yet joined with me in urging upon the Profession the importance of this fact as a guide in practice.

To me, it is its *practical* value that is ever uppermost

in my thoughts. The larger set of doses may be given, and have been given for ages, as remedies to act on the principle of *contraria contrariis*. My conviction, in respect to these, is, that they commonly do harm; that they often increase disease, and render recovery much more difficult and tedious; and that they sometimes kill outright. The smaller doses, on the other hand, I contend are the doses which ought to be given, and on the same principle of *contraria contrariis*; because I am well convinced that these doses so given, are very much less likely to do harm, and have very much more power to cure. Who will put this plain statement to the test of experiment in his own hands? It demands this verification, and the demand cannot be silenced by argument.

A claim is thus set up, in regard to the action of drugs on living men, as well in health as in illness, of a threefold character—that they act locally, on one or more parts of the body in preference to others; that they have characteristic kinds of action on the locality which is the seat of the action; and that the kind of action is determined in contrary directions by larger or smaller doses of the same drug. To this last remarkable fact I have given the name of *Antipraxy*, from ἀντί, “one against another,” and πράξις, a “doing” or “action.”

The proof of this fact or law consists of two series of experiments; one on the healthy; for this former Essays must be referred to: the other on the sick; this will be found, partly in former Essays, more fully in the Appendix already written, but not yet published.

IV. WHAT REMAINS TO BE DONE.

Alexanders need not weep because there is nothing left for them to conquer in struggling with the difficulties of medical knowledge. The task, as now set before the medical profession, in regard to Therapeutics, is a great and worthy one—to acquire real knowledge of drugs, and specially to perfect the practical application of the two laws of Organopathy and Antipraxy—the seat, and the contrary kind, of action—in every disease, and for every drug.

That the task is a large one will be very visible, if we consider for a few moments the length and breadth of this great subject.

What is our present knowledge of drugs? We think we have known some drugs for a long time, and fancy ourselves familiar with their proper use; but this is a mistake. There is not one drug concerning which we know its seats of action, and kinds of action, throughout its various doses, as these may be and ought to be known.

Sir Thomas Watson, in his Address as the first President of the Clinical Society, expressed this view, when he said:—"We want to know, distinctly and clearly, what is the action of drugs." This, surely, was a sufficient acknowledgment that we have not yet obtained this knowledge.

There are no drugs that we know as we ought to know them. Further experiments on the healthy, as well as on the sick, will teach us much more than we yet know about them. And if it is so of those with which

we are best acquainted, how much more about others, of which at present we know so little!

Then again, how much we have to learn about doses! With respect to the great majority of drugs, almost every thing belonging to their different doses. What I can say I know is this: with respect to some drugs I know how a dose containing the hundredth part of a grain or drop of the tincture will act upon myself, with a degree of certainty sufficient to enable me to state it beforehand. This shows that exact knowledge of doses is attainable, and if so, without question it ought to be attained.

As an example of what I mean, and of the work which has to be done, it may be mentioned that the following drugs act on the liver (each also on other organs) and in the doses indicated, and the effect of that action is an increased secretion of bile: namely, Mercury (the triturated metal) in the hundredth part of a grain, Cinchona (tincture of the bark), Chamomile (*Matricaria Chamomilla*, tincture of the plant), Bayberry (*Myrica Cerifera*, tincture of the root), each in the hundredth part of a drop, and Club Moss (*Lycopodium Clavatum*) in the thousandth part of a grain. All these doses, taken in health, increase the quantity of bile; and they have been given successfully as remedies in sundry disorders of the liver. It is required to know further, the shades of difference in the action of these doses of the five drugs, both when comparing them with each other, and also when contrasting them with *other doses* of the same drug.

The action of Aconite and Digitalis on the heart

might be similarly spoken of. So have we to learn the actions of the larger doses, and of the smaller doses of all drugs, and also of the middle doses between the larger and the smaller, which divide them, and which partake of both their kinds of action.

When the seat of the action of any known dose of a drug, and also the kind or direction of its action, have been satisfactorily discovered, both the law of uniformity, with which all who study nature are now familiar, and abundant practical experience, justify us in being certain, that this particular quantity of this individual drug will always tend to act in the same manner in every experiment, whether on the healthy or on the sick. It may be hindered in this action, or even wholly prevented from performing it by counter-acting influences; but so may gravitation, or any other force of nature with which we are acquainted.

If, therefore, in the treatment of a patient, we have a definite aim, and if we know a drug, which, in a certain fixed quantity, has a corresponding aim, we may have confidence that this drug, in the same quantity, will certainly carry out that aim, unless it is prevented by other forces stronger than itself.

There are other subjects in medical treatment with which we are far from being perfectly acquainted, but which can only be alluded to here. Our knowledge of disease has been greatly advanced in the last and the present generations by most praiseworthy and self-denying labour; but there is room for further advances. What it is most important for us to know is the *natural* history

of diseases—their progress unmolested by interference. It is only now and then that opportunities for observing this are honestly offered; but when they are, they should be utilized to the utmost, the observations being made without the bias of a favourite system, or a fashionable theory.

There is the question of food for the sick, which is quite unsettled. Sixty years ago the starving system was in fashion, and patients literally died for want of food. Over-feeding and over-stimulating then came into vogue, doing perhaps still more harm, especially in fevers. The “little and often” of broths and brandy helping to kill not a few. Some may think it well that “the sixth Commandment is suspended by our Medical Diplomas.”

Then again, on the subject of rest and exercise, our notions are not clear, nor our practice in the least uniform. Plato was not a physician, but he gave good advice to physicians, when, on the treatment of disorders, he said “the best adjustment of the body is that which is effected by gymnastics. The next best by carrying the body, as in sailing, or any other mode of conveyance which is not fatiguing. Wherefore we ought to manage our disorders by regimen, as far as a man can spare the time, and not provoke a disagreeable enemy by medical treatment.” (Timæus). By having recourse to the mode of using medicines recommended in these Essays such provocation can be avoided.

In like manner Sydenham advocated riding on horse-back:—“Of all remedies that I know, nothing so cherishes and strengthens the blood and spirits, as riding on horse-

back, long distances, every day. . . . Indeed, deadly as phthisis is, killing two-thirds of those who die of chronic diseases, it has a specific in riding, as truly as ague has in bark.* Again:—"Bark is no surer a cure for ague than riding is for phthisis."† I also can bear some testimony to this. One patient, a gentleman subject to lung disease, was kept in comfortable health, for a quarter of a century, by small doses of medicines and by riding on horseback. He is living still.

But my subject is the use of drugs as medicines, and I conclude by repeating, that a great work is here set before the Medical Profession. One man has done what he could, let others do the same. They will stand upon my shoulders, as I have stood on the shoulders of my predecessors, and so will see further than I have been able to see. New workers may enter into my labours, as I have thankfully entered into the labours of those who have worked before me, and may receive a more abundant reward. And this I earnestly desire. It is thus that the vast and noble fabric of a true Therapeutics can be built upon the foundation now laid. The cloud, which has hung thickly over Medicine so long, is lifted; there is a bright horizon; and the prospect of a brilliant sun-rise out of the sea, which has been tempestuous, but is now becoming calm. Is not this enough to stimulate the enterprise and to kindle the enthusiasm of the coming generations?

* Epistolary Dissertation, 1681. Sydenham Society's Edition, Vol. II., pp. 106—7.

† Op. Cit., p. 296.

ESSAY L.

OBJECTIONS TO ANTIPRAXY.

*“ And differing judgments serve but to declare
That truth lies somewhere,—if we knew but where.”*

WILLIAM COWPER. (*Hope*).

ANALYSIS.

OBJECTIONS TO ANTIPRAXY STATED IN A LETTER BY DR. JOHN
SYER BRISTOWE, F.R.S., SENIOR PHYSICIAN TO AND
LECTURER ON MEDICINE AT ST. THOMAS' HOSPITAL, ETC.

MY REPLY.

ESSAY L.*

OBJECTIONS TO ANTIPRAXY.

"Disagreement should carry with it no detraction; there should never be anything but honour paid to the search of truth."

F. W. H. MYERS.

It would be idle to cope with the irrational opposition offered by many. It would be unbecoming to contend with the opposition arising from self-interest, jealousy, or other unworthy motives. But there is a serious and truthful opposition raised by a few which commands attention. This opposition arises in part from misunderstanding. An entirely true view of what has been set before these able men has not yet been taken by them. To opposition of this character earnest attention is due, and this attention shall be given to it. A happy result need not be despaired of.

The opposition of which I am speaking has shown itself to me mainly in private letters from men for whom I have the highest respect, and their letters have been written under the influence of real regard both for the subject and for me. In Essays XXXIX. and XL. portions of several of these letters, and one entire one,

* Written in 1885.

have been answered; and this Essay will be devoted to the latest letter of this kind, which I have received. The talented and well-known writer gives me leave to mention his name. The Letter is given entire, at the request of the writer, with the omission only of some very friendly courtesies; and, as will be seen from it, contains an invitation to me to reply in this manner. The interpolated figures, (1), &c., refer to the corresponding figures in my reply.

Many medical men will agree with the views exhibited in this Letter, and will rejoice to see them so well expressed. It may be looked upon as a representative Letter. My reply, therefore, will not be personal, but will be addressed to this large section of the Profession, in the hope that some may be persuaded to reconsider their mode of practice, and to change it for a better, to the great advantage of their patients, and ultimately, to their own distinct satisfaction.

“ 11, Old Burlington Street,

“ London, W. December 27, 1884.

“ DEAR DR. SHARP,

* * * * *

“ Your Paper on Toxicology [Essay XXVII.] I have read carefully more than once, and have thought over. Like every thing you write, it is logical in form, and singularly clear. No one who reads it, can, I think, misunderstand you—at any rate I believe I understand your argument; and if I understand it, I must, I am sorry to say, dissent from it. But this, I take it for granted, you expected. (1). I shall not discuss the

whole Paper, but shall limit the few remarks I am about to make to its essential points.

“‘The *kinds* of action of comparatively large and small doses of the same drug are opposite, or in directions contrary to each other.’ I admit that with respect to some drugs, there is *primâ facie* evidence that in small doses they act differently from what they do in large ones. Thus, I admit that small doses of Rhubarb often cause some degree of constipation, while large doses purge; and that small doses of Opium excite the action of the nervous system, while coma is induced by larger doses. (2). But it does not seem to me that we ought to rest contented with this mere appearance of opposition; but that we ought to look more deeply into the matter; first, in the sense of making sure what we mean by contrary actions; (3) and in the next place, of determining how these contrary actions are obtained, before adopting the conclusion of the opposite action of small and large doses. (4). Now in the case of Rhubarb, it is generally taught, and I have no reason to doubt it, that its constipating effects depend on the fact that, associated with the purgative principle, there is also an astringent principle, (5) and that it is owing to this combination that purgation by large doses of Rhubarb is followed by constipation. (6). How far this explanation applies to the alleged astringent effect of small doses I am not prepared to say. (7). But there is one general consideration in regard to purgatives which may apply here, and ought not to be overlooked. Some purgatives act mainly on the smaller bowel, some on the larger. (8). Now supposing a small dose of purgative medicine acting

mainly on the small bowel be administered, the result would probably be that some irritation and some peristaltic action of the smaller bowel would be produced, and its contents would be hurried, with more or less colicky pain into the colon, and there all action would probably be arrested. That is, the patient would have diarrhœa from one part of the bowel into another part, but no diarrhœa in the usual sense of the term, and probably even some constipation. Now I believe that this happens not infrequently when comparatively small doses of certain purgatives are given; and I know from personal experience, that it often happens when disturbance of the bowels results from unsuitable diet, or climatic influences. In such cases I fail to see any essential opposition in the effects of large and small doses respectively of either medicines or the causes of diseases. The opposition is limited to one item in the collective effects of the agent. (9).

“Next as regards Opium. Are its effects of mental excitement and its effects of coma really opposite conditions? In a certain sense, of course, they are. (10). But again, I cannot help thinking that their opposition is not true opposition. Thus, in the first place, the two conditions pass gradually into one another; (11) the morbid condition of over-excitement passes step by step into the morbid condition of coma; and in the second place, what is called mental or nervous excitement is not always, probably is rarely, the result of increase of mental or nervous power, but due to the removal or abeyance of some of those restraints which the will and higher elements of the mind habitually exert over the subordinate elements. This is certainly so with respect

to insanity; (12) and the excitement caused by Opium or Alcohol (13) is a species of insanity. Now supposing (as I think not unlikely) that the first effect of Opium is to deaden some of the higher mental faculties, and thus to allow subordinate mental faculties to run riot uncontrolled, and that the later effects are the consequence of its benumbing qualities involving a wider and wider area of faculties, it would be easy to understand how apparent excitement would not be the true opposite of coma, and how it is that the two conditions merge the one into the other. I am not maintaining that the explanation I have here given is the true one; but it is at least plausible; and at any rate illustrates the nature of my general views as to my inability to adopt your opinions. (14).

“But my strongest objection to your hypothesis is suggested by your deduction from your law which I have quoted; namely that, as regards poisons, ‘small doses are antidotes to the injurious effects of larger ones.’ Now things which are really opposite or antagonistic to one another must start from a common point and proceed in opposite directions, the point of departure being a neutral point; (15a) or at any rate the conditions must be of an equivalent character; and this you seem to admit by the use you make of the cases of snake poisoning which you quote. You there assume that the small dose caused the blood to coagulate, while the larger dose caused it to remain fluid. (16). It certainly seems to me that if medicines have truly opposite effects when given in very small and in large doses, there must be some quantity of such medicines

half way between the extremes, in which all medicinal and poisonous effects should be neutralised, and which therefore should be inert. (15*b*). Is it so? I take it for granted that it is not. And that I imagine is your opinion in reference to medicines. But it is not your opinion apparently in reference to poisons. (17). For here you hold, not only that the effects of larger and smaller doses are opposites in the full and strict sense of the term, but you teach that the small doses can really neutralise the effects of the larger doses: in other words, as I would venture to put it, that there is a mean dose of every poisonous substance which is absolutely inert. (15*c*). To explain my meaning and yours more fully—let it be assumed that 10 grains of white arsenic constitute the poisonous dose, and that the millionth of a grain is the antidotal dose; your contention is that if the patient has taken the poisonous dose, its effects will be neutralised by the taking of the antidotal dose. If this be a fact, then I contend that the exhibition of 10 grains and one millionth of a grain in a single dose should be inert. (18).

“The subject of the treatment of diseases by medicines is, I admit, one of exceeding difficulty; and one in regard to which I am very sceptical whether any general laws of value exist or can be discovered. (19). I do not believe in the slightest degree in Hahnemann’s hypotheses; and I agree with few, if any, of the fragments of systems comprised in the chaos of “orthodox” Therapeutics. You will not therefore, I trust, regard me as disrespectful if I decline to be a convert to your teaching; nor should I have discussed the subject unless

you had yourself invited me. As it is I have not discussed it fully. I have at most only suggested difficulties to the acceptance of your views. You may think lightly of them; and doubtless will be able to adduce reasonable arguments on the other side. (20). I shall be pleased to receive them if you think it worth while to send them to me; especially if you care to discuss it in one of your Papers.

"My own *Materia Medica* lies within a very narrow compass. I am entirely satisfied of the exceeding value of a certain number of drugs in a certain number of diseases, and of the general usefulness of tonics, of one kind or another, in a very wide range of maladies. But I do not know how those medicines act on which I have the greatest reliance; but I do not see how in any sense they can be said to act either on the principle of homœopathy, or on that which you advocate. (21).

* * * * *

"With kind regards, I remain,

"Yours very truly,

"J. S. BRISTOWE."

It has been remarked already that many medical men will agree with this letter, and will be ready to say that Antipraxy has been driven out of court, and may at once be dismissed without further thought. I venture to think, on the contrary, that it has not received any damage at all, and I hope to be able to show that it has not. I am not one of those who love controversy for its own sake, and would never

engage in it unless there seemed to be some encouragement to hope that those who differ may be brought to agree. In this hope I now write.

It ought to facilitate this result if we take the preliminary step of finding common ground on which both sides can take their stand. This common ground is defined in the following postulates:—

First: it is necessary to agree that it is the observation of *phenomena* as *facts* that we are in search of; and this without reference to their modes of action, and with the exclusion of all hypothetical explanations.

Second: that these facts can be learned from *observation* and *experiment* only; and not from any reasoning, however plausible or ingenious.

Third: that to discover the action of drugs, the experiments with them must be made first upon persons in *health*.

Fourth: that they must be made with *definite doses* or quantities of each drug.

Fifth: that afterwards, experiments must be made with the same drugs and doses on the *sick*.

Sixth: that the results of these experiments, both on the healthy and on the sick, must be accepted as *observed facts*. Any attempts to push them aside, or invalidate them by any reasonings whatever, will then be felt to be impossible.

These six postulates being agreed to, the paragraphs in the Letter, which are marked with figures, may be hopefully dealt with, and in a comparatively brief as well as satisfactory manner.

1. "I understand your argument, and if I understand it, I must, I am sorry to say, dissent from it. But this, I take it for granted, you expected."—I am at a loss to understand why I should expect this. When a man has made a discovery, which he is sure is of value to his friend, and tells him of it, why should he expect, as a matter of course, that his friend will refuse to accept it? It is more natural for him not only to hope, but to expect, that his friend will gladly accept it.

2. *Primâ facie* evidence of Antipraxy is admitted.—"Thus, I admit that small doses of Rhubarb often cause some degree of constipation, while large doses purge; and that small doses of Opium excite the action of the nervous system, while coma is induced by larger doses."—The Letter makes this admission with respect to "*some*" drugs. Dr. Lauder Brunton, when commenting on my Papers in the "Practitioner" (for 1878 and 1879), admits that "there are *many* drugs which in small doses will produce an action the contrary of that which they produce in large ones." These admissions need only to be extended to *all* drugs and we are of one mind. It is easy to add to Rhubarb and Opium, other well-known drugs, which act upon important organs, and which so certainly have this contrary action in larger and smaller groups of doses, that it may be presumed that the same admission will be made respecting them. I will mention a few. Aconite and Digitalis act on the heart: certain larger doses of Aconite quicken the heart's beats; certain smaller ones slow them. Of Digitalis certain larger doses enfeeble the heart's action; certain smaller ones strengthen it. Digi-

talis also acts on the kidneys, and certain larger doses increase the secretion of urine; while smaller ones suppress this secretion. Mercury acts on the liver, in the larger doses diminishing the secretion of bile; in the smaller increasing it. Chamomilla acts in the same manner on the liver, the larger doses diminishing and the smaller increasing the secretion of bile. Myrica cerifera (Bayberry) again does the same, the larger doses obstructing the course of the bile; the smaller ones increasing its flow. I think the action of Podophyllum on the liver is the reverse of all these. Belladonna acts on the iris, in the larger doses dilating the pupil; in the smaller contracting it. Physostigma (Calabar bean) reverses these actions on the iris. Purgatives, from Castor Oil, the mildest, to the most drastic, as the White Hellebore of Hippocrates, possess these contrary modes of action, according to the dose. So-called astringents have it in like manner; for many years the medicine for constipation from torpor of the bowels, on which I have relied with confidence, has been Opium. I have not found it fail, and this because the dose which I have given (one drop of the first dilution of Laudanum), sometimes repeated two or three times, gently relaxes the bowels when taken in health. Here are a good many examples, and more might be added. I have experimented upon myself and my friends with more than twenty drugs, and with similarly uniform results. Let it not be forgotten that these are experiments on the *healthy*; that the same results appear, when they are tried upon the *sick*, the Cases given in previous Essays, and to be given in the Appendix to this volume, abun-

dantly testify. The law of uniformity, now recognised by all who study nature, justifies the induction from these individual facts of the universal or law-fact expressed by the word Antipraxy. It is a true induction, which consists, not in a mere list of individual facts—that would be only an *enumeratio simplex*—but in the combination of these individual facts observed by the senses, and a mental process, which sees that other drugs, not yet tested, may safely be included as being governed by the same law.

To invalidate the testimony of these experiments, there is no evidence brought forward on the other side. So far as appears, none of the experiments have been repeated, nor have other experiments been tried. Explanations of the actions of Rhubarb and Opium have been offered, which shall be considered presently. The truth, as it appears to me, is that there is nothing in opposition but the bias of mind, which is naturally and almost unavoidably produced by habitual thinking in one groove.

3. "We ought to look more deeply into the matter; first, in the sense of making sure what we mean by contrary actions."—I am glad of another opportunity of trying again to make this meaning plain. Antipraxy is not a pugnacious or fighting contrary; its definition is *a movement in opposite directions*. And in explanation of this movement, any vital process may be referred to as an example. It is seen in the contraction and relaxation of muscles; in the increase and diminution of the secretion of glands; in the size of vessels, their contraction or dilatation; in the rising and falling of

temperature; and in many other ways. But not only in such obvious examples as these; it is equally present in every vital process resulting in the formation of structure in health, and of organic change in disease. In studying this subject it is necessary to remember that *all* the operations of life are *functional*, and consequently, all admit of movements in contrary directions.

4. "In the next place, of determining how these contrary actions are obtained."—It has been maintained constantly and strenuously in these Essays, that what we ought to desire to know is all that observation and experiment can teach us of *what* happens; *how* it happens is commonly beyond our discovery. But whether we can find this out or not, the truth of the observed fact remains, and is not diminished by our ignorance of its cause; and, which is of the highest interest to us, its practical usefulness also is seldom impaired. Physicians have given doses of medicine for two thousand years, and none of them have yet known *how* one of these doses has brought about its effects, whatever those may have been. This ignorance is as profound with respect to large doses as it is to small ones; to ourselves as to our forefathers. Dr. Bristowe, at the close of the Letter, acknowledges it of himself, and it is no disgrace, nor is it peculiar to him; it is common to us all, and it is well when we remember it. To explain this *how*, a chaos of hypotheses—guesses—has occupied men's minds, which has been the greatest obstacle to the progress of true medical knowledge; and not until the conviction of the truth of this has sufficient power to induce physicians to discontinue,

utterly and for ever, indulging in "supposing" these "plausible" guesses, will that progress become rapid. In the Greek language the verb εἶδω means both *to see* and *to know*; and the perfect tense οἶδα, *I have seen*, is used as a present tense in the sense of *I know*, for what one *has seen* that one *knows*. The secret of true wisdom is to rest contented with this.

5. "Associated with the purgative principle there is also an astringent principle;" and further on—"the benumbing qualities of Opium."—It is much to be regretted that such expressions occur in so thoughtful a Letter. Language of this kind is really little more than a veil, which hides from our own eyes, and from the eyes of others, our absence of knowledge. It exposes us to the satire of Molière, which will never be forgotten, while such expressions continue to be used; and it reminds us of Sir Robert Peel, who, when he was asked in Parliament, what are the duties of an Archdeacon, gravely replied, "they are archidiaconal duties." It is true that Chemistry has, for some time, laboured diligently to help us in this matter, but its want of success is conspicuous.

6. "Purgation by large doses of Rhubarb is followed by constipation."—Constipation commonly follows purgation by whatever drug, and is usually explained as the natural reaction to the previous condition. The bowels have been over-stimulated, and now they are exhausted, and are unable to perform their natural functions. But whether this is so with respect to Rhubarb, or whether

the constipation is the effect of the "astringent principle" it is said to contain, has no bearing on the subject in hand. The remark belongs to the study of the various effects of the *same dose*. We are now engaged in studying the various effects of *different doses*; so that the remark is irrelevant.

7. "How far this explanation applies to the alleged astringent effect of small doses I am not prepared to say."—It does not apply at all to the action of the smaller doses of Rhubarb. If it were granted that there are two "principles," one purgative the other astringent, in Rhubarb, they must be there in *the same proportions*, whether the dose is large or small, unless different processes have been followed in their preparation, which is not in the supposition. If, therefore, the purgative principle overpowered the astringent principle in one dose, why should it not do the same in any other dose? Suffer me to say again that all this is speculative not practical thinking, and ought to be discarded altogether from this practical subject. The expression "alleged astringent effect" seems to imply that the writer is unacquainted with experiments with drop doses, or fractions of a drop of the Tincture of Rhubarb. I wish a dozen or two of those I am now addressing would try them on themselves—the experiment would not kill them.

8. "Some purgatives act mainly on the smaller bowel, some on the larger."—This is, to me, a grateful acknowledgment, in one example, of the first fundamental

truth of the action of drugs on the living body. That this action is always *local*, and that the locality is characteristic of the drug, has, I think, been proved in Essay XVII.

9. "The patient would have diarrhœa from one part of the bowel into another part, but no diarrhœa in the usual sense of the term, and probably even some constipation."—This is a very interesting story of the action of some doses of Rhubarb or some other aperient, and I thank Dr. Bristowe for it; but I hope he will accept my comment. The doses which act as he has so well described are the *middle doses*; these partake of the action of both the larger and the smaller doses. That they should be brought forward in refutation of Antipraxy brings prominently before us the different notions which medical men have as to what are "small doses." The dose of Tincture of Rhubarb, to act as an aperient, is put down at 2 dr. to $1\frac{1}{2}$ oz. Probably from 5 to 15 drops will by many be called small doses. It is likely that these will act as the Letter describes; that is, they will first show the action of the larger doses in producing irritation and colicky pains; then this action will stop and that of the small doses will follow, and so constipation will be the ultimate result. There is no necessity to suppose that the colon has "arrested" the first action. The small dose which produces constipation, without these preliminary colicky pains, is (for Rhubarb) one drop, or with some of us, less than this. Of course the simple Tincture is meant. We have nothing to do with compounds.

10. "Next, as regards Opium. Are its effects of mental excitement and its effects of coma really opposite conditions? In a certain sense, of course, they are."—What that sense is we are not told, nor what is meant by "true opposition." That they are opposite conditions in the sense given in the definition of Antipraxy must, I think, be admitted. (See reply 3). They are certainly, as regards nervous power, "a movement in opposite directions."

As in the case of Rhubarb (9) so in that of Opium, we have to observe *what* happens—*how* it happens, or is brought about, I believe we shall never find out—nor is it necessary that we should. The "diarrhœa from one part of the bowel into another part," I willingly accept on Dr. Bristowe's testimony, as a fact—as something which has happened.—I should like to know the *dose* which did it. *How* it was done I do not enquire. So with Opium—the effects described in the Letter, again reveal the mistake common to the great majority of the Profession, as to what are *small* doses. Effects are spoken of which could not follow from what is meant in these Essays by small doses—such as "deaden- ing some of the higher mental faculties," &c. The dose of Opium, whose effects both in health and in illness are most familiar to me, is Opium 1 — that is one hundredth part of a drop of the Tincture. This does not, and one would think cannot, deaden the mental faculties; what it does is this—in health, it excites the brain—this, if continued, would soon become visibly injurious—in illness, it relieves coma even when it extends to some forms of apoplexy. Again, in health,

it slightly relaxes the bowels; in illness, it relieves torpid constipation. It is not meant that a single dose of this strength is always sufficient, it may be necessary to repeat it several times. Neither is it meant that Opium 1 is the only small dose. What Opium 1 does for me, Opium 1x (the tenth of a drop) will do for another. The contrary action of different doses has been again and again expressed in these words—"a certain *group* of larger doses, and a certain *group* of smaller doses." There is also a group of middle doses, which partakes of both the actions. And there are doses above this group of larger doses, and below the group of smaller doses, of which I say nothing.

A short time ago, a gentleman was visiting our Hospital at Rugby, when one of the Physicians came into the room, rubbing his hands and looking very much pleased. "You are happy—is it on account of your patients?" "On account of *one* of them," was the reply. "I have given him a *very, very* small dose of Opium for constipation, and it has succeeded in relieving him!" This physician has been for a long time a strenuous opponent; but he and others who have pursued the same course, may accept Antipraxy without feeling it to be discreditable to do so, and may often be made happy in this manner.

11. "The two conditions pass gradually into one another."—How can this be a reason why they were not previously opposite conditions? Day and night, light and darkness, are certainly opposite conditions, and yet they "pass gradually into one another," every twenty-

four hours. Look at the *heart*; at one time it is beating quickly, at another time it is beating slowly—that is, quicker or slower than when it is in a healthy condition. Are not these opposite conditions? And yet they necessarily pass into each other. Look at the *kidneys*, or any other secreting organ; at one time they are secreting more than is their function in health, at another time less. Are not these contrary or opposite conditions? And yet they may and do pass gradually from one into the other. Take the *thermometer*; does it not tell us that to-day the heat is above the healthy temperature, while yesterday it was below it. Surely, these are opposite conditions, and yet they pass gradually into one another. And so all round. And so, I think, we may safely say, that excitement and oppression of the brain—the former the effect of the smaller dose of Opium, the latter the effect of the larger dose,—are opposite conditions.

12. “This is certainly so with respect to insanity.”—Insanity is a most obscure subject, and its phenomena, and the causes and manner of their production, cannot, I think, be profitably introduced into the study of the action of drugs—at least in the present state of our knowledge, or rather of our want of knowledge, in respect to them. The excitement sometimes caused by *very large* doses of Opium is excessive, but it is not insanity, and in a Court of Justice would not be allowed to be sufficient to remove responsibility from a criminal. I once, a great many years ago, had to take charge of a young man, an Organist, who had

swallowed *an ounce* of Laudanum. There may have been at first, some stupor; I think it was taken for granted that there would be, and I undertook to walk about with him in his room all night, and did so. The stupor, if there was any, soon passed away, and he became excessively excited, but not insane; in a few days he recovered.

13. "Alcohol."—If we would avoid endless controversy and hopeless disagreement, one of the first requisites is a definition of words. In the earlier Essays this was frequently insisted upon, and in them was given a definition of what I understand to be the meaning of the word "drug." It is a substance which cannot be classed as *food*; moreover, its action on the healthy living body of man is more or less injurious, *in all doses*. Alcohol does not come under this definition—it is not injurious in all doses—and it has been repeatedly insisted upon that alcohol is not a drug, and that its action is not governed by the law of Antipraxy. Any objections, therefore, founded on the effects of alcohol, may be dismissed as not relevant.

14. "I am not maintaining that the explanation I have here given is the true one; but it is at least plausible, and at any rate illustrates the nature of my general views as to my inability to adopt your opinions;" in the next paragraph, "your hypothesis."—Here again the necessity for a careful definition of words comes to the front. "Opinion" and "hypothesis" seem to be used as synonymous terms, or nearly so; they may,

therefore, be dealt with together under the word "hypothesis."

The meaning attached to "hypothesis" in these Essays has uniformly been *conjecture of a special kind*—an opinion or guess in medical matters—without proof; for the moment it can be proved to be true, it ceases to be *hypothesis* and becomes *fact*. The difference between hypothesis and fact is, therefore, essential. Hypothesis admits of argument; fact does not—is independent of argument. Hypothesis rests upon probability; fact rests upon evidence. Hypothesis is content with being thought plausible; fact claims belief. Very many times it has been contended that *I have no hypotheses*; that which is stated, is believed to be *fact*. Of course, I may be mistaken, but no amount of *argument* can convict me of this; facts can be disproved only by the production of better *evidence* against them than that on which they rest, and which has made me believe that they are facts.

This subject is of so much importance that it will be worth while to pursue it a little longer. Let us listen to what some of the most talented men have said upon it.

Hippocrates has been anticipated (Essay XIX.). The paragraph quoted ends with these words:—"I have not thought that Medicine stands in need of an empty hypothesis."

Plato shows us plainly what *hypothesis* is. In the Dialogue of "Timæus," as has also been anticipated (Essay XLVIII.), the principle speaker, Timæus, a disciple of Pythagoras, begins at the request of Socrates,

with a prayer to God to rescue them (himself and the friends he is addressing) from the difficulties of the strange and unwonted inquiry into the Creation, in which they were engaged, and to bring them to *probability*. For, he says, phenomena are to be *reasoned out by the method of probabilities*. His reverence for God forbids him to go further than observation and thinking, for:—"he who should attempt to verify all this by experiment would forget the difference between the Divine and human nature." This "method of probabilities" is the foundation of *hypothesis*.

Aristotle saw the difference between fact and hypothesis very distinctly, for in the beginning of his "Topics" he describes it in these words:—"Things true and primary are those which obtain belief, *not through others*, but *through themselves*; for there is no necessity to investigate the "why" in scientific principles, but each principle itself ought to be credible by itself. Probabilities are those which *appear* to all, or to most men, or to the wise, and to them either to all or to the greater number, or to such as are especially renowned and illustrious." But with him it was—"video meliora, proboque; deteriora sequor." And so he gave himself up to the dialectic syllogism, which teaches how to argue, not from true, but from probable assertions. And he succeeded in thus misleading mankind.

Lord Bacon, on the other hand, encouraged us to *experiment*: for without experiments, hypothesis, that is, conjecture, must, as it did entirely for many centuries under the mastership of Aristotle, and as it does still to a great extent, be the miserable substitute for knowledge.

Harvey, as every one knows, was an experimenter. "I profess," he says, "both to learn and to teach anatomy, not from books, but from dissections; not from the positions of philosophers, but from the fabric of nature." He also shows us the true place and use of hypothesis—a "working hypothesis." After much time spent in observation and experiment, he says:—"I *began to think* whether there might not be a motion [of the blood] as it were in a circle. This I afterwards found to be true."—The hypothesis became a fact.

Sydenham, who now stands out, after two centuries from the publication of his writings, "as the great representative of the practical medicine of practical England," was constantly protesting against speculation, and urging the observation of facts. "It is clearly impossible," he says, "that a physician should discover those causes of disease [and he might have added, those actions of medicines] that are not cognizable by the senses, so also it is unnecessary that he should attempt it." Again: "I have ever held that any accession whatever to the art of healing, even if it went no further than the cutting of corns, or the curing of toothaches, was of far higher value than all the knowledge of fine points, and all the pomp of subtle speculations; matters which are as useful to physicians in driving away diseases, as music is to masons in laying bricks." I am inclined to speak in stronger terms than even these; music may entertain, without hindering the masons' work. Speculations, especially *explanatory* hypotheses, are a very grievous impediment to the progress of real knowledge.

It is indeed true that some clever men of our own

time would have us think that, even with experiments, we cannot advance beyond probabilities. They argue that, as mathematical demonstration cannot be applied to our experiments, there can be no certainty in them. It is plain that there is a method of reasoning which is suitable for magnitudes and numbers, which cannot be applied to chemical or therapeutical experiments; but it does not follow that certainty cannot be arrived at, with respect to natural phenomena, in any other way than by mathematics. I think that a "certainty" may be arrived at by observation and experiment, which is something very different from "probability"—if words are to have definite meanings.

Perhaps a few examples will not only illustrate my meaning, but help to prove its truth:—Two grains of Lead added to two other grains of Lead make four grains of Lead. Eight grains of Oxygen and one grain of Hydrogen combine chemically and the result is nine grains of water. A certain group of doses, that is, a certain quantity of Aconite acts on the heart and quicken its beats; a certain other group of smaller doses, that is, another quantity, acts on the heart and make its beats slower. These are *individual* facts.

Two and two of *anything* make four of that thing. When *any substances* combine chemically, they combine in fixed proportions by weight. When *any drug* acts on any organ of the body, it acts in opposite directions, in certain larger and smaller quantities. These are *general*, in the sense of universal facts, or as I have called them, *law-facts*.

When we are told that the first of the individual

facts, and the first of the general facts here mentioned, *are* facts; but that the rest are only probabilities, hypotheses, or guesses, I think it is a mistake. A chemist would be surprised if any one told him that the composition of water was only a probability. A physician well acquainted with the action of Aconite does not think it hypothetical.

Suffer me, therefore, to assert once more in the words of Sir Isaac Newton, that I frame no hypotheses. From the beginning of these Papers it has been stated, in the plainest language I could succeed in finding, that it is knowledge, not opinion, that is sought for; this knowledge must consist of facts, not of hypotheses; these facts must rest on evidence, not on argument; and the opinions of persons in authority must not be mistaken for evidence; the evidence must be such as any one who is willing, may see for himself. This only is the "recognition of the priority of *direct observation*, and its paramount supremacy to every thing else."

15a. "Now things which are really opposite or antagonistic to one another, *must* start from a common point, and proceed in opposite directions, the point of departure being a neutral point."

15b. "It certainly seems to me that if medicines have truly opposite effects when given in very small and in large doses, there *must* be some quantity of such medicines, half-way between the extremes, in which all medicinal and poisonous effects should be neutralised, and which, therefore, should be inert."

15c. "In other words, as I would venture to put it,

that there is a mean dose of every poisonous substance which is absolutely inert."

From the three-fold repetition of this thought, it is evident that the action or non-action of the middle doses, between the larger and the smaller groups, is the difficulty which presses with the heaviest force against the reception of Antipraxy, in the mind of the writer of the Letter. From this it may be concluded that the same difficulty will also strongly oppress other medical minds. I think it is in my power to remove this difficulty so thoroughly that, henceforward, it shall cause no further trouble to any one.

The only postulate required is that experiment may be taken refuge in. In this way, and in this way only, can troublesome and vain conjecture be escaped from. These experiments have already been made, and may be repeated by any one. It has been shown in previous Essays, that the middle region, or dividing line, between the group of larger doses acting in one direction, and the group of smaller doses acting in the contrary direction, there is another group of doses which—are not "neutral," nor "inert,"—but which *combine in succession the two actions*. These middle doses act first in one direction, and then in the opposite. For example: a dose of Aconite, less than that which only quickens the heart's action, and larger than that which only slows it, first quickens it for a short time, and then slows it for a longer time. There is no neutral dose. There is no middle dose which is inert. This, I think, is so clear, that no more will be heard of the objection.

16. "The cases of snake poisoning which you quote. *You there assume* that the small dose caused the blood to coagulate, while the larger dose caused it to remain fluid."—The assumption here referred to is not mine, but belongs to Dr. Lauder Brunton and Sir J. Fayrer. This, I think, is plain from the quotation from their Paper in the "Proceedings of the Royal Society," quoted in Essay XXVII.—the Essay on which the Letter is a comment. These gentlemen say they have arrived at the following conclusions:—

"The blood appears to remain *fluid* after death when a *large quantity* of the cobra-poison has been directly injected into the circulation, as, for example, into an artery or vein." . . . And "the blood undergoes partial or complete *coagulation* when a *small quantity* only of the cobra-poison has been injected into a vein or an artery." Upon this they make the following remark:—

"Why the admixture of a large and quickly fatal injection of the cobra-virus into the circulation of animals should produce comparatively permanent fluidity of the blood, or interfere with its ordinary coagulability soon after removal from the body or after death, and why the injection of a smaller and more slowly fatal quantity should interpose no obstacle to its speedy coagulation, are questions extremely difficult to account for or explain. We can only state the *fact* that, in the one case, coagulation occurs speedily, and in the other this coagulation is retarded or altogether prevented by some cause at present unknown." It is not wonderful that I should have thought this fact, as stated by two eminent men,

an example of Antipraxy. The contrary action of the larger and smaller dose was not an assumption of mine.

17. "But it is not your opinion apparently in reference to poisons."—I was not aware that anything had been written which could bear this interpretation. In my thoughts there has been no distinction between the Antipraxy of Therapeutics and that of Toxicology, beyond this—that in regard to the former, experiments have been so multiplied as to leave no excuse for doubt; whereas, in regard to the latter, further verification is necessary.

It will be seen from the Paper of 1875 (Essay XXVII.) on which the Letter is founded, that the thought was expressed in 1873 (Essay XXII.) as a suggestion. In 1875, it was repeated as a deduction from Antipraxy, and the remark was added, "there remains the duty of verifying it by experiment." Some experiments are given, as having been already made; among them, those of Dr. Lauder Brunton and Sir J. Fayrer; and it is said, "These experiments have been begun, and I have no doubt that, before any considerable time has passed, a sufficient number of proofs will be made known, and the principle now expressed will be established, not only as a consequence of a previous induction, but also as a practical fact." I think some of the recent experiments of M. Pasteur will be found to tell in this direction.

18. "Then I contend that the exhibition of 10 grains and one millionth of a grain in a single dose should be

inert.”—I think the proposal made in the paragraph of which these words are the close, is to be lamented—the only passage in this well-written letter, of which the same need be said. It will be seen that the experiment proposed is one of *changed conditions*. To add the millionth of a grain to 10 grains of Arsenic, to give them *together*, and then to expect them to act *separately*, is a proposition unworthy of the writer. An *antidote* presupposes the poison to have been *already* taken. To neutralise and to antidote, as scientific expressions, are not synonymous.

With respect to a dose of 10 grains of Arsenic, it will be remembered that writers on Jurisprudence, (Taylor, for instance), tell us that three or four grains of Arsenic are a fatal dose; ten grains, then, we may conclude would probably kill two or three people; whether the millionth part of a grain, given as an antidote, would have any power, I cannot tell; the experiment has not been tried. I hope no opportunity for it will ever occur. Mr. Taylor reports, from Dr. Burne, “the case of a young woman, who took in divided doses in three days [given her as a medicine], one-fifth of a grain of Arsenic. Symptoms of inflammation of the stomach, and alarming symptoms of a nervous character appeared.” This would have been a fair opportunity of seeing what a millionth of a grain, or even less, could do as an antidote. Or it might reasonably be tried in cases where larger doses than this patient had taken, have been swallowed.

19. “I am very sceptical whether any general laws

of value exist, or can be discovered."—The existence of such scepticism in the minds of observant and thoughtful men is the strongest possible proof of the desperate "chaos"—no better word could have been found—on which the art of medicine has existed up to the present hour. And if there were a just foundation for this scepticism, the same confusion must always continue to exist, for without law there can be no science.

But that "laws of value exist" ought not to be doubted for a moment. God has created all things, and necessarily must govern all His works. And He governs them by laws, so that nothing is left to chance. Assuredly, sober thought cannot but, sooner or later, arrive at this conviction. And next, these laws, or many of them, "can be discovered." It is true that God's thoughts and ways are immeasurably above our thoughts and ways; so that the "Why" and the "How" will commonly be out of our reach; but phenomena are under our observation, and it is possible for painstaking study to find out many of the laws by which they are governed. In proof, I will limit myself to two examples:—

What could have been more of a "chaos" than the countless varieties of *motion* which surround us on every side? And this, not only on the earth, but in the heavens. It might well have been thought hopeless to discover the laws of motion. But, thanks to the persevering industry of such men as Kepler, Galileo, and Newton, they have been discovered, and now are greatly admired for their simplicity and beauty. Look up into the sky, and there is a bright "wanderer"—*πλανήτης*—

and it has many companions, whose movements, sometimes in one direction, then in the opposite, sometimes quicker, and then slower, have puzzled men's minds for ages. These movements are no longer a "chaos," and the "wanderers" are seen to be moving in a stately dignity and a sublime regularity which are impressively awful.

The other example I will adduce is chemical combination. Until modern days this was another "chaos." Ingredients were put together without weight or measure, and nothing but confusion could exist. But, no sooner did men begin to examine *one thing at a time*, and to use the balance as Mr. Cavendish did, than appearances changed, and laws of combination were discovered. These laws are now seen to be full of beauty in themselves, and when we submit to their government, full of utility to us.

So of every other department of nature which has become a science. Meteorology and Medicine yet lag behind; but the former of these is beginning to catch up the successful runners in the race, and why should not Medicine do so too?

The causes of the present "chaos" in Therapeutics are mainly two. First, that hitherto, medicines have been given only to the *sick*; so that their action has been obscured by being mixed with the morbid actions of disease, which were already going on. And secondly, that one medicine has always been mixed with others, instead of being given alone; so that it could not carry on its own action, but was constantly interfered with by the struggle which others made to work in their own way at the same time. In the words of a

former President of the Royal College of Physicians (Dr. Paris), they are always "fighting together in the dark."

Now, it is most clear that both these mighty causes of chaos, which have been kept in operation for thousands of years, may be removed at any moment, when men choose to make the resolution. We can test medicines in health, and so learn their action uncomplicated by disease. We can test one medicine at a time, and so learn its action undisturbed by other medicines.

This being done—and how easy to do it!—we shall find not only that laws of value exist, but also that they can be discovered. "The character of the true philosopher is to hope all things not impossible, and to believe all things not unreasonable."* And the character of the true physician should not fall short of this.

20. "Doubtless, you will be able to adduce reasonable arguments on the other side."—*Facts*, it has already been said, do not rest upon argument, but upon evidence; they cannot be proved by any arguments however reasonable, they can be proved only by satisfactory evidence. I am therefore obliged to plead inability to adduce reasonable argument on the other side. Hypotheses may be attacked and defended by argument. I have no hypothesis to defend. My business is with facts, individual and universal, as these can be learned by experiment. When any thing is advanced, with me the first question is this—Is there evidence to prove it to be true?

* Sir John Herschel. *Discourse on Natural Philosophy*, p. 8.

21. "My own *Materia Medica* lies within a very narrow compass."—I think all the objections in the Letter have now been noticed and removed; and I thank the author for writing them. It does not seem to be within my province to criticise Dr. Bristowe's medical belief. I, therefore, gladly refrain from undertaking to do so. It is enough that the attempt has been made to set before him, and those who think with him, a truer and a better faith.

No doubt, I shall be reproached with having slipped away from the ground stood upon in the early Essays. That I have moved *forward* from that ground to another which is firmer, is freely acknowledged; but it is not confessed to be a mistake, nor to have been done from fickleness. "I am an old man, but I hope not too old to learn." These are the words of an admirable man with whom for a quarter of a century, I had the privilege and the happiness of enjoying a friendship of the warmest kind. We had many long and earnest conversations on the subjects of these Essays—which he always read with interest—and now I cannot do better than repeat his words:—"I am an old man, but I hope not too old to learn." There is an old proverb:—"A wise man changes his mind, a foolish one will not."

To conclude—my grateful thanks are again offered to Dr. Bristowe for his letter. If any thing has been said in this reply, which appears to be unkind, ungracious, or disrespectful, I heartily beg to be forgiven: there has been no such intention, but the contrary. If

any thing has been said which will induce him and his colleagues to reconsider their present knowledge, and to undertake the further experimental investigation of this practical subject—a subject, to the sick, important beyond expression—my labour will have had its reward, and God must have all the praise.









