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Contributors

Sharp, William, 1805-1896.
Royal College of Surgeons of England

Publication/Creation

London : George Bell and Sons, 1892.

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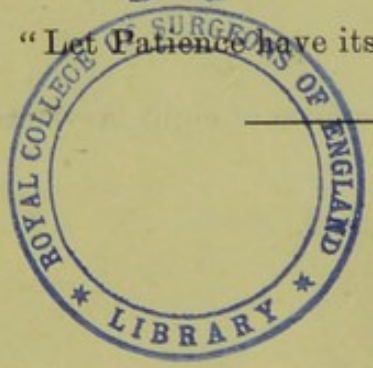
ESSAY LX.

A DRUG IS ITS OWN ANTIDOTE.

BY

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

"Let Patience have its perfect work."
ST. JAMES.



LONDON:
GEORGE BELL AND SONS,
YORK STREET, COVENT GARDEN.
1892.

ANALYSIS.

I. Introduction.

The ancient unscientific method in Therapeutics has continued to this day, proved by Dr. Francis Adams. What is Science, explained by Sir John Herschel. Science possible in Medicine: Three law-facts have been discussed; a fourth is now added. Its problem. Antipraxy imperfectly anticipated by John Hunter. Repetition of some of the defects in the present practice.

II. Experiments with larger and smaller Doses of

Titanium.
Aconite.
Chamomilla.
Ipecacuanha.
Mercury.
Belladonna.
Physostigma.
Ignatia.
Tartar Emetic.
Sanguinaria.

III. Conclusions.

These are stated in the form of eight law-facts.

IV. Reflections.

Explanatory and practical.

V. A Legacy.

ESSAY LX.

A DRUG IS ITS OWN ANTIDOTE.

"Knowledge is Power."
LORD BACON.

I. *Introduction.*

DR. FRANCIS ADAMS, the scholarly translator of the best Greek medical literature which has been handed down to us, in his Preface to the extant works of Aretæus, says of him:—"His system of treatment can scarcely be too highly commended. . . . Even at the present day, when judged of impartially, it will be difficult to convict him of having in any single instance laid down erroneous rules of treatment. And not only are his general principles in Therapeutics to be admired, but also the skill and taste with which he reduces them to practice. For, in general, it will be admitted that it would be difficult to accomplish the fulfilment of the indications laid down by any more appropriate means than those he makes use of. . . . Moreover, there is scarcely a single class of remedies presently in use from which he does not at one time or another draw certain specimens, from the simplest and most delicious of the culinary preparations up to the most potent resources of Surgery and of Pharmacy. The fermented juice of the grape, and other savoury potations; acids, bitters, astringents, carminatives, narcotics, diuretics, emmenagogues, cathartics, and emetics; soap to cleanse the skin, and cantharides or mustard to stimulate it; the lancet, and in extreme cases the actual cautery; such are the remedial means recommended and employed by the learned Cappadocian with admirable skill and discrimination, to combat the multifarious symptoms of disease which he himself has so graphically described."*

* *Aretæus*, by Francis Adams, LL.D. pp. xiii, xiv. Sydenham Society's Edition.

This is a long quotation, but it serves an important purpose; it suffices to show that the Medical Profession, since its commencement by Hippocrates more than two thousand years ago, has had but *one method* of treating its patients; with only such changes of theory and practice, as the pendulum of fashion swinging backwards and forwards within its own limits, would permit. The same method has been persisted in, and the permanent character of this method has been *the absence of Science*. There is another and a better, but as yet an untrodden path.

Sir John Herschel, in his interesting and instructive "Discourse on the Study of Natural Philosophy," explains very clearly what a law-fact, or as it was then called a general fact or law of nature, is; and what practical use may be made of it. It would be well if every medical student would make himself acquainted with this book. The following are a few of its sentences:—

"We must never forget that it is principles not phenomena—laws not insulated independent facts—which are the objects of enquiry. As truth is single, and consistent with itself, a principle may be as completely and as plainly elucidated by the most familiar and simple fact, as by the most imposing and uncommon phenomenon." To the student of nature "there is no natural object unimportant or trifling, from the least of nature's works he may learn the greatest lessons." "Accustomed to trace the operation of general causes, and the exemplification of general laws, in circumstances where the uninformed and unenquiring eye perceives neither novelty nor beauty, he walks in the midst of wonders; every object which falls in his way elucidates some principle, affords some instruction, and impresses him with a sense of harmony and order." "We may regard a law of nature . . . . as a proposition announcing that a whole class of individuals agreeing in one character agree also in another . . . . or as announcing a relation between two phenomena . . . . viz. the relation of *constant association*, inasmuch as it asserts that in whatever individual the one character is found, the other will invariably be found also."

Of the practical use of such laws of nature, he says

—"We may consider the great importance of a knowledge of them to mankind,—

(1.) In showing us how to avoid attempting impossibilities.

(2.) In securing us from important mistakes in attempting what is in itself, possible, by means either inadequate, or actually opposed, to the end in view.

(3.) In enabling us to accomplish our ends in the easiest, shortest, most economical, and most effectual manner.

(4.) In inducing us to attempt, and enabling us to accomplish, objects, which, but for such knowledge, we should never have thought of undertaking." So that "if the laws of nature, on the one hand, are invincible opponents, on the other, they are irresistible auxiliaries."

When mathematical calculations can be applied, as in Astronomy and Optics, for the solution of the problem, the conclusion arrived at is called a demonstration, and is accepted without hesitation as true. When this is not possible, as in Chemistry and Medicine, certainty is arrived at by another process, called by Sir John Herschel, "the verification of predictions." How can this process be adopted in Therapeutics? It may be adopted in this manner:—A number of substances varying extremely in many of their properties, are found to agree in this, that when taken in any or all doses by a healthy person, they act injuriously upon him. To this class of substances the name of *Drugs* has been given. The definition manifestly excludes all substances which are food, or pure stimulants as alcohol, or are inert; but it includes some substances which are inert in their crude state, but which, when greatly subdivided by trituration or solution, become injurious. This common property of injuring our health is, then, a law-fact, and in a student of nature, curiosity is excited to learn what other law-facts may also belong to these substances. Experiments have shown that many of them—that is, all that have been experimented with—have the property of acting locally or organically, upon some parts of the body in preference to other parts; then the prediction is, that every other drug, when experimented with, will act in like manner. Again, experiments have shown that many drugs—that is, all that have been so experi-

mented with—act in contrary ways in different doses; and the prediction is that all other drugs, when experimented with, will act in like manner. Here are two law-facts added to the first, and they appeal, as predictions, to be verified by further experiments; "the grand and indeed only character of truth being its capability of enduring the test of universal experience, and coming unchanged out of every possible form of *fair* discussion." (Herschel.)

An essential feature of our study of natural objects is a classification of them, and an essential feature of a class is that all the objects put into it have *something in common*—one property belonging to each of them—they may differ widely in other properties, but unless they agree in possessing one, which is the same in all, the class is faulty and worthless. Let this requisite be applied to drugs, and let it be seen that nothing is classed as a drug which does not resemble other drugs in having the one property belonging to them; having this, it will be found to possess the others also that are common to them all.

The word "science" is often carelessly used instead of the word "knowledge." It has been shown in former Essays, that the word should be restricted first to mathematics, and next to a combination of observation, experiment, and inductive reasoning, spent upon the phenomena of nature until law-facts are discovered, from which fixed and permanent rules of working may be deduced. Natural Science is mathematical or physical.

In the future it will be seen that the physician's first duty is to understand that his Profession is a Science. Through past time it has been called *Ars Medica*, the Medical Art, and it has been universally practised as such. The youth of Medicine has lasted long enough, and the time must have arrived when men will study and practise medicine, not as an Art but as a Science. It is high time for the beggarly and often mischievous elements, on which it is yet founded, to be cast away, and for a foundation to be sought, and sought till it is found, much more satisfactory and stable.

Such is a statement of the case in hand mainly in the language of two men of acknowledged great ability

and information. The importance of the subject will justify a re-statement of it in other words :—

The first and ruling thought throughout these Papers is the conviction that all the phenomena of diseases and of drug-action are governed by laws or law-facts. It has not been difficult for men to think that when God created the heavens and the earth, and saw that all that He had made was very good, that *then* all created things were subjected to the government of laws. Right reason teaches us this independently of Revelation. But it has been difficult, and it is difficult still, to think that the disorders and confusions brought into the world, and still continued in it, by sin, should also be under the government of laws. Nevertheless, it is certainly true that all these distressing irregularities and disorders, including diseases and their remedies, have been thus subjected by the Almighty Ruler—they are all under law.

A sufficient proof of this is, or ought to be, manifest to every one on asking himself this question,—How can any help be derived from the observation of the phenomena of diseases and the action of remedies, if these are not regulated by fixed laws? For example :—Of what use would it have been to Hippocrates, and to all ages since his, that he depicted the "Hippocratic countenance"—death in the face? Of what use would it have been to the last generation to have taken so much pains to notice and attempt to describe the many changes in the pulse? Of what use is it now to be so careful in measuring the temperature by the thermometer? All the symptoms thus so carefully noted, are effects of the "disorderly" conditions of disease, and yet they are insisted upon as reliable guides in diagnosis, in prognosis, and in treatment. How could this be if they were all irregular, casual, and lawless? Certainly, in that case they would be no guides at all.

The last paragraph suggests a remark which may be introduced here in a parenthesis. It is worth while to notice the manner in which the single symptom of *temperature* is at present made so much of—a symptom limited to what the thermometer can tell us about it, and that is only as to less or more—a very narrow range. Formerly the pulse was the great object of study; that again was only one symptom, but it had

a much wider range than being merely quicker or slower; our fingers were carefully educated to notice a great deal about it besides that. Hippocrates, who is boasted of as the head of orthodox medicine, knew nothing about either the thermometer or pulse, but the symptom he studied most had much more of information in it than can be obtained from either thermometer or pulse—he studied his patient's countenance, and it would be difficult to find among any of his successors, a physician more skilful in his diagnosis and prognosis. Would it not be wiser if we could give up riding upon our present fashionable hobby? A maid-servant may be quite on a level with a physician in "taking the temperature"; and, which is much more serious, when a high temperature in any "low" fever leads to the giving of *Aconite* to reduce it, it leads to a very grave mistake. At all times there will have been a few individuals whose good sense will have preserved them from being carried away by the tide of the prevailing fashion, but no one will deny that a fashion of some kind has always made itself conspicuous.

The continuity and uniformity of the phenomena of disease, and of the action of drugs, from the time of Hippocrates down to our own time, is a sufficient proof that they are governed by laws. And now, another, and independent, and sufficient proof can be advanced, namely, that some of these laws have been discovered.

In the foregoing Essays three of these laws have been discussed and illustrated in considerable detail, and the object of the present endeavour is to explain a fourth.

The condition of Toxicology, as regards its most important element, the treatment of cases of poisoning with antidotes, is not a creditable one. If a thoughtful medical man will pursue the subject attentively through any number of active poisons, and note the antidotes prescribed—omitting, of course, as having their own independent value, mechanical and chemical means—he will see that everything is empirical and haphazard, and that it is no exaggeration to speak of the condition of Toxicology as not creditable.

Then it cannot be called presumption to look around hopefully for some method of improvement; it must be

a method not hitherto tried, if a real improvement is to be made, and possibly, some principle or law-fact may be discovered, which shall introduce clearness and accuracy, and exchange empiricism for science.

In the last Essay it was attempted to be shown that, for a question in science to be successfully answered it must be put into the form of a problem with known and constant elements, and with only one unknown quantity to be found. The problem thus discussed in that Paper was to find what happens when a healthy body, one drug, and one dose, are put together, and that same dose is any number of times repeated.

The problem to be examined now consists of four elements—a healthy person, one drug, and two doses, one a larger the other a smaller dose; and the question to be answered is,—What happens when first the larger of these doses is taken and then, after an interval sufficient to allow the effects of that dose to appear, the smaller dose is taken? The experiments made to solve the problem of the last Essay threw rays of light into what has hitherto been a dark chamber of Therapeutics, and we may hope that experiments made to solve the present problem will increase that light.

In Essay XXII, published eighteen years ago (1873), in which it was shown that the action of some smaller doses is contrary to that of some larger ones, it was said:—"This suggests the idea that, for the virulent poisons, such as *snake-venom, arsenic, opium,* &c., for which no antidotes are yet known, the best antidote *may be* very small doses of itself." The only example that could then be given was a case of poisoning by *mercury* in which the third trituration of mercury (the millionth part of a grain) "was manifestly beneficial."

In Essay XXVII, on "A scientific principle for Toxicology," published in 1875, it is said this idea "is repeated not as *a suggestion* but as *a deduction* from the contrary action of different doses. . . . It is a principle by which the use of medicinal antidotes may be regulated. . . . As a principle it has been learned by reasoning. . . . There remains the duty of verifying it by experiment." And a reference is made to some experiments with the poison of the *cobra*

by Dr. Lauder Brunton and Sir J. Fayrer, which have, among their conclusions, the two following :—

"The blood appears to remain *fluid* after death . . . when *a large quantity* of the cobra poison has been directly injected into the circulation." And, "The blood undergoes either partial or complete *coagulation* . . . when *a small quantity* only of the cobra poison has been injected into a vein or an artery."

Sixteen years have elapsed since these remarks were made; the subject has lain dormant until other matters more pressing were disposed of; the time has arrived for its resumption, and the question is now proposed, not as a suggestion, or hypothesis in the useful sense, nor as a deduction reached by reasoning, but as a fact proved by experiment.

In Essay LII—"Therapeutics ought to become a Science," published in 1888, in the portion devoted to the contrary action of groups of larger and smaller doses of the same drug, and which is called *Antipraxy* (contrary action), the following criticism is quoted from a letter by a University Professor of Pathology :—

"May I remark that the doctrine of Antipraxy is opposed to all that we at present know regarding the action of remedies, and that you do not bring any evidence that is worth a moment's consideration in support of it?"

To-day (Sept. 2, 1891), while reading the 16th of JOHN HUNTER'S "Lectures on the Principles of Surgery,"* a paragraph was met with which excited the liveliest interest; it is as follows :—

"Medicines affect the living principle by their stimulant or sedative qualities only, unless they act chemically as caustics; they have the effect of irritating or quieting, and these two contrary effects are generally to be found in the same substance, the difference resulting from the difference of quantity; thus a small quantity of *heat* relaxes and softens, but a larger quantity will stimulate. A stimulus is what either causes a natural action, or increases one which has already taken place, or excites an action contrary to one present, as by giving an opiate when a patient cannot sleep. Medicines may either stimulate or irritate; such as increase the natural

* *Works of John Hunter*, by J. F. Palmer, 1835, vol. i, page 477.

power of acting are stimulants; such as excite new and unnatural actions are irritants. The powers of both will be according to the medicine and the parts combined; for fixed air in the stomach increases its action; in the lungs it destroys action," &c.

The paragraph has in it almost as many inconsistencies of expression as it has sentences, but the only sentence calling for our notice at present is this:— "Medicines have the effect of irritating or quieting, and these two contrary effects are generally to be found in the same substance, the difference resulting from the difference of quantity; thus a small quantity of heat relaxes and softens, but a larger quantity will stimulate."

This is Antipraxy imperfectly announced—imperfectly because the word *generally* is introduced into the announcement, this is an admission that there are exceptions; consequently, the fact as stated by John Hunter is not a law of nature; it will also be noticed that the individual fact adduced in proof is irrelevant—*heat* is not a medicine. It would appear that the thought was a passing one, and that it was not followed up, as was John Hunter's wont, by experiments; still it is a suggestion of Antipraxy, which ought not, for more than a hundred years, to have escaped observation. It is due to truth, and to the memory of John Hunter that the discovery made to-day should be acknowledged, and it is done at once with alacrity and pleasure.

There is much dissatisfaction in the minds of many medical men with the present condition of medical practice; in the minds of a few this dissatisfaction amounts to a conviction that there are serious errors in this practice; among these errors are the following:—

(1). The mixing of drugs in prescriptions. There is still a great mystery in the properties of drugs as medicines, and it is beginning to be perceived that, to become acquainted with these properties with any degree of accuracy, they must be prescribed singly or alone, so that the action of each drug may be studied by itself.

(2). Another conviction is also beginning to penetrate the medical mind, namely, that these mysterious properties of drugs can never be satisfactorily learned

by experiments on the sick only; and that it is imperative for experiments to be undertaken with them on persons in health.

(3). The conviction that similar necessities, and with as much urgency, belong to the study of the different *doses* of each drug must follow the two preceding convictions. The random changing of doses, so common at present, will be found by careful experiment to be a huge blunder.

(4). It follows obviously that these experiments on the healthy, both with drugs and with doses, must be made upon ourselves, and that experiments on the lower animals must be abandoned, because they can never give us that true knowledge of the action of drugs which we ought to have, *before* we prescribe them as medicines for the sick.

(5). Another error will be avoided when the Divine laboratory of nature is depended upon, rather than the laboratory of the chemist. When the sap of plants, or a tincture from their bark or their roots, is prescribed, and not the "alkaloids" obtained from them. For example, it is certainly a mistake to assume that the actions of *Nux vomica* and of *Ignatia amara* may be estimated by the percentage of *strychnine* they respectively contain. If these chemical products are to be prescribed they must first be experimented with in health as new drugs.

(6). There is a practice common in both schools, of giving certain drugs in large doses under the title of *food;* this also is an error. When an ingredient of healthy blood is absent from it or is deficient in quantity, the cause is not that the ingredient is not contained as usual in the ordinary food, and must be supplied in a medicine; the absence or deficiency arises from imperfect digestion or assimilation of the food taken. What is wanted is not the supply of a larger quantity of the absent element, but an improvement in digestion or assimilation, and this improvement should be the physician's object—*if he knows how to accomplish it*—in prescribing. In the homœopathic school this is a double error; in addition to the error just noticed it overlooks the fact that the action of the small dose given as a remedy is interfered with by the large dose given as food.

Assuredly, all these are subjects worthy of the serious consideration of physicians; not to know how to perform the duties of our office is a sad calamity: what is it to be unconscious of our ignorance?

II. *Experiments.*

The first experiments made relative to this subject were described sixteen years ago in Essay XXVII (1875), with the title—"A Principle for Toxicology." Since that time other subjects have put this aside, and it has lain dormant. It deserves more consideration, and the present Essay is a second contribution towards its elucidation.

An experiment tried thirty-five years ago (1856) goes as far to establish the truth of the statement made in the Title of this Essay, as it is possible for one experiment to do. The first half of it has received a very remarkable confirmation from a most unconscious source in the experiment made on himself by Mr. Percy Richards, as recorded in Essay LVII. Without knowing anything of the previous experiment, he took the same dose, namely, two grains of the first centesimal trituration, (the fiftieth part of a grain of the cubic crystals of Titanium); this caused the immediate appearance of albumen in the urine, for the first time in his life, and which continued for three months before he could get rid of it. The second half of the original experiment is confirmed by a series of experiments made very recently with other drugs, and now carefully reported.

Experiments with Titanium.

Its local or organic action.
Its contrary actions in a larger and a smaller dose.

Dr. Sharp.—1856. Two grains of the first centesimal trituration of the cubic crystals of *Titanium* were taken once a day for a week. The effects, as written that year, were summed up as follows:—"He became greatly disordered, and felt and looked wretchedly ill. The action of the drug was upon—

1. The *stomach;* bringing on nausea, loss of appetite and feeling of discomfort.
2. The *brain* and *nerves;* giddiness, imperfect vision,

the peculiarity being that *half an object* only could be seen at once, desire to keep the eyelids closed.

3. The *blood;* a perceptible derangement of the system, which could not without danger have been carried further."

It was afterwards discovered that a very copious escape of albumen was going on in the urine, and as a consequence a stage of great nervousness and debility ensued. This continued for *two years* in spite of visits to the sea-side and every other remedial measure that he could think of himself, or that could be suggested by a very able physician—Dr. Sutherland, then of Leamington. At the end of this time it occurred to Dr. Sharp to try a smaller dose of the poison itself, and he took a few doses of the second trituration of *Titanium* (the ten thousandth part of a grain). The albumen disappeared in a few days, and in an incredibly short time he was restored to his usual health. Neither before this experiment, nor since it, has albumen been found in the urine. The following letter will show what are Dr. Sutherland's recollections of this illness:—

"Edinburgh, 17 June, 1891. My dear Friend, As your inquiry recalls to me your condition in 1856–7, I have thought it over. I found you at that time in a very serious condition—weak; reduced in flesh; and evidently the victim of Bright's disease, so far as *albuminuria* is a sign of that ailment, and which was abundant. I suggested *Mercurius corrosivus*. But I had such an unfavorable opinion as to the probable result that I hinted the danger to Mrs. Sharp. I have on two occasions found albuminuria present under the influence of mercury, and on both it disappeared on the mercury being given up. It turned out in your case that the Merc. corr. made no impression on the secretion as to quantity or deficiency of density (on the usual tests). The idea that you had brought on the ailment by experimenting with *Titanium* was your own suggestion, as well as the small doses which you so successfully brought to bear upon this serious condition. I am, my dear Friend, yours affectionately, J. S. Sutherland."

It has been said already, but the value of these facts is sufficient to justify its repetition, that it can scarcely be thought possible for a law-fact of nature to be more emphatically proved by a single experiment than is this—

that a drug is its own antidote—by the experiment here described.

The treatment of the illness caused by Titanium, from which Dr. Sharp suffered so much, was in the skilful hands of Dr. Sutherland; notwithstanding this, it is evident that Homœopathy failed, while the first trial of Antipraxy succeeded immediately and perfectly in a most surprising manner.

Experiments with Aconite.

Its local or organic action.
Its contrary actions in a larger and a smaller dose.

Dr. Applebe was requested to test Aconite in a larger and a smaller dose.

"Oct. 3, 1891. My dear Dr. Sharp. Last night I tried the following experiment:—

At 7 P.M., two hours after taking tea, I counted my pulse; it was 68 in the recumbent posture. I took 10 drops of Tincture of Aconite (B. P.).

At 8 P.M. pulse 76; face flushed; slight headache. I took one dose of Acon. 1, one drop (first centesimal dilution).

At 8.30 pulse had slowed down to 72. Repeated dose.

At 9 o'clock pulse was 64. Third dose taken.

At 10 P.M. pulse 60. No dose.

11 P.M. pulse 70."

Second experiment.

"Now, this evening at 5 o'clock my pulse was 66; I took 10 drops of Tincture of Aconite (B. P.).

At 6 P.M. pulse 78; face flushed; headache. Took no antidote.

At 7 P.M. pulse 80. No antidote.

At 8 P.M. pulse 86; a good deal of congestive headache. Took one drop of Acon. 1.

At 8.30, pulse back to 76.

At 9 P.M. pulse 74. Another dose of Acon. 1.

At 9.30, pulse 70; headache *gone*.

Am writing this letter; shall count pulse again at 10 o'clock.

10 P.M. Pulse just 70. Feel none the worse for experiments."

Surely, these experiments are good proofs that a drug

is its own antidote. They should be studied in connection with those made with *Aconite* and reported in Essay LVII. Experiments made by six provers are given in that Essay; those by Dr. Applebe ought to be specially noticed along with the above; he says:—

"October, 1889. My normal pulse in recumbent posture at 7 P.M. to-day is 64. *One minim* of Tinct. Acon. (B. P.) in ten minutes reduced the pulse to 60; at 8 P.M. pulse again 64"—*one action.*

"*Three minims* reduced pulse in five minutes to 60; in half an hour pulse was 68; in one hour pulse still 68; in two hours 64"—*two actions.*

"*Six minims* taken, in five minutes pulse was 66; in half an hour pulse 70; in two hours pulse still 70; in two hours and a half pulse 68; four hours pulse normal (64)"—*one action.*

The larger doses taken in the present experiments (1891) also had only *one action.* They were antidoted by a smaller dose which has only *one action.* The intermediate doses with *two actions* do not form part of the subject of this Essay.

Mr. Gerard Smith's remark on the 15 *minims* allowed in text-books to be given to patients, ought not to be forgotten:—"Dec. 11, 1889. I have seen *fifteen minims* of the B. P. Tincture of Aconite given as a dose; it does not kill a man, but it produces a slightly delirious state; it does not produce perspiration; and the well-known vascular relaxation of the lesser doses is absent."

In taking ten drops instead of ten minims it would appear that Dr. Applebe had forgotten the details of his former experiments. This makes their agreement in action all the more remarkable.

With Dr. Applebe one minim of the Tincture of Aconite lessened the frequency of the heart's action in ten minutes by four beats, and did nothing else. Three minims lessened the action in five minutes by four beats, and afterwards quickened them by four beats above what they were at first. Six minims quickened the pulse from the first, in five minutes by two beats, in half an hour by six beats, without any previous slowing. Ten drops raised the action of the heart in three hours by *twenty beats,* without any previous slowing. The antidote being then taken it was quickly reduced to its healthy condition.

One further remark must be permitted. The experiments reported in Essay LVII indicate a group of doses of Aconite reaching from half a drop of the $\frac{1}{100}$ part of a drop up to one minim of the Tincture, as lowering the pulse only. The one minim dose was Dr. Applebe's. It is very interesting to observe that in his experiments now related the one hundredth part of a drop reduced the action of the heart still more powerfully, though it had been quickened by a larger dose of the same poison.

Experiments with Matricaria Chamomilla.

Its local or organic action.
Its contrary actions in a larger and a smaller dose.

Mr. Beverley in March last experimented with *Cham.* 1 (first centesimal dilution), in doses of one drop. The result was free evacuations of bilious motions. (See Essay LIX.)

"1891, Oct. 11. Being quite well, I took 5 minims of the strong tincture of *Chamomilla,* at 5 o'clock in the evening. The only effect observed was that my pupils, which at the time the drug was taken were large, in about an hour and a half afterwards had become a little contracted ; this contraction gradually passed away with the action of the drug as I thought. At 10 o'clock the same evening, I again took 5 minims, and afterwards went to bed. I was awakened by sharp, darting pains through the lower portion of my chest, and a feeling of sickness came over me in the intervals of the pains. After breakfast I went to stool, the motion was small, and caused some pain in the lower bowel ; again after dinner I was obliged to go to stool with a similar result, only with more pain ; after this I had a headache chiefly in the forehead, and a feeling of depression came over me. About 4 o'clock on that evening (the 12th) I took 1 minim of *Cham.* 1, and about three quarters of an hour later, I took another similar dose with the result of a good motion, which was rather relaxed ; in less than an hour and a half the headache went away, the pupils became natural, and I felt quite myself again."

Second and Third Experiments.

"Oct. 15. In a second, and a few days later, in a third experiment, I find the drug acts in exactly the

same way that I have described in my first experiment. When taking 5 minims of the strong tincture at bed-time, my pulse was 76 in a minute, in the morning it was 72. I noticed that bile was *decreased* by the strong tincture; the motions were very light-coloured, offensive and watery; after taking the centesimal dilution, in the second experiment 18 hours after the large dose, and in the third experiment 13 hours after (a dose of 1 minim of *Cham.* 1) the bile was *increased abundantly,* the motions soon afterwards becoming natural. In the second experiment the pulse became normal—76—in about two hours; in the third in about three hours, and I felt well."

These experiments are sufficiently instructive. The antidotal power of the small dose against the injurious action of the large one is manifest. Mr. Beverley was asked to notice the pupil because, in Hahnemann's provings it is said of the Eyes, "The pupils became dilated. Contracted pupils." As usual we have these contrary effects given without a hint as to the different doses which caused them.

Experiments with Ipecacuanha.

Its local or organic action.
Its contrary actions in a larger and a smaller dose.

Mr. Beverley says in a letter dated November 9, 1891: —"On Nov. 7th I took *half an ounce of Vinum Ipecac.* at about 5.30 p.m. (an hour after having tea). In less than a quarter of an hour I was *very* sick. As an antidote I tried *Tinct. Ipecac.* 1 (the first centesimal dilution) in one drop doses. The first dose, taken about 6 p.m. did not seem to delay the vomiting much if any. The second dose, taken ten minutes later, did alleviate the unpleasant symptom to some marked degree. The third dose, taken ten minutes after the second, stopped all vomiting, but left me in a state of depression. I then went to bed, and the next morning felt quite myself again except that I had rather a bad headache, which disappeared about dinner-time." In another letter dated Nov. 19th, Mr. Beverley says, in reply to enquiry, "Between the first and second of the small doses I was sick very little, but vomited a good deal. Between the second and third doses I was not sick at all, only

vomiting kept on. After taking the third dose I was myself again." He was well in twenty minutes after taking the first dose of the antidote. It seems certain that the action of this powerful emetic was stopped sooner and more suddenly than it would have subsided of itself, if it had been left without interference. Three doses, each containing the hundredth part of one drop of the tincture, antidoted the vomiting caused by half an ounce.

Experiments with Mercury.

Its local or organic action.
Its contrary actions in a larger and a smaller dose.

Mr. Seabroke.—September 4, 1891. He had been previously asked what aperient he may occasionally have taken, and had mentioned three or four grains of *blue pill.* He called this evening to tell me that, last night on going to bed, he took six grains of *blue pill;* early this morning he had a motion distinctly showing the action of the mercury, and he then took two drops of a solution of *Merc. Viv.* 3 (a small quantity of the third centesimal trituration of mercury dissolved in proof spirit), and the feeling in the bowels caused by the blue pill quickly passed away, and there had been no more motion. He has had a slight headache to-day, as if the liver had been disturbed. He will repeat the experiment in a week.

Second Experiment.

September 18th.—"On Thursday, Sept. 10, he took eight grains of *blue pill* (*Pil. Hydrarg.*); on Friday morning, had gripings and a loose evacuation, and he then took two drops of the *Merc.* 3, as before; had no more inclination to a motion till afternoon; then felt a grumbling, and took a second dose; the feeling passed away. On Saturday morning, as a busy day was coming, he took two drops more, and had no motion at all that day. On Sunday he was as in health. He considers these experiments entirely confirmatory of the fact under examination." It will be noticed that the quantity of metallic mercury taken in the first experiment was two grains, and in the second nearly three

grains, and that each dose of the antidote contained only the one millionth part of a grain.

Experiments with Belladonna.

Its local or organic action.

Its contrary actions in a larger and a smaller dose.

Mr. Haslam was requested to dilate both pupils by the topical application of *Belladonna*, and then to apply a smaller dose to one eye, but not to the other, in order to compare the time required by each pupil to become natural. It will be seen that this object was frustrated by the difference in the sensibility of the two eyes. To apply the small dose to one eye would have been fruitless.

"Wednesday, Septr. 9, 1891.—Painted round each eye with Tincture of *Belladonna* (strong Tincture), taking care to apply it equally to each eye, and watching the pupils all the time. Continued this for 28 minutes, during which time I noticed no alteration of which I could be certain. Between 40 and 50 minutes after commencing, dilatation of the left pupil had clearly begun, but there was no change in the right. In an hour after commencement the diameter of the left pupil had enlarged in the proportion of 5 to 3, and subsequently became slightly larger still. But there was no apparent change in the right pupil until at least two hours after commencement. About 5 hours after commencement the right pupil was nearly as large as the left, but as far as I could observe it was never so much dilated; and 48 hours after commencement, the left pupil was still quite perceptibly dilated, while the right did not differ perceptibly from its normal size. Sixty hours after commencement (perhaps sooner), both pupils were of normal size again. During the greatest dilatation it was necessary to remove any object to a distance of more than two feet from the eye to get anything like a sharp image on the retina.

On Saturday, Septr. 12, I found on rising that I had sciatica. In the evening it occurred to me that this might be caused by the large dose of Belladonna absorbed through the skin, and I began taking one drop doses of *Bell.* 1, three times a day, I felt no sciatica after the following Wednesday."

The next proposal came from Mr. Haslam himself, namely, " (i) to apply the strong tincture to both eyes and to observe carefully the time taken by each pupil to return to its normal condition without any application of a small dose; (ii) again to apply the strong tincture to both eyes and when dilatation had been clearly established to commence applying a small dose to both eyes, and to observe and compare with the times in (i), the time now taken by each pupil to regain the normal size." This proposal was gladly accepted, and the following is the result :—

(1) An accurate measurement of the iris and pupil of each eye was taken, when the diameter of the iris being reckoned 100, that of each pupil was 30.

" Wednesday, Nov. 25, 1891. 8, P.M. Applied 20 minims of Tinct. Bellad.	R.pupil = 30. L. = 30.
Thursday, Nov. 26. 6, A.M.	R. = 36. L. = 48.
Much discomfort in seeing, especially between 9.30 and 10, A.M.	
„ 8, P.M.	R. = 36. L. = 48.
Friday, Nov. 27. 6, A.M.	R. = 33. L. = 40.
„ „ 8, P.M.	R. = 33. L. = 40.
Saturday „ 28. 8, A.M.	R. = 30. L. = 30.

(2) *Second Experiment.*

Wednesday, Dec. 2. 8, P.M. Applied 20 minims of Tinct. Bell.	R. = 30. L. = 30.
Thursday, Dec. 3. 6, A.M.	R. = 36. L. = 48.
Began applying small dose (1 in 5). After this distinctly less discomfort in seeing.	
„ „ 8, P.M.	R. = 35. L. = 40.
Friday, Dec. 4. 6, A.M.	R. = 33. L. = 40.
Application of small dose continued.	
„ „ 8, P.M.	R. = 33. L. = 40.
Saturday, Dec. 5. 12, noon	R. = 30. L. = 35."

If the results of these two very careful experiments are compared, it will be observed that the contraction of the left pupil (the more sensitive of the two) was rapid after the first application of the smaller dose; and if the application had been then discontinued the contraction would have gone on. By continuing it the contraction was rather retarded than hastened. The small dose was one part of the strong Tincture and four of water—a dose strong enough to have two actions, which sufficiently explains the retardation.

Experiments with Physostigma.

The action of the Calabar bean on the pupil is stated to be the reverse of that of Belladonna. This has been found to be so in experiments mentioned in Essays XXI (1870), and XXV (1874). But the action of Physostigma was not nearly so powerful as that of Belladonna.

Dr. S. H. Ramsbotham (Leeds), and Mr. Haslam (Rugby), were asked to test the Calabar bean in the same manner as Belladonna. Dr. Ramsbotham writes :—

"19 Sept., 1891. I spent some time this afternoon with Physostigma, but I cannot report any result. I rubbed half a drachm of the *Tinct. Physostigma* round about both eyes, occupying nearly a quarter of an hour in the process. Then I went on with my ordinary letter writing with a mirror in front of me, in which from time to time I examined my eyes. When in the course of an hour I could not safely say I saw any difference in the pupils, as they seemed just the same size as when I began, and my sight continued as clear as before. After some time longer there was still no change in the pupils. Perhaps, as I am myopic, the drug may not have much effect upon me."

Mr. Haslam writes :—"I am sorry to say that my experience with *Physostigma* has been similar to Dr. Ramsbotham's. I could see no effect at all on either pupil."

Experiments with Ignatia amara.

Dr. H. Wynne Thomas (Bromley, Kent). "Age 26; fair; rather inclined to constipation, otherwise have very good health. I smoke a little, but stopped from Oct. 27 to Nov. 1; almost a total abstainer."

"Oct. 29, 1891. Dinner at 1.30, light tea at 5; bowels acted naturally at

6.0 P.M. for the second time to-day.
7.15. Took 30 minims of *Ignatia* (strong Tincture) in 1 ounce of water.
8.15. Took 50 minims in 2 ounces of water.
9.0. No symptoms yet; had supper.
9.45. Called out to patient; back of neck feels stiff, legs also stiff and rather inclined to give way; felt rather dazed.

Oct. 30. Very good night; breakfast at 8 (a stuffy cold in head from driving yesterday in dog-cart, coming on two days).

12.30. Took 30 minims in water on empty stomach (no food since breakfast).

1.20. Took 30 minims again; no symptoms.

1.35. Dinner.

5.0. Light tea.

6.15. No symptoms; took again 30 minims.

7.30. Do. do. 40 minims.

8.30. Slight stiffness as yesterday; took 20 minims.

9.0. Supper.

9.30. Stiffness going off; took 20 minims.

10.15. No symptoms; went to bed; bowels not moved to-day.

Oct. 31. Feeling quite well to-day.

2.30. Bowels moved with difficulty.

5.0. Tea as usual.

6.0. Bowels moved naturally.

6.45. Took 70 minims of *Ignatia* (strong Tincture); no symptoms."

In three days more than five drachms have been taken, at very short intervals and in doses of different sizes; no considerable symptoms have arisen. No doubt predisposition or idiosyncrasy has been one cause of this, but the rapidity of repetition and the changing of the doses, have also been causes. In these respects the experiment is very instructive. To learn the action of *Ignatia* on the rectum was the object aimed at, and if two or three days had been allowed after the first dose, perhaps, more effects might have been experienced. It is not yet known by the Profession that different doses of the same drug fight each other as much as, and sometimes more than, similar doses of different drugs. Of course there was no opportunity of taking the smaller doses as an antidote.

Experiments with Tartar Emetic.

Its local or organic action.
Its contrary actions in a larger and a smaller dose.

Dr. Wynne Thomas.—"Nov. 26, 1891. Last evening I made the following experiment with *Antim. Tart.* the details of which I give you direct from my Note-book:—

"Antimonium Tart. Nov. 25, 1891.

5.30. Had a cup of tea and two small cakes of bread and butter.

7.0. Took 1 grain of Ant. Tart. in 2 drachms of water.

7.30. No effect. Repeated dose.

8.0. Feel a slight inclination to vomit. Repeated dose and vomited immediately some brownish matter (no doubt tea, &c.).

8.15. Feeling quite recovered. Repeated same dose.

8.25. Again feeling an inclination to vomit, I took 1 drop of Antim. Tart. 1 (the first centesimal dilution).

8.30. Repeated dose of ditto.

8.35. Feeling sick. Repeated dose of ditto.

8.40. Do.

8.45. Do.

9.0. Do.

9.30. For twenty minutes or so have felt peristaltic action of bowels going on, but no pain.

10.30. Feel the action of bowels going on; *feeling of sickness has gone off;* very metallic taste in mouth. Repeated dose of 1 centesimal and went to bed.

Nov. 26, 1.0. Soon went to sleep as usual, but woke feeling that bowels would move; however, went to sleep again.

2.30. Woke feeling bowels must act.

2.50. Went down three times, the bowels moved, copious very watery dark-brown motion being the result; no pain or soreness.

7.30. Went back to bed and slept soundly till I was called. Took 1 drop of Antim. Tart. 6; and again on getting up. Tongue coated with brown fur; disagreeable metallic taste.

8.0. Bowels again acted just the same as before.

During the day have felt well, but a feeling as if the bowels would act if I let them. At 6 P.M. a very small formed motion; tongue now almost clean. I certainly think that by taking the 1 centesimal I did stop the vomiting, as all through I only vomited once and never retched at all."

It seems that in this experiment the smaller dose did

antidote the larger, but the antidote was repeated so often as rather to aggravate the diarrhœa.

Second experiment.

"Dec. 7, 1891. Last night I took an ordinary tea at 5 o'clock and nothing after till breakfast to-day.

10.50 P.M. Took *Antim. Tart.* one grain in water.

11.0. Feeling an inclination to vomit I took two drops of Ant. Tart. 6.

11.10. Great increase in secretion of saliva, and feeling as if I would vomit. Took Ant. Tart. 6.

11.20. Much the same but less sickly. Took do.

11.40. More inclined to vomit. Do.

11.55. Vomited some clear watery mucus containing some food, followed by severe retching.

12.15. Feeling better went to bed. Took another dose of Ant. Tart. 6. Soon went to sleep.

12.35. Woke and vomited with painful retching; soon after went to sleep, and slept soundly till 7.30.

Dec. 7th. Tongue brownish yellow; nasty taste; feeling rather squeamish about stomach all day, and taste of Ant. Tart. One motion at 5.0 quite natural. So I am afraid this experiment is not of much value. I have felt the after effects very much more this time than I did before with the previous larger doses."

We learn from this experiment that the sixth dilution is too small a dose to have any effect as an antidote. On this account the experiment is of great value.

Third experiment.

This experiment was delayed by a rather severe attack of the prevailing epidemic, but has come just in time to be inserted here.

"Jan. 25, 1892. Took tea of bread and butter and one slice of cake at 5.0 P.M. Supper of two small sandwiches and sponge cake, and one small cup of tea at 9.0 P.M.

11.0 P.M. Took Antim. Tart. one grain in a little water.

11.30. Retched a little, copious flow of saliva.

11.50. Vomited twice, followed by empty retching. Took afterwards two drops of Antim. Tart. 2 (centl.).

12.10 A.M. Took again two drops of Ant. Tart. 2. Almost immediately vomited again; so repeated dose of Ant. Tart. 2.

12.20. Colicky pains in bowels.

12.30. Colicky pains still more sharp; feel as if bowels will act; repeated dose of Ant. Tart. 2.

12.35. Pains in abdomen so bad that on my way almost fainted, was very giddy, ears felt as if stuffed with cotton wool, managed to fall on to a chair; after two minutes got to water-closet and a large loose motion was passed.

1.0. Feel quite well again, and went to bed.

Jan. 26. Slept soundly till 7.30. No more sickness nor action of the bowels (9 A.M.). Tongue rather furred with a brownish-yellow fur; feel quite well."

Fourth and fifth experiment.

This also was delayed by Mr. Beverley having had the influenza.

Mr. Beverley.—"Jany. 22, 1892. I tried the experiment with Antimon. Tart. yesterday, and I can quite corroborate your views. I also enlisted the services of a friend, who was greatly, and I may say agreeably, surprised with the result. After the experiment we were both left with very bad headaches, the cause of which I cannot tell, whether from the drug itself or from the effect of being sick. We both took the drug in exactly the same way as I took the Ipecacuanha—taking one grain of *Antimon. Tart.* and after vomiting, taking *one* drop of the first centesimal dilution—three doses of which, with ten minutes intervening each dose, stopped my sickness. I was not purged, but my bowels were more relaxed to-day than usual. In my friend's case five doses of the first centesimal dilution were taken before the sickness was stopped, the same time between each dose as in my own case. Afterwards the bowels were uneasy during the day."

The antidotes of Tartar Emetic at present in use are vegetable decoctions or infusions—as the decoction of

Cinchona or the infusion of *Galls*—which are believed to have the power of decomposing the poison. If they do possess this power, of course, it is proper to use them. The experiments now described belong to another method which is worthy of attention. They seem to indicate that the first centesimal dilution of *Antim. Tart.* in doses of one drop, taken *only a few times*, at intervals of ten or fifteen minutes, is the best antidote, at least for some doses of the crude substance.

Experiments with Sanguinaria Canadensis.

Its local or organic action.
Its contrary actions in a larger and a smaller dose.

Dr. Sharp.—In Essay LVIII an account is given of his becoming blind by cataract in 1886, and of the cure of the right eye by *Sanguinaria*. This cure has lasted five years. In October, 1891, he found himself again becoming blind, and Dr. Simpson was good enough to examine the eyes:—the lens in the left eye is more opaque; that in the right eye is again "cloudy"—a new cataract is forming. The *Sanguinaria* 1 was taken once a day, with occasional intervals, for a month; Dr. Simpson said "the opacity has not increased;" but the remedy had to be discontinued on account of a very disagreeable smell, and a great discharge from the nostrils. After this unpleasant state had continued without abatement for a week, one drop of *Sanguinaria* 3 was taken on Dec. 2nd. The next day, Dec. 3rd, the odour was not so bad; on the 4th it was nearly gone; on the 5th it seemed gone; on the 6th there was a faint return of it; on the 7th this continued, and a second dose of *Sanguinaria* 3 was taken; after this the odour was distinctly less; on the 8th a third dose was taken; on the 9th all perceptible smell was gone, and the excessive discharge had ceased. It should be added that, five years ago, exactly the same disagreeable odour was troublesome; but it was not then attributed to the drug. For the eye other remedies may be tried, if life is continued.

Reporting this gives the opportunity of remarking that, in all experiments with different doses of the same drug, the terms "larger" and "smaller" are relative only. When others refer to this subject they speak of

"large" and "small" doses. They are not so spoken of here, because the larger doses may really be very small ones. In the experiment with *Titanium* the large dose was the 50th of a grain, the small one the 10,000th of a grain. In this with *Sanguinaria,* the large dose was the 100th of a drop, and the small dose the millionth of a drop. These are appreciable doses; smaller ones than these, which may be called infinitesimal, are not noticed. The time for their study has not yet arrived.

The novel facts bronght to light by these experiments may teach us many instructive lessons; one of these lessons is so important that it may be alluded to here, namely, the mischief done by the frequent repetition and the long continuance of doses. It cannot be doubted by any well-informed person that many patients have been killed by the continued repetition of the larger doses of drugs; and though the smaller doses are not murderous after this fashion, they certainly do harm when they are repeated frequently, and when they are continued long.

Some of the experiments are examples of the casualties which may be expected to occur in any series of experiments; they are valuable on this account.

III. *Conclusions.*

The conclusions which have now been arrived at in this experimental inquiry may be stated as follows:—

1. There are substances in nature which *in all doses* act injuriously on man when taken in health, and which in a certain group of *small doses* act curatively when taken in illness. They are called Drugs.

2. Each Drug has a local or organic action on some part or parts of the body, while it circulates through the remaining parts without their noticing its presence.

3. Each Drug has different kinds of action in groups of different Doses.

4. There are groups of larger Doses of each Drug having an action in one direction only; and there are groups of smaller Doses having an action in the contrary direction only.

5. There are groups of Doses between the larger and

the smaller ones, which have both kinds of action in succession to each other.

6. The smaller Doses with one kind of action only, may be repeated many times at varying intervals and the action will continue to be the same.

7. These smaller Doses are the true remedies for diseases.

8. The best Antidote for the injurious or poisonous action of the larger Doses is the smaller Dose of the same Drug.

The enquiry itself has been carried on with patience and perseverance; light has dawned upon it by degrees; errors have been detected; truth has been discovered; the evidence from the many experiments recorded is *cumulative*, and it seems to put these conclusions beyond question.

The conclusions, if true, are so many law-facts of nature, and upon them a system of Medicine, that is, a method of treating the sick, may be built, which, by the blessing of God, will increase the measure of health and the length of life of every one who has the full benefit of it. And it will give the benevolent physician such an amount of felicitous satisfaction as, at present, he can form only a very inadequate conception of.

The experiments which have afforded proofs of these law-facts have also proved that the method of treating diseases by medicines to be founded upon them, must be by the use of only one drug at a time, and by the use of it in the smaller doses only.

IV. *Reflections.*

1. These Essays are respectfully, and it may be added with truth, affectionately addressed to Medical Practitioners. They are addressed to their understanding and their conscience. The design of them is, not to put forward opinions or theories, but to solicit attention to *facts*—facts which oblige a great change in the medical treatment of the sick, and, therefore, facts deserving the attention that is asked for them. The change of treatment advocated in these Papers may be accepted and adopted by medical men, not only without any justifiable reproach for unprofessional conduct, but

with increased self-respect and dignity. There is nothing in the change calculated to diminish the consideration due to our Colleges and Institutions, nothing to diminish their hold upon either the profession or the public; on the contrary, the change, accepted in good faith and with courtesy and good humour, would greatly increase the respectability of the profession and the confidence of the public, for the reason that it would be felt by the one and be seen by the other that hypothesis and empiricism had been exchanged for science and true knowledge.

2. A few words are necessary in explanation of the Title of the present Essay. To what extent is a drug known to be an Antidote to itself? It will be remembered that besides the smaller doses which have only one kind of action, or action in one direction only, there are larger doses which have only one kind of action but in the contrary direction; while there are intermediate doses which have both actions in succession, one being contrary to the other. The experiments tried hitherto, with reference to antidoting a poison, belong to the larger doses with one action, the antidote in such cases being the smaller doses with the contrary action; it will be understood, therefore, that no opinion is expressed as to what may be the antidote for the intermediate doses; no experiments having yet been tried with them. What is meant to be stated now is that if the case of poisoning we are called upon to treat is one produced by the larger dose having one action only, the smaller dose with its contrary action will be the best antidote that can be given.

3. Another limitation must also be made clear. Some medical men have entertained the idea that the products of disease may be used as remedies, and they have called such practice *Isopathy*; it may, therefore, be suggested that these diseased products may be antidotes to themselves, and so be considered as included in the teaching of this Essay. That they are not so included it ought to be sufficient to remark that diseases and their products are not *Drugs*. It is well known that the substances included in the term drugs are natural substances in their healthy condition, — they are poisons, but their poisonous action is not the result of disease. This opportunity may be taken also to remark that such

experiments as those of M. Pasteur and those of Dr. Koch are obviously not experiments with drugs, and so cannot be included in our present subject. Whatever may be the final results of these or of similar experiments with diseases and their products, they are altogether outside the limits of the action of *drugs*, and, therefore, have no connection with, nor influence upon, the investigations carried on in these Papers. It need scarcely be added that this study of the action of drugs is independent of the present theories about *germs*.

4. It is to be understood that when a drug is said to be an Antidote for itself, it is meant that the smaller dose is an antidote or remedy for the *effects* produced by the larger dose. So long as the larger dose remains in the body it must continue to act, so that neither the smaller dose of the same drug, nor any other antidote, can possibly counteract it. For example, when solid pieces of white arsenic have been swallowed on bread and butter, and are remaining in the stomach, before the small dose of arsenic can be given with any hope of doing good, the solid arsenic must be removed from the stomach, if possible, by the stomach-pump: unless this can be first accomplished it will be in vain to give any antidote whatever. The doctrine of this Essay does not set aside the use of either mechanical or chemical measures when these are necessary.

5. These Papers have throughout reported a research after *facts* and a rejection of *theories*. It has been said that men cannot avoid theorizing; it is probable that those who have said this have themselves never tried to avoid it. It is certainly possible to avoid theorizing; and not only possible but a duty to do so; for it is to step out of the bye-paths of error, and to take the first step towards the path in which the attainment of true knowledge may be hoped for. This being so, it is the duty of every man, and especially of every medical man, to aim at finding this path to truth, first by exercising restraint upon his imagination, and next by an active and prolonged seeking for facts. From time to time much has been already said about the difference between theories and facts. It has been the lively wish of the writer that his readers should see this difference very clearly. A theory is not a real thing; it has no existence independent of the mind. Lord Bacon compares

theories to spiders' webs, as spun out of the brain of man, but this simile inadequately expresses the difference; a spider's web is a real thing, and though spun out of the substance of the spider, really exists as a material object to be seen and touched; as such it is a fact; a theory is not a fact. Facts, to use a favourite Scotch expression, are objective; they exist quite independently of our minds, and whether we think of them or not. Natural facts are God's work. We may take a dose of *Aconite*, but having done this we have no further power over it—that it goes to the heart and alters its beating is quite independent of our will—it is the will of God that is seen in this, and when we learn this action of the drug upon the heart, we are learning from God's teaching, not from man's. Theories are human teaching and commonly erroneous. Facts are Divine teaching and necessarily true; any error connected with them must be in us and not in them.

6. Pains were taken in Essay XXVIII (1876) to show that Facts are of two kinds—individual and general or inductive, and the value of this distinction has been felt in all that has followed. It is also possible to look at individual facts as of two kinds. It has been said in the preceding paragraph that *natural* facts are Divine; but there are also human facts—the work of man. The dose of *Aconite* is an example of both—the taking it by us is the human fact; its action on the heart is the Divine fact. It is these latter facts that have been untiringly pressed upon the attention of physicians, in the hope that from them they will learn how to treat their patients. This is being taught by an unerring Master.

7. This deserves to be looked at again. Individual facts may be put into two classes—Divine and human. You are taking a walk on some waste ground, you notice a delicate plant about a foot high with pretty flowers half yellow half white. You gather this plant, take it home with you, bruise it, press the sap out of it, and mix this with an equal quantity of alcohol, you let the insoluble parts subside, and pour off the clear liquid. This clear liquid will not ferment or be spoiled for some time, it is the mother tincture of Wild Chamomile (*Matricaria Chamomilla*). Surely, every one will see that the creation and growth of the plant are God's work, and may be called Divine facts, and that all that you have done with it

are human facts. Again, you take two drops of this tincture of Chamomilla and add to it 98 drops of proof spirit, and this is the first centesimal dilution—each drop containing the one hundredth part of a drop of the sap of the plant. You take one drop of this *Cham.* 1 in a little water night and morning for a few days; these are human facts. The doses you have taken are immediately beyond your control, they circulate in the blood, act on the liver, increase its secretion of bile, and give you bilious evacuations; these are Divine facts. You have a patient suffering from a torpid liver; you know what these doses of *Cham.* 1 do in health, and you have no doubt that they will certainly try to do the same in a person out of health, and you prescribe them for your patient; the deficiency of bile is remedied, and your patient is cured; here is a series of human facts followed by a Divine one. If you would be content to learn in this manner from *facts* only, you would learn from Divine teaching. Unhappily, you are not content with this method of learning; you must have these facts *explained;* the explanation is a theory—a human imagination, and almost always erroneous. You are determined to be led in this manner, and so are commonly misled; you prefer to follow a mistaken theory, rather than a true fact.

8. This determination to be guided by explanatory theories leads to another great mistake, namely, borrowing from other sciences, especially from chemistry. Now, the true and grand law-facts of Chemistry are not, and cannot be made to be, the law-facts of Medicine; much less can the erroneous theories of Chemistry be made useful in Medicine. And yet this course is persistently followed; as an example, take the chemical theory of *phlogiston,* from this the antiphlogistic treatment reigned unquestioned by physicians for half a century. This theory, as every one knows, was that when a metal which is inflammable is burned, phlogiston escapes from it, and it becomes an uninflammable calx. Were this true the calx, having lost phlogiston, ought to weigh *less* than the metal did before it was burned. Experiments have shown that the calx weighs *more* than the metal, and so, instead of having lost something it has gained something; this something is now called oxygen, and the calx is called an oxide. Upon this

erroneous theory of combustion the medical treatment of inflammation, for so long a time, was universally founded. And so it has ever been; medical men have learned facts, but they never have been content to be guided by the teaching of the facts they have learned. You will have the facts explained, and the explanation must be your guide; so you are continually enticed away from the bright path of true knowledge, and misled into the cloudy bye-paths of speculation and falsehood. In this way your time and labour are wasted, and, which is worse, your patients' lives are shortened. A new Divine fact will often *explain* an old one, and this explanation is of great value, and may be thankfully accepted, but theoretical explanations are spurious and worthless. If you will be content to seek and apprehend Divine facts, you will have an infallible teacher; you may sometimes misunderstand your teacher, but the error is then your own, not your teacher's; on the contrary, if you will follow the beaten track, you follow a guide so fallible that it is almost always wrong.

9. It has been said above that a new fact will often explain an old one, that is, a newly discovered fact will prove to be the cause of another fact which has been long known, but its cause only theoretically explained: *e. g.* it is known that small doses of drugs cure some diseases, and it is now also known that these doses cure because their natural action in health, and, therefore, in disease, is contrary to the particular morbid action going on in the case in hand. The second fact—the action of the dose in health—is an explanation of the first fact—the action of the dose in disease. But you will notice that this explanation is partial only; it shows that the second fact is the cause and the first fact is the effect, but it does not show how the effect is brought about; this part of the mystery of the action remains as impenetrable as before. Every natural phenomenon is a mystery—the action of drugs is a great mystery. And what is a *mystery?* It is made up of two parts—one is the *fact* which may be perceived and known, the other is the *modus operandi*, the manner in which the fact came into existence, which is hidden from us, and cannot be known by our finite intelligence in its present condition. It is childish ambition to seek to know what is beyond our mental capacity.

10. Any one who has been a student of Medicine for some years will have been surprised and pained by many things that he could not fail to observe, but few things will have grieved him more than this—the difficulty which exists almost universally in the medical mind in learning to distinguish between a fact and a theory. It seems impossible for the average physician, and even for some of the superior sort, to understand and appreciate the difference between a fact in nature, which exists whether we see it or not, and a theory, which is a mere notion or thought in the human mind. The love of speculation and theory seems to injure the reasoning faculties so as to render them incapable of successfully investigating facts.

11. A few words on human authority:—*Authority* has its own legitimate sphere, and it has also its limits. Within these limits it is entitled to the greatest respect and obedience; the discovery of *truth* is outside these limits, and is free from its restraints. It would be a useful task if, in this literary age, some competent writer would take for the subject of a volume the legitimate sphere of Authority. He would probably divide his treatise into two parts—the first containing facts showing what are the true limits of this sphere, within which Authority can claim, and is entitled to, our highest respect and our submissive obedience;—the second setting forth the facts proving that a great many false claims have, from time immemorial, been maintained, whereby much injury has been done to mankind; among these illegitimate claims those made in regard to *truth*, the search for it, and its influence when found, have been deplorably hurtful. In these Essays absolute authority is claimed for Facts, none at all for the reporter of them beyond what is due to every honest man. For it is believed to be true that "in the whole history of human thought it never has happened, and it never will happen, that a question once raised has been settled or silenced *by authority*. The only way in which questions can be set at rest, and discussions of them made to cease, is by finding the right answer and *proving* that it *is* right." (Alfred Plummer, D.D.)

12. The "question raised" in this investigation is this:—What is the action of Drugs? The answers, so

far as experiments have already given them, are enumerated in the paragraph of this Essay headed "Conclusions." These answers may be ridiculed, or they may be rejected; they may be denied, or they may be ignored; but the question and these answers to it cannot be silenced; they will remain, and, from time to time, they will present themselves and demand the attention of the Medical Profession, until they are tested by fair experiments, and the truth concerning them is perceived and acknowledged.

13. The most novel of the answers given to the question—What is the action of Drugs? is that every drug possesses a group of larger, and a group of smaller doses, which have contrary actions. To this property the name of *Antipraxy* has been given. This is a new and surprising law-fact, and it may reasonably be asked to produce its credentials. Some of these were first shown in Essay XXII (1873), and others have been put forward in the Essays which have followed. In the present one Antipraxy has been submitted to the severest possible tests. Has it cleared itself of all doubt? The following is a three-fold affirmative reply:—(1). Its first appearance was in Dr. Sharp's experiment on himself in 1856–7, with *Titanium*, in the long and serious illness caused by the larger dose, and the astonishingly rapid cure by the smaller dose. Of this novel fact it may fairly be asked:—Can it be reproduced in other drugs? This question is now affirmatively answered by the experiments here recorded with Aconite, Chamomilla, Ipecacuanha, Mercury, Belladonna, Physostigma, Ignatia, Tartar Emetic, and Sanguinaria. (2). As a new fact, founded on experiments with some drugs only, and not with every drug, is of the nature of a prediction, it may fairly be asked:—Has the prediction been fulfilled? Assuredly, it has been abundantly fulfilled by many additional experiments, and the question is answered in a convincing manner affirmatively. (3). Antipraxy is a definite statement of law. It may reasonably be asked:—Has it been verified? Yes, very largely, is the reply. The experiments in this Essay, in which it is seen that the contrary action of the smaller dose is so distinct and energetic that it can antidote the powerful action of the larger dose of the same drug, are the strictest test to which the definite statement of

Antipraxy can be subjected, with a view to its verification. They have responded successfully, and without failure. How contrary are these results of experiment to *à priori* reasoning! If when you had been made ill by a dose of any drug, it were proposed to you to take some more of it even in a smaller quantity, would not this proposal make you exclaim—"of course, it will make me worse!"

14. From the numerous experiments which have now been reported in these Papers it is certain that drugs have two contrary kinds of action in different doses. It is, therefore, obvious that they may be employed as medicines for two opposite conditions of disease; the larger doses may be given for one condition, the smaller doses for the opposite condition. It is well known that, for more than two thousand years, drugs have been given as medicines in their larger doses, and the result is acknowledged to be a failure—the most intelligent physicians have scarcely any faith left in them of the efficacy of medicines; so that, in the words of Sir John Forbes, in his legacy to the Profession after fifty years of hard work in it, "Medicine must either mend or end!"

15. It is, therefore, certain that the use of these larger doses of drugs ought to be abandoned; and experiments with smaller doses in health have shown that it is highly desirable to learn what they can do as medicines, when they are given for conditions of disease contrary to those for which the larger doses have, hitherto, been given. What is meant by this one example ought to make quite clear:—*Opium* in its larger doses stupifies the brain, and constipates the bowels; these larger doses, as every one knows, have been continually prescribed for wakefulness and for diarrhœa. The new method would be to give the smaller doses for congestive apoplexy, and for torpid constipation. Homœopathists will say that they have been giving small doses for nearly a century. Yes, but there is a wide difference between the two methods. Hahnemann's principle or law of similars is not a guide to the dose; the small dose was arrived at by him in a roundabout and accidental manner; at first he gave the common medicinal doses, but so frequently found his patients made worse by them instead of being cured, that he made the doses

less and succeeded better; he went on making them less and less until he lost his head and fixed upon the thirtieth dilution as the best dose of every drug, and for every illness, acute or chronic; he stopped at this decillionth of a grain only because, as he said, we must stop somewhere, but his followers have not stopped here but have gone wildly further, as they declare that there is no law for the dose, it is "the outcome of experience" only. *Antipraxy*, as you have seen, *is a law for the dose.*

16. Now, if *Opium*, in its smaller doses can cure, as it has cured, cases of apoplexy arising from common causes, it is possible that it may be able to cure apoplexy caused by the larger doses of opium itself. The experiments given in this Essay show that it is not only possible but probable, and justify its being prescribed as an antidote in such a case. And the same may be said of any other drug. Let it be remembered that the smaller doses, given as remedies, must be small enough to have actions in one direction only; and also that these may be repeated as long as they do good, but must be stopped before a morbid condition is produced which is the contrary of that for which the doses have been prescribed as a remedy. For example:—*Aconite*, in a small fraction of a drop of the tincture, may be given for a quick pulse accompanying inflammation; the dose may be repeated at longer or shorter intervals according to the severity of the case, until the beats of the heart are reduced to the healthy standard, but must not be repeated after this, or the heart's action will become slower than is healthy. There is no mysterious *Pathy* in this, it is all plain English. God, in His mercy, will open men's eyes to see it, and to read it aright. Experiments like these, cautiously made, are worth more than volumes of argument.

17. The confusion about *Doses* alluded to above is one of the many and grave defects of Hahnemann's system. Some of these have been pointed out in the course of this examination of Homœopathy, and one more must be noticed here, which does not reflect discredit upon him, for it was unavoidable in his time. This defect is the non-application of modern chemical analysis and of microscopical observation to the various secretions, particularly to that of the kidneys. In the provings of each drug, as they were first given us, and as we still

have them, there is ample notice of excess and deficiency in quantity, of unhealthy colour, of sediments, &c., but there is nothing to indicate the presence of albumen, or sugar, or the different saline substances, now known frequently to exist in the urine of the sick. This defect ought to be removed by the homœopathists of this generation; it is honest and useful work for many skilful and industrious hands, and it is here commended to their attention. An interesting study of Nitrate of Uranium is not forgotten, but these remarks are meant to apply to the drugs best known, and which are in daily use. This deficiency is another proof of the necessity for fresh experiments with drugs in health.

18. This reference to chemical analysis must not be misunderstood. It has been the ambition of successive generations of talented physicians to bind Medicine to the chariot of Science running its course in their time; so we have had *iatro-mechanics* and *iatro-chemists*. At the present day a very strenuous endeavour is being again made to insist upon Pharmacology being a branch of Chemistry, and even Physiology is thought to rest upon Chemistry and Physics, and to be dependent upon their improvement for its progress. The existence of *Life*, and its workings in living bodies, seems to be forgotten. All such efforts in times past have failed; and the present will also fail. Mechanics, Chemistry, Physics, and other branches of Natural Science, as Electricity, Optics, Acoustics, can help us in sundry outlying departments of Medicine, but not one of them can be its foundation.

19. The improvement of therapeutics has been sought in these Essays by a different method. In another respect the work has differed from that of the distinguished labourers alluded to. When they speak of drugs they speak of their "mode of action." This is not discussed here because it is believed to be beyond our reach; the *modus operandi* of drugs in the living body is what we cannot know. We have been in search of *what* a drug does, not *how* it does it.

20. Dr. John Hodgson Ramsbotham, of Leeds, forty years ago (in 1850), laid it on the writer's conscience to investigate Hahnemann's Homœopathy. The first step was a diligent experimental search after the Principle or 'Law of Similars.' This seemed to be a question inde-

pendent of the dose, and was so taught by Hahnemann. In pursuing this course a mistake was fallen into, since discovered and explained in former Essays. The Law of Similars of Homœopathy is now exchanged for the Law of Contraries of Antipraxy. It will be noticed that this mistake was originally fallen into by Hahnemann himself, and that it has been perpetuated by his followers ever since. Hahnemann was led to his conclusion by the legitimate method of experiments with drugs on healthy persons; he studied one thing at a time, and arrived at '*similia similibus.*' The next step was the study of the comparative actions of different doses of the same drug by a like experimental method. When the time came for this step to be taken it was found that though Hahnemann's Principle refuses to say anything about the Dose, the Dose has a great deal to say about the Principle; for it distinctly declares that 'like curing like' is not true, and may safely be abandoned. Had Hahnemann taken this second step he might have discovered the mistake.

21. This abandonment of the 'Law of Similars,' necessitated by experiments with doses, is necessitated also by the experiments reported in this Essay, in connection with Toxicology. In the early Essays it was often pointed out that the principle of 'Similars' could not include 'the Same'—a drug was not to be expected to cure the symptoms caused by itself; they must be similar symptoms arising from some other cause—*simile* is not *idem*. The experiments now made show that a drug in its smaller doses is an Antidote to the mischief it has done in its larger doses. This is *idem* not *simile*. On this ground also Hahnemann's Principle is condemned.

22. Notwithstanding the clearness of all this, there are some who will say that there is still left a small shred of Homœopathy in this, that a drug has a group of larger doses which, when taken in health, cause symptoms 'similar' to those that the group of smaller doses can cure. The reply is this:—The best remedy of any drug lies in the action of a group of its smaller doses. How can we learn what this curative action is? In three ways: first, by experiments with them on healthy persons, this is the best, the most reliable way; second, by experiments with them on the sick, this has

a serious drawback in the complication of the illness, but it helps; then, we know now that there is a group of larger doses of the same drug, whose action in health is the contrary of that of the smaller doses, this, therefore, is a third way of increasing the knowledge we are in search of, namely, the action of the smaller doses. In this way the action of the larger doses may help us, but this help does not lie in the *sameness* of the action to that of a disease, but in its *contrariety* to that of the smaller doses. If Homœopathy had not been thought of before, it would not have been thought of now. Those who can derive comfort from this last refuge of Homœopathy may be allowed to enjoy it in peace; others will see that the connection of the group of larger doses of a drug is, not with any disease, but with a group of smaller doses of the drug to which itself belongs, and with this connection they will be satisfied. The direct connection, which it is the duty of the physician to become acquainted with, lies between the organs in which the morbid action is going on, and those in which the action of the smaller doses takes place, in other words, the seat of the action of the disease and that of the doses must be the same, and the kind of action of the two must be in opposite directions.

23. In this investigation of the action of Drugs, answers to five questions have been experimentally obtained:—First, Is there any action common to all Drugs, and at the same time sufficient to distinguish them one from another? This question has been answered by the fact that, both in health and in disease, Drugs have seats of action in this or that part of the body, and that this local, or organic, or pathogenetic, or curative action is characteristic of each Drug, and distinguishes it from the rest. The second question has reference to the different Doses of each Drug:—Is the action of these Doses alike in kind and only different in degree, or is it different in kind as well as in degree? The answer found by experiments in health has been that the various Doses of each Drug differ in the kind of action as well as in the degree of it, and they may be arranged in groups; *e. g.* there are groups of larger Doses having only one action in one direction; and there are groups of smaller Doses having

only one action, but in the contrary direction; and there are groups of intermediate Doses having both these actions in succession. The third question is this:—Is the action of these groups of Doses in the same organs, and of the same kind in disease that it is in health? The answer has been that the seat of action is the same and the kind and direction of it are the same, or tend to be the same in disease as in health. The fourth question refers to the repetition of the same Dose:—What happens then? Experiments in health have revealed the fact that when small Doses, having one action, are repeated at short intervals, it is the same action that is repeated, so that effects, which at first were slight, may be so increased by repetition as to become fatal. The fifth question is this:—What will happen when Doses that are *not the same* are repeated? The answer, so far as it has now been found, is that when a larger Dose has been taken first, and a smaller Dose is taken afterwards, the smaller Dose acts as an Antidote to the larger.

24. Adaptation of means to an end is acknowledged to be a proof of intelligence and power. In God's works in Nature this adaptation is perfect; look where you will you can find no exception to it. Medical men cannot be too wide awake to this fact, or meditate upon it too much. Such observation and thought will convince them that, although "God has given us medicines to heal our sicknesses," these means have never yet, *in our hands*, been even tolerably well adapted to that end. This conviction should excite in us a longing desire to see some nearer approach to a right adaptation of drugs in the use of them as medicines, and this should lead us to a diligent re-examination of Therapeutics, with the help of any new light which may have been thrown upon it.

25. The experiments recorded in these Papers seem to prove that up to the present time Drugs have been given for purposes the very opposite of those for which they are best adapted. They have been credited with properties which, either they do not possess, or which belong only to certain large doses of them, while other smaller doses of the same drug possess precisely contrary properties. We learn from a careful use of these smaller doses as medicines that they are better suited to

our purpose, that is, that they are better remedies than are the larger doses. *Opium* in small doses is a better remedy for coma and constipation, than it is in large doses for wakefulness and diarrhœa.

26. It is certain that Homœopathy has been an improvement upon all previous methods of healing the sick by medicines; but it is unwise to suppose that it is the final improvement. Our knowledge, always small and imperfect, ought to be progressive, and one step in advance ought to be followed by another. It is necessary, therefore, to look on the defects of Homœopathy, as well as on its merits. Among these defects of the "law of similars" as explained by Samuel Hahnemann is *its vagueness*. Several of the early Essays were occupied with this subject. Hahnemann contended that one disease cured another by this law; that mental emotions acted homœopathically upon each other; that the imponderable agents—light, heat, electricity, and magnetism—were all governed by this law of similars. At the time referred to (1850–6) his disciples were amplifying indefinitely the application of this law "into all the regions of morals, politics, and education."* The early struggle was to show that all these applications of the law must be swept away, for that it belonged only to *Drugs*. But further, all the laws of nature are definite and limited, and "similarity" is not definite nor limited, it has no boundary line, you cannot tell where it begins nor where it ends, hence you are constantly liable to be led into errors by it. Again, Homœopathy—the law of similars—as is abundantly proved in the early Essays, "says nothing about the *dose*;" and this fact has been constantly maintained by leading practitioners of Homœopathy to the present time. It is believed that *Antipraxy*—the contrary action of groups of larger and smaller doses of the same drug—is an improvement of our knowledge, and specially in these two particulars in which Homœopathy is defective. Antipraxy *is not vague*, it is not unlimited, but on the contrary, has the definite boundaries of a law of nature; and it tells us something which it is very necessary for us to know about the Dose. As a principle it guides us to the choice of *both the drug and the dose;* the experiments in the later

* See Preface to 'An Investigation of Homœopathy,' p. viii, 1856 (7th Edition of these Essays).

Essays are a proof of this. We learn what is the action of the smaller doses by experiments with them in health; we learn that this action is also the same when taken by the sick, *even when their ailments have been caused by the larger doses of the same drug*. What can be more confirmatory evidence than this? Homœopathists think that Antipraxy leaves them practically where they were before; they will find themselves mistaken. The old school tries to take no notice of it, and in appearance to pass by on the other side; like the dying Cardinal it makes no sign—*it also will die.*

27. The question has been asked, "What about the Heads of the medical pofession?" They occupy a difficult position. It is to be feared that they are wanting in moral courage, and are too much like butterflies that flutter only in the sunshine. They have treated Hahnemann as their fathers treated Paracelsus. Hahnemann resembled Paracelsus in some respects, but greatly differed from him in others; his work, notwithstanding his transcendental theories and his childish dogmatism, deserved the patient investigation which has been devoted to it in these Tracts, and the discovery of truth which in this way has been made, is a gratifying recompense. The nominal leaders in the republic of Medicine have lost a grand opportunity, and thereby have forfeited the respect of future generations.

28. Homœopathists now say that they do not apply the "Law of Similars" to the action of the drug when given as a remedy in disease; they understand it only as a rule for the selection of the drug. This is a real departure from Hahnemann, and it gives up half the battle. The remaining half cannot be contended for long. Experiments seem to show distinctly that the various doses of each drug can be arranged in groups, each group having a characteristic action of its own. There are groups of larger doses having an action in one direction only; there are groups of smaller doses having an action in the contrary direction only; and there are groups of doses between these larger and smaller doses having both these actions in succession. In practice the smaller doses are given as medicines, that they may act as contraries to the disease; the only "similars" to the disease, therefore, must be looked for in the group

of larger doses having an action contrary to that of the smaller doses given as remedies. Where are the provings of this group of larger doses to be found? No where as yet. Then it follows that two series of experiments in health have to be undertaken—one of large doses of each drug in order to discover the group which has only one action in one direction; the other of small doses to discover the group which has only one action and in the contrary direction. These two series of experiments would be elaborate beyond measure, and not unattended with danger, and there is no probability that they will ever be accomplished. On the other hand, the demand made by Antipraxy is comparatively simple and easy. It asks for careful experiments with small doses only, and the group which has only action in one direction is at once known as the best remedy for any morbid process which is in the contrary direction. Such a series of experiments is reasonably practicable, and, surely, there is some probability that it may be accomplished. This does not forbid advantage being sometimes taken of the knowledge we have already, or may hereafter obtain, of the actions of the larger doses of the same drugs. The original experiments made by Hahnemann and his friends have often been urgently asked for, but they have never been given; if they had been published we should have known the doses the experimenters took.

29. Some of the experiments in this Paper are severe ones, but they belong rather to Toxicology than to Therapeutics, and it will not be necessary to continue them with many more drugs. They are sufficient to justify any medical man in following their teaching in his practice. For example, if he should be called to a case of congestion of the brain caused by an overdose of *Opium*, he may safely give, as an Antidote, a dose of one drop of the first centesimal dilution of the Tincture of Opium in a teaspoonful of water, and repeat this dose once or twice at an interval of fifteen minutes. If this treatment is followed by some relief, he may wait and expect further improvement; if it is not followed by relief, he may resort to other measures.

30. Hippocrates tells us that he should esteem him to be the best physician, who makes the fewest mistakes. What an admirable acknowledgment this is of his con-

sciousness that some mistakes in the life and work of every physician, himself included, are inevitable, and what humility there is in this acknowledgment! The writer of these pages is glad to think that he has been conscious of having made mistakes, and that he has been willing to own it. That he has, notwithstanding, brought some precious truth to light, he cannot doubt; and the great Husbandman, who watches over the sowing of the seed, and who brings about the harvest in its time, will then winnow the chaff from the wheat. To this future the labourer joyfully commits his toil.

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V. We read in Addison's charming 'Spectator':— "If in the present life our happiness arises from the subduing of our desires, it will arise in the next from the gratification of them." (No. 574.) These Essays are the happy reward of work done in the study and practice of Medicine; work begun with the utmost disinclination, but carried on cheerfully with contentment and thankfulness for seventy years; and they are now left as a legacy to the younger generations of the Medical Profession, with many prayers to God, through Jesus Christ, for his blessing.

Horton House, Rugby;
January 21, 1892. (*My 87th Birthday.*)