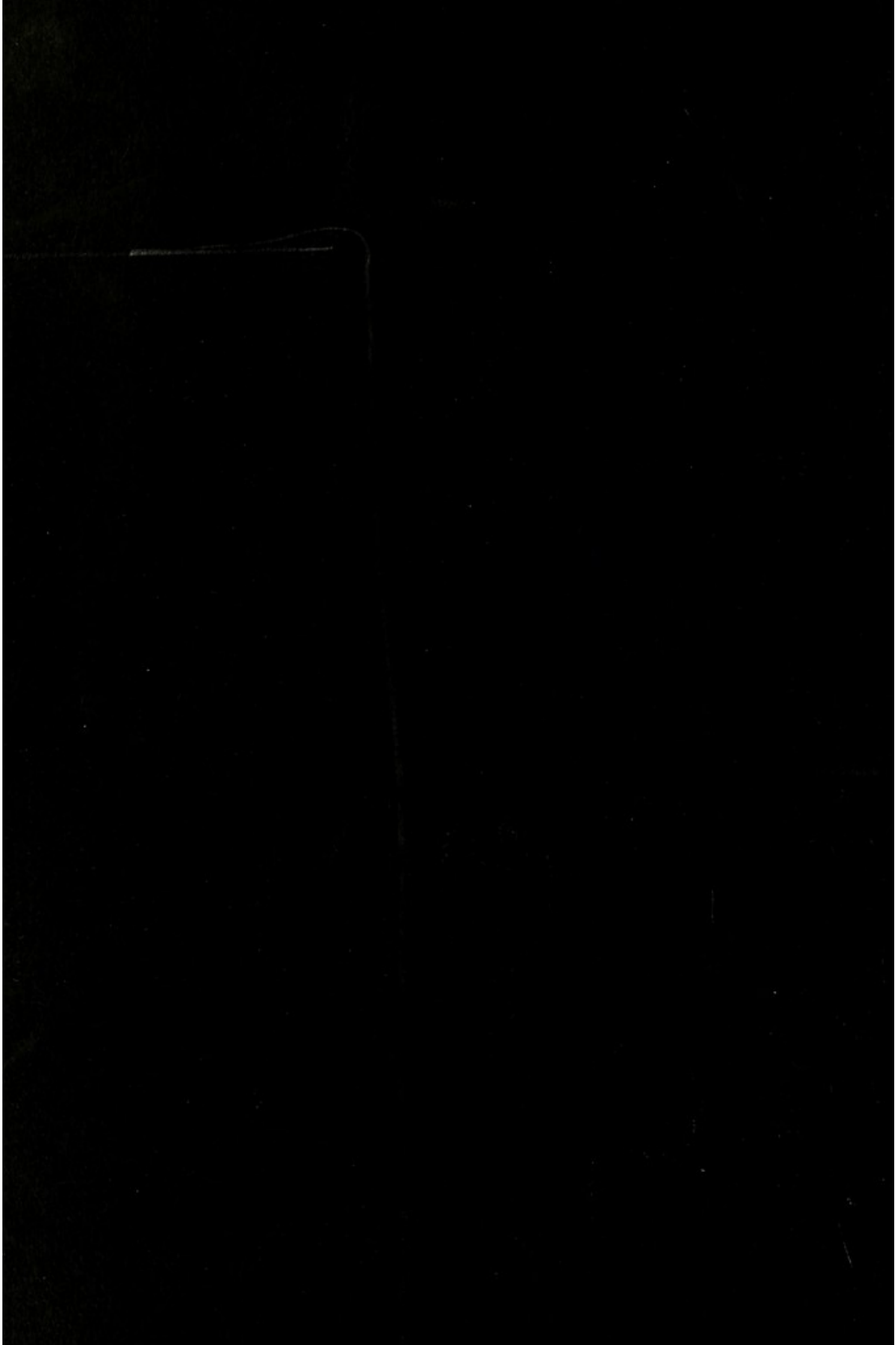
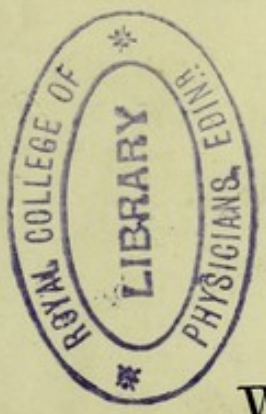
The repetition of the same dose / by William Sharp.



ESSAY LIX.

THE

REPETITION OF THE SAME DOSE.



BY

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

"It is required of stewards that a man be found faithful."
ST. PAUL.

LONDON:
GEORGE BELL AND SONS,
YORK STREET, COVENT GARDEN.
1891.

ANALYSIS.

Introduction.

All phenomena are governed by laws.

Repetition of the same Dose.

The next step in advance. Is at present in obscurity,

Experiments with small doses of—Aconite.
Digitalis.
Castor oil.
Rhubarb.
Veratrum.
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Chamomilla.
Belladonna.

Conclusions.

1. Local or organic action of Drugs.
2. Contrary action of groups of larger and smaller Doses. Action of these doses in one direction only.
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ESSAY LIX.

THE REPETITION OF THE SAME DOSE.

"It is only by making fresh advances that we can secure what is already gained." W. H. BENNETT.

Introduction.

It has been said, and the words express a brilliant truth, that "Ideals are the very soul of life." The saying, like most sententious utterances, needs to be qualified—the Ideal must be *true*. There are false Ideals, which are only misleading; such as in Mechanics, to discover the perpetual motion; in Chemistry, the elixir of life. These are *ignes fatui*, which have misled many. A true Ideal must be a *fact* and not a *theory*; such an Ideal is ennobling; it inspires a hopeful and persevering industry; it is a mighty encouragement in the performance of duty. Those whose minds are enlivened by such a bright prepossession may fairly be asked, and will not object to be asked, to say what their Ideal is. If I am asked what is mine, the answer is ready—it is the belief that all phenomena, from the moment of their coming into existence, are subjected to the government of laws, by the will of their Creator. This is the Ideal of my professional life, and as a consequence I have greatly admired those who have been privileged to discover some of these laws, and I have endeavoured to tread in their steps. It was the Ideal of Galileo, of Kepler, of Newton. It is an illuminated path. It must not escape notice that a man's Ideal is not his object, design, or purpose; it is that which stimulates and leads him on in the pursuit of his object, in the accomplishment of his design, in the fulfilment of his purpose.

2. The discovery of a law involves the solution of two problems. The first problem is how to obtain a distinct

knowledge of the individual phenomena or facts under consideration; the second is how to put this knowledge of individuals together so as to find the law that governs them. The second cannot be taken in hand until the first has received a clear solution. The subject we are engaged upon is the action of Drugs as medicines, and the first problem is how to obtain a clear knowledge of the action of each individual drug. Perhaps, if I may be allowed to make use of analogy, this may be clearly seen. Mathematicians tell us that to solve a problem consisting of known and constant elements or data, and of only one unknown quantity to be found, is often sufficient to exercise the highest faculties of the human intellect. What would be such a problem to us? To find out the action of individual drugs the data, I think, ought to consist of one healthy person, one drug, and one dose. These should be the known and constant elements, and the unknown quantity to be discovered would then be—what happens when they are put together? The finding of this would be the solution of the problem. Whereas, the problem as dealt with by orthodox physicians for more than two thousand years has differed from this exceedingly in both its parts. The elements if known have not been constant, for the body has not been one in health but has been varied by diseases; the drug has not been one but mixed with many others; and the dose has varied in every possible manner. Then, as regards the unknown quantity to be found, instead of there being only one they have been countless in number; the drug has not been taken by the healthy but has been given to the sick; hence the unknown quantity of disease is added—this alone is a complication greatly hindering, if not absolutely forbidding a solution of the problem; again, the drug given at one time has not been one but many, sometimes as many as fifty or more, each drug adding another unknown quantity; and, again, in single experiments the dose has been varied, each variation adding another unknown quantity. The consequence of all these complications has been that a solution of the problem—What is the action of each drug?—has been impossible; and it must continue to be impossible so long as the present custom of prescribing medicines is continued; so that the forecast is certain that, at the end of another two

thousand years, the solution will be no nearer than it is at present. Under these conditions of the first problem it would be in vain to take up the second. This second, the putting together the results of the first, has been considered in Essay LII.

3. It may be hoped that it is now clear that the only method by which to discover the true action of drugs on the human body is the problem with one unknown quantity, whose data are one healthy body, one known drug, and one known dose. Experiments carefully made with these limitations are certain to yield good results, by which the question—What is the action of drugs on man?—may be satisfactorily answered. During the eighteenth century a few individuals essayed some experiments of this kind; notably, Stöerck of Vienna, Alexander of Edinburgh, and Crumpe in Ireland (see Essays XVI, XXVI, and XXXII). After them Samuel Hahnemann in Germany devoted the greater part of his life to this work in a most laborious and praiseworthy manner, and succeeded as the founder of a Sect, not as the teacher of his Profession. His success has been conspicuous, his failure is not less so. Among the reasons for this failure may be mentioned the following :—

(1.) The problem he undertook to solve, instead of having three, had only two elements—a healthy person and a single drug, and of these only the drug was known and constant. The results of experiments by several persons are huddled together in an undistinguishable mass. And the element of dose, which ought to have been both known and constant, was neither; it was not only allowed to vary to any extent, but it can never be known, for it is not given—it is not reckoned as part of the problem. All this want of precision makes his experiments essentially imperfect.

(2.) He rejected Pathology, and recorded nothing but symptoms. The medical profession, with its present knowledge of Pathology, cannot consent to its exclusion. The condition of knowledge on this subject in Hahnemann's time may be a sufficient apology for his omission of it, but that omission necessitates a repetition of all his experiments.

(3.) The form in which he has presented the fruits of his labour is most damaging and disappointing. His "Provings," that is, his experiments with drugs in

health, are contained in his 'Materia Medica Pura' and his 'Chronic Diseases,' and fill several volumes. The contrary symptoms found in juxtaposition in almost every drug, the addition of symptoms from cases of fatal poisoning without this being indicated, the innumerable insignificant transitory sensations, and other reasons, make the study of these volumes, even to those who are most anxious to be instructed by them, excessively perplexing and repellent.

(4.) The experiments of all who proved the same drug being mixed together, the different sensitiveness of individuals is obliterated. There is no record of absence of effects, which must have occurred to some experimenters. There is no help in the great difficulty caused by predisposition or Idiosyncrasy.

(5.) The effects of each drug are topically distributed over all parts of the body, so that the characteristic action of the drug on special organs cannot be discovered. It is impossible to learn from them Organopathy.

(6.) No doses are given, and as the action of a drug depends quite as much upon the dose as upon the drug, the symptoms given are to this extent worthless. There is no indication of Antipraxy.

(7.) No information is given as to the repetition of doses, so that what may have been caused by repetition cannot be learned.

The provings of Hahnemann, as he has presented them to us, can never become permanent documents. They must be done over again; the varying element of dose must be made constant and be found in the problem; Pathology must take its place in the observations; each proving must be arranged and reported as a separate case, so that a clear notion of the disorder or disease caused by the drug may be had, and so that the Idiosyncrasies of the experimenters may be noticed. All these improvements are necessary if the action of drugs in health is to be, as I am sure it ought to be, our chief guide in our prescriptions of drugs as medicines. Hahnemann's Provings will remain only as historical curiosities, and the solution of the two problems as attempted, however imperfectly, in these Essays, has to be proceeded with.

Even homœopathists, if I am rightly informed, are now neglecting these Provings, and are returning to the old

method of experimenting on the sick. This is the plain English of what is called learning *ab usu in morbis*. The difference between the two schools is that the one tries experiments with large doses, the other with small ones. The new has the advantage that the good done by small doses is incomparably greater than by the large ones, and is accompanied by much less harm; but to call this homœopathy is absurd. There is no *similarity* in it; it is as much *contrariety* as the old practice; it is Antipraxy not Homœopathy.

4. It must be evident now why orthodox physicians, during so many centuries, have succeeded so little in curing diseases by medicines. They have so muddled the data of the problem, and have added so many unknown quantities to it, that its solution has been rendered impossible. And it must also be evident why homœopathists have not succeeded even better than they have. One known element, which ought to have been constant, has been allowed to be variable; and another element which ought to have been recognised and constant, was neither recognised nor constant; so that the attempt to solve the problem has only partially succeeded. It may be asked, How, then, did Hahnemann arrive at the small doses, which has given Homœopathy the success it has had? Homœopathists reply:—"Hahnemann for many years employed the ordinary doses of drugs, which he gave as specifics in accordance with his therapeutic rule. Then, to avoid physiological action [*i. e.* aggravation of the disease that necessarily followed the giving of these large doses], he gradually reduced the dose." (Dr. Black, Address at York, 1872). "The dose is the outcome of experience." (Dr. Dudgeon, see Essay, LII). Hahnemann discovered his rule for the choice of the drug as a remedy by experiments on the healthy, but he left the dose in the condition it was in before him, to be settled by the old familiar mode of experiments on the sick, and there it remains with homœopathists, as with the orthodox, to this day.

5. The experiments I have the privilege of recording in this Essay will be found to be examples of the problem as represented in the preceding paragraphs. The elements or data are one person, one drug, and one dose; and the unknown quantity sought is the action of the same dose when it is repeated. The answer is unequi-

vocally given. Let me not, however, be misunderstood; it is not pretended that this is a mathematical problem —it is not a question in mathematics but a question in science; the language of mathematics is borrowed only in the hope of stating the matter with precision.

Repetition of the same Dose.

6. It has been shown that the rule for the choice of a *remedy* in each case for which a physician is called upon to prescribe, is an inference or deduction from the law-fact expressing the local or organic action of each drug—the seat of action of the drug must be *the same* as that of the disease. It has also been shown that the rule for the choice of the *dose* of the remedy prescribed is an inference or deduction from the law-fact expressing the contrary action of a group of larger and a group of smaller doses of the same drug—these groups of doses having one action or action in one direction only—the action of the drug must be in *the contrary* direction to that going on in the disease. And practical experience, that is, experiments on the sick—for all experience in practice is got by experiments on the sick—proves that the contrary actions of the smaller doses are more successful, and especially are freer from damaging accompaniments than are the contrary actions of the larger doses. For example, it is better to give the smaller doses of *Opium* for constipation than the larger ones for diarrhœa.

7. These two important rules in Therapeutics being now settled, the time has arrived when another step in advance may be taken, or when another ladder may be sought, by which we may again climb up to a higher platform. What, then, is the next perplexity in which the physician finds himself involved? It is the *repetition* of his doses, and when they ought to be *discontinued*. These are questions which, up to the present time, have received no satisfactory answers. It is plain, therefore, that our next duty is to grapple with these difficulties.

8. The subject of the repetition of doses is a wide one, it embraces three distinct questions—First, What happens when a dose is several times repeated? Second, What intervals of time are there to be between each

repetition? Third, When are the repetitions to cease? Of these, the first will engage our chief attention in this Essay—What happens when a dose is several times repeated? This also is a large question and must be subdivided, for it includes *all* the doses of a drug, and we have seen that these are arranged in groups, each group having a characteristic action of its own—there are groups of larger doses having one action in one direction, and there are groups of smaller doses having also only one action but in the contrary direction, and there are intermediate groups of doses having both these actions in succession. Our present study is confined to the group of smaller doses having only one action in one direction.

9. This, at first sight, will seem a very small part of the whole subject of repetition of doses, but it is the pith of it; it is the practical part of it; and, therefore, the most important part. These small doses are the very doses that physicians are to prescribe, because, of all the doses of a drug, they are the fittest and most successful remedies. To know, at the time he writes his prescription, what will happen if he orders the dose to be several times repeated, is exactly the piece of knowledge he ought to possess. The limitation, therefore, of the experiments now given to this precise point is the very best way of obtaining a practical summary of a large subject.

10. Like the two first rules—that for the choice of the drug, and that for the choice of the dose—a rule for our guidance in the repetition of a dose, must be an inference or deduction from a law-fact governing the action of the dose when it is repeated. How is this law-fact to be discovered? The first law, governing the action of drugs (*Organopathy*), was discovered by experiments made with drugs on the *healthy*. The second law, governing the action of doses (*Antipraxy*), was discovered by experiments made with doses on the *healthy*. The third law, governing the action of repeated doses, must be found in the same manner, by experiments on the *healthy*.

11. The action of a *repetition* of doses may be one of three kinds—(1) It may be in the same direction as that of the *first* dose, and so increase its effect. Or (2), it may be in the contrary direction, and so produce a con-

trary effect. Or (3), it may simply neutralise the first action, and so seem to produce no effect at all. Some groups of doses may act in one of these ways, and others may act in one of the other ways; and the Idiosyncrasy of the experimenter will have some influence with each of them. All these phenomena will be governed by some law-facts, and experiments ought to be made to discover what they are. The statements just now made are merely *à priori* conjectures; only experiments can teach us the facts.

12. Here I am arrested by a question—"Why should you disturb the established order of things?" Because I think it unsatisfactory, and both requiring and admitting of improvement. "But if so, why is Orthodoxy maintained?" Orthodoxy is maintained by some from a sincere belief in it; and by others from a submissive acquiescence in it from motives of policy; others remain in it from indolence, they have learned it, and will not take the trouble to learn any more; and others from thoughtless indifference; while the many, having received it on "Authority," abide in it with a serene and unthinking satisfaction. None of these considerations are reasons why its character should not be examined, and if *that* is found wanting, why it should not be exchanged for a better.

13. Again, another question—"What is there particularly wrong in the customary repetition of the doses of a medicine?" Because its main foundation is a paltry one. I lately asked a medical man what reason he has for the often repeated direction in prescriptions —"Two tablespoonsful of this mixture to be taken three times a day"? He replied, "It is simply a matter of convenience; it gives the patients something to do between their meals, and they like it; it does not matter whether the medicine is a powerful poison or a mere *placebo*." For the most part these doses are nauseous; they often destroy the appetite for the meal that is coming, and why do the patients like them? Only because physicians have taught them that such disagreeable things are absolutely necessary for their restoration to health. Such a character given of the repetition of doses is a bad one, and, surely, stands in need of correction and improvement.

14. Another reason is given, which appears, but is

not, more rational than the one just noticed. In common practice when a medical prescription has seemed to do good, it is said, "By all means continue to take it." This is a rule quite satisfactory to both doctor and patient. Why is it satisfactory? Because neither doctor nor patient are aware of the mischief which such repetition of the doses of drugs must very often, if not always, be doing. When men's eyes are opened to see what harm drugs in any doses can work in living organs so exquisitely constructed and so consummately performing their several functions, there will be great astonishment not free from indignation, and innumerable bottles of medicine will meet with swift destruction. Hahnemann suggested a better rule—when a remedy has given relief, it is not to be repeated while the relief continues—the objections to it are the practical difficulty of following it, and the responsibility of following it being thrown mainly upon the patient or his attendants, whereas it ought to rest upon the physician. Possibly, a better rule may be found, and the responsibility of obeying it made to rest upon the physician.

Experiments in health.

In the following experiments, generally only one organ is mentioned on which the drug is said to act; it is not to be inferred from this that it does not act on any other organ. The one mentioned is the one which attracted the attention of the experimenter, who, for the most part, was pursuing his usual habits and very busily engaged in his daily occupations.

Aconite—its action on the heart.

My early experiments with this drug were not made with any dose less than *one drop* of the first centesimal dilution; these are given in Essay XXII (1873). During the eighteen years since I have many times taken doses of *half a drop* of the same dilution, and always with the same result—an immediate slowing of the pulse, and this slowing is increased by every repetition of the same dose. It is one action continued and increased, so that the inference is that a too long continuance of the dose would paralyse the heart and stop its motions altogether.

1891, March 31. This evening, being quiet and my pulse 78, I took half a drop of *Acon.* 1. Immediately the pulse became slower; in 2 minutes it was 76; in 4 minutes, 72; in 6 minutes, 68; in 8 minutes, 64, and had then become intermittent, missing one beat every fifteen seconds; this continued for four minutes, and then passed away. The great weakness of old age forbade a repetition of the dose.

The same slowing of the pulse has been observed in numberless cases of inflammatory fever, when this dose of half a drop has been given as a remedy. On one occasion it was given to a boy in the school, who was suffering from a sharp attack of pneumonia, and repeated every three hours for two days or rather more. Rapid improvement took place, but the pulse was brought down from 130 to 40. Certainly it would not have been prudent to have continued it longer, but stopping then, the boy was cured.

Digitalis—its action on the kidneys.

Some years ago I took the Tincture of *Digitalis* in doses of one drop of the first centesimal dilution, two or three times a day for about a fortnight—thinking only of its action on the heart. At the end of this time I was suddenly surprised by a total suppression of the secretion of the kidneys. Of course, I ceased repeating the dose, and the kidneys gradually recovered their natural action. Had the repetition been continued, is it not probable that I should have died of uræmic poisoning?

Castor oil—its action on the bowels.

Mr. Seabroke, in 1876 (see Essay XXXI), took *Oleum Ricini* in doses of $\frac{1}{50}$th of a drop night and morning for three days, and was so constipated for five days that he wisely desisted from taking any more. Surely, mischief would have befallen him if he had not.

Rhubarb—its action on the bowels.

Dr. Brett—"1891, Feb. 21. Two of our household have been experimenting daily with the *Tinct. Rhei,* as you requested (my letter was dated Feb. 11). Hitherto we have not observed anything unusual. The last day

or two my bowels have been more constipated than usual—that is all I am able to say. My friend has not been similarly affected."

"Feb. 23. Since writing the above I have discovered that my bowels are much more constipated than they have been for months past, and my throat has been dry. Certainly I am not so well as before taking the medicine."

"March 2. I am quite convinced that the Rhubarb constipated me, as I have not been so uncomfortable in my bowels for a long time, and *I had to take a pill* to get rid of the stomach-ache and constipation. I am all right again now."

It has been long known as an isolated fact that *Rhubarb* in small doses is a remedy for some forms of diarrhœa. Eighty-four years ago, in 1807, when between two and three years old, I was very ill with diarrhœa, and my great-uncle William Hey came out from Leeds to see me. He prescribed a drop of the tincture of rhubarb, to be repeated a few times, and I was very speedily well. My mother has often told me this. It has been prescribed, I have no doubt, by other medical men. I have now the pleasure of giving a satisfactory explanation of the fact. *Rhubarb* in its small doses cures diarrhœa because it is the natural action of these small doses of Rhubarb to constipate the bowels—as is seen by their doing so in health—and it is an example of the universal or law-fact of Antipraxy, that is, of the contrary action of a group of larger doses and a group of smaller doses of each drug.

Dr. Brett's friend must not pass without notice. The same dose had no perceptible effect upon him. Here comes in the interference of Idiosyncrasy, which often causes great disappointment to every medical practitioner. But this is not peculiar to Medicine, nor to the laws governing drugs; every natural law is subject to interferences of like significance; if you throw a stone up to the heavens the law of gravity will bring it down, but in its fall it may be caught by the branches of a tree, and so not reach the ground, where gravity ought to have brought it.

Another physician, to whom I had made the same request that I did to Dr. Brett, namely, that he would take *one drop* of the tincture of rhubarb night and

morning for a fortnight, and who wrote to say he would do so, took repeated doses of two drops, then of three drops, and then of four drops, without experiencing any effect on the bowels. This is a parallel case to those of castor oil given in Essay LVII. With two of the experimenters five drops of the oil had no effect, while one drop constipated both of them. Medical men have a very inadequate notion how much the effects, even of the commonest medicines, depends upon the *precise dose.*

Veratrum album—its action on the bowels.

This is believed to be the *White Hellebore* of Hippocrates, and his favourite purgative. In modern times it has been laid aside on account of its excessively violent action, when given in ordinary medicinal doses. I have frequently taken it in doses of one drop of the first centesimal dilution of the Tincture, and it has constipated me or restrained diarrhœa.

Dr. S. H. Ramsbotham.—"1891, Feb. 22. I have now taken *Veratrum album*—a drop of the 1 cent. dilution, night and morning for more than a week, that is, since the 15th inst. On the 17th and yesterday (the 21st) I had one motion only, instead of my ordinary two. On the 18th I was full of aches and pains, oddly enough, the most prominent were a sense of constriction across the chest and a curious tension of the sciatic nerves on both sides, as if when I bent my legs it stretched the nerves and made me so uncomfortable that I preferred to sit with my legs sticking out straight in front. All this went off by evening; I did not discontinue the medicine, and have not felt it since So it may have been only cold."

This experiment was repeated in March without any decided effect upon the bowels.

Afterwards, one drop of the first decimal dilution of *Veratrum album* was taken night and morning for a week. Two days during this time the bowels acted only once during the 24 hours, but the next day matters resumed their normal condition. A journey prevented the continuance of the experiment.

"1891, April 3rd. I began one drop night and morning of the strong Tincture of *Veratrum album* a week ago—on Thursday, 26th March. Since Sunday I

have been content with one action of the bowels *per diem*, instead of two, my usual habit; but that stool, though quite formed, seemed to have more moisture about it than usual and it was forcibly expelled at one sudden discharge. Is it not possible that I am here on the line, and getting a mixed action, that larger doses would have a distinctly purgative action, and that though my own idiosyncrasy prevents the action of smaller doses in health, they might nevertheless act in the presence of disease? I have been recalling one occasion when laid up with a sharp attack of choleraic diarrhœa, in which *Arsenic*, and my favourite remedy, *Pulsatilla*, seemed of no avail. Improvement set in after the very first dose of *Veratrum*."

In these trials of Hellebore in small doses it met with strong constitutional resistance to the production of its natural action; but there is sufficient evidence to show that the tendency to that action was present. When these experiments become numerous it may be expected that such obstructions will be frequently met with. We see gravitation interfered with in all sorts of ways every day, but does this lead any one to doubt what its action is?

Bovista—its action on the heart and kidneys.

Mr. Seabroke has furnished me with the following experiment with *Bovista* (Puff-ball). He took one drop of the first centesimal dilution night and morning. The following careful notes were taken each morning:—

	Time.	Pulse.		Time.	Pulse.
1891. Feb. 19	10.45	68	Feb. 20	11.45	51
	11.45	55		12.20	64(disturbed.)
„ 21	10.0	64	„ 23	12.30	59
	10.20	64		1.0	56
„ 25	12.45	58	„ 26	11.45	59
	1.5	54		12.5	55
„ 27	10.30	69			
	10.50	55			

It will be observed that the action on the heart, with one exception which was accounted for by a disturbance, was in the same direction; the heart's beats

became less frequent. The skin was a little drier than usual.

The action on the kidneys was also very noticeable, their secretion was greatly increased; on one day it was excessive, and, as it happened, singularly inconvenient.

The effect of *Bovista* in this dose on the heart resembles that of the small dose of *Digitalis*, not that of *Aconite;* but on the kidneys it is the reverse of *Digitalis*. In the language of Orthodoxy—not Bovista—but *this dose* of Bovista, is a cardiac tonic, and a renal stimulant.

Chamomilla—its action on the liver.

In February I wrote to Dr. Applebe requesting him to be good enough to experiment with *Matricaria Chamomilla* (Wild Chamomile). As the object was to learn the effect of the repetition of the same dose I asked him to take one drop of the first centesimal dilution night and morning for a week or more. I received from him the following interesting letter:

"1891, March 2nd. I have been experimenting with *Chamomilla* for the past ten days, taking one drop night and morning in half a wine glass of cold water. I have also enlisted my pupil, Mr. Beverley, as a volunteer to experiment on. In my case the first dose acted very much on my bowels; it seemed to stimulate the liver, and brought away a good deal of bile. Strange to say, since then, although taking the Chamomilla, I have been constipated, and that is the only effect I notice. Mr. Beverley, on the other hand, finds it acts as a slight laxative whenever he takes it. Though this effect is quite appreciable, if he leaves it off he does not become constipated, but just in his normal state. Neither he nor I notice any other effect of the drug."

"March 26th. On my return home this morning I found that Mr. Beverley had taken notes of the effects of *Chamomilla* 1 in one drop doses [in another experiment]. He is reading hard just now and does not in consequence take very much exercise, the result being that he has had constipation. I enclose you his own notes, which are really instructive and very interesting. I shall begin with half a drop to-morrow morning and continue for a week taking it. I shall then let you know the result of the smaller dose."

Mr. Beverley. "1891, March 26th. Having suffered from constipation for some days, I tried *Chamomilla* 1 in one drop doses, with the following results:—

"The first dose taken at bedtime on March 11th had no effect; the second dose taken first thing in the morning of the 12th, no action; the third dose, taken on the same evening, however, caused a slight action of the bowels; the fourth dose taken on the morning of the 13th caused a relaxed motion; fifth dose taken the same night, ditto; sixth and seventh doses caused relaxed motions; eighth and ninth doses, taken as before, caused two free evacuations on the following day; tenth and eleventh doses caused two very relaxed motions, of a bilious hue; twelfth and thirteenth, taken as before, caused only one relaxed motion; fourteenth and last dose, similar to tenth and eleventh doses, viz. two very relaxed motions of a bilious hue."

Dr. Applebe. "1891, April 8th. I have tried (as you suggested) half drop doses of *Chamomilla* 1. I took one dose every night at bedtime for ten days. The effect was similar, viz. slight diarrhœa each morning, for six days; then no effect was produced until the eighth day, when the bowels were freely moved again. I think there can be no doubt but that Chamomilla is a good hepatic stimulant. Evidently, in my own case the drop dose taken at first was too large; but the smaller dose was enough to create the secretion of bile sufficient to purge me gently."

Chamomilla in the first centesimal dilutions was experimented with on myself in 1874, and its action on the liver was clearly indicated. (See Essay XXVI.)

The preceding testings of small doses of drugs may be objected to as fragmentary. They are of exceedingly great *practical* value for all that. The following experiment, undertaken for me by Mr. George Percy Richards, is free from this objection:—

Belladonna—its actions.

"1891, Feb. 12. One minim Belladonna 1 (first centesimal dilution) thrice daily.

Result—No appreciable variation in pulse, respirations or temperature. Pupils normal—opthalmoscope shows fundus oculi normal. No change

apparent in excretory system, either skin, urinary, or intestinal.

Feb. 13. Repeat.

Result—Unable to detect anything unusual until 8 P.M. At 7·30 P.M. pulse 75. Respirations 19. Temperature 99° F. At 8 P.M., twenty minutes after taking the Belladonna, pulse 71. Respirations 18. Temperature the same (99° F.).

Feb. 14. Repeat.

Result—8 A.M. Pulse 70. Respirations 18. Temperature 98°. 3 P.M. Pulse 68. Respirations 18. Temperature 99°. A decrease in the amount of urine excreted. 10 P.M. Pulse 65. Respirations 17. Temperature 98°. Mucous membrane of the tonsils slightly pale. Pupils "very" slightly contracted. Bowels normal. No change apparent in the skin.

Feb. 15. Repeat.

Result—8 A.M. Pulse 71. Respirations 19. Temperature 98·2°. 3 P.M. Pulse 67. Respirations 17. Temperature 98·5°. The amount of urine daily excreted is certainly less. The bowels remain normal, if anything slightly confined. Pupils slightly contracted. Tonsils somewhat pale. 10 P.M. Pulse 68. Respirations 19. Temperature 99°. The pulse during the whole day is very firm and hard. This points, I think, to a stimulation of the vaso-motor centre in the medulla, or more probably to a stimulation of the non-striped muscular fibres of the blood-vessels.

Feb. 16. Repeat.

Result—8 A.M. Pulse 65. Respirations 17. Temperature 97·6°. 3 P.M. Somewhat depressed. Pulse tense 68. Respirations 18. Temperature 98·6°. I notice, however, that although feeling so depressed, yet the mind seems exceptionally clear. Pupils certainly contracted. Asked a friend to examine the fundus oculi, without naming the experiment to him, he says the fundus is normal, if anything "somewhat pale." Urine remains scanty and pale in colour; phosphates less than normal; no albumen whatever. Bowels somewhat confined. 10 P.M. Pulse 68, hard. Respirations

17. Temperature 98·5°. Depression is very marked.

Feb. 17. Repeat.

Result—8 A.M. Pulse 70. Respirations 19. Temperature 98°. Not nearly so much depression. The pulse remains firm. 3 P.M. More depression. Pulse 68, firm and wiry. Respirations 18. Temperature 98·4°. When walking perspire very readily although the day is cold. Bowels remain somewhat confined. 10 P.M. Pulse 67, hard. Respirations 17. Temperature 97·6°. Considerable depression, an exceptional thing with me. Can read easily without the glasses I generally wear (+ 1 D). Urine remains scanty and pale. Pupils contracted (decidedly).

Feb. 18. Take 4 minims of Belladonna 1 thrice daily.

Result—8 A.M. Pulse 68, firm. Respirations 19. Temperature 98·2°. Still depressed. 3 P.M. Depression marked. Pulse 66, remains hard. Respirations 17. Temperature 99°. 3 P.M. Pupils remain contracted. Conjunctiva yellowish in colour. Bowels very confined. Urine still scanty and pale. 10 P.M. Depression. Pulse 67, wiry. Respirations 17. Temperature 99°. Pupils contracted. Tonsils and throat generally, pale. There is also an increase in the amount of saliva.

Feb. 19. Repeat.

Result—8 A.M. Violent diarrhœa. Pulse weak, 78. Respirations 20. Temperature 97°. Pupils contracted. 3 P.M. Diarrhœa continues. Urine scanty (very). Depression very marked. Considerable perspiration. Throat remains pale. Still continue to read without the glasses. Pulse 75, full but not hard, readily compressed. Stools very bile-stained. 10 P.M. Pulse 73, but weak, evidently the blood pressure is low. Respirations 20. Temperature 98°. Pupils contracted. Bowels moved twice during the evening. The salivary secretion is increased.

Feb. 20. *Cease taking the Belladonna.*

8 A.M. Pulse 70, not so weak. Respirations 19.

Temperature 98°. Depression far less. Pupils somewhat contracted. Bowels have not been moved. Urine scanty, higher in colour. 3 P.M. Bowels moved at 2 P.M., diarrhœa very slight. Pupils not contracted so much. Conjunctiva normal. Less perspiration when walking. Pulse 74, not nearly so readily compressed. Respirations 19. Temperature 98°. 10 P.M. Bowels moved at 7 P.M., diarrhœa very slight. Pupils slightly contracted. Urine less scanty. Pulse 72, rather full. Respirations 19. Temperature 98·7°.

Feb. 21. 8 A.M. Pulse 70, normal. Respirations 18. Temperature 98°. (See Feb. 14.) Pupils can hardly be said to be contracted. 3 P.M. Pulse 76, normal. Respirations 20. Temperature 98·8°. Bowels normal. No depression. No excess of saliva. Perspiration not apparent. 10 P.M. Pulse 72, normal. Respirations 18. Temperature 99°. Urine more abundant.

Feb. 28. Pulse, respirations, and temperature are quite normal. Excretory system as well. Pupils not contracted. Throat not pale. No depression whatever.

"It will, I think, be interesting to compare these results with certain larger doses of the 'mother tincture.' We know that certain large doses dilate the pupils and diminish all the secretions of the body except that of the kidneys. Certain doses paralyse the 3rd nerve and stimulate the sympathetic, and we get dilated pupil and paralysis of accommodation for near objects, *i. e.* their image is blurred. This is interesting in regard to my reading excessively well without glasses. We also know that certain doses produce cerebral excitement, with marked stimulation of the respiratory centre in the medulla. We also know that certain large doses produce increased pulse rate, due probably (1) to an inhibition of the inhibitory fibres of the vagus, and (2) stimulation of the cardiac ganglia. The variations in temperature are very slight when compared with the *diurnal variation tables*, such as those of 'Landor's and Stirling's Physiology.' The depression of the pulse and respiration is much more marked. The attack of diarrhœa on Feb. 19th I will not attempt to explain."

Conclusions.

15. Two conclusions, as stated at the end of Essay LVIII, have already been arrived at. The first has reference to *drugs*. Many years ago (from Essay XVII, 1867, onwards) I took pains to show that each drug has a characteristic local action on some organs or parts of the body, while other organs or parts were affected subordinately, and others apparently not affected at all, and I think it is clear that this local or organic action is a universal or law-fact, embracing all drugs. I was careful to abstain from any attempt to explain this mysterious property, but in order to gain attention to a matter of so much practical importance, I had the presumption to give it a name, and called it *Organopathy*. Dr. Hering, one of Hahnemann's friends and co-workers, received it with vulgar ridicule as an absurdity; and a Professor of Pathology in an English university met it with contempt as what everyone had known from time immemorial. Nevertheless, as a *law-fact* it is true notwithstanding Dr. Hering's ridicule, and it is a new truth notwithstanding the Professor's contempt.

16. The second conclusion has reference to *doses*. This has been gradually reached by persevering with experiments. It was first found (see Essay XXII, 1873) that each drug has a group of doses acting in one direction, and another group of smaller doses acting in the contrary direction, and that it is these smaller doses that are most successful in the treatment of diseases. *Aconite* is given as the first example, and it is said, "A dose of one or of two drops of the first centesimal dilution first quickens the heart's action for a short time (one, two, or three minutes), then retards it." And it is remarked that of all the varied actions of Aconite upon the heart, "the only curative influence is the second action of the small dose." I had not then tried less doses than *one drop* of the first dilution. This was eighteen years ago. On numberless occasions since then I have taken *half-drop* doses of the same dilution, and have invariably found the heart's beats to become slower at once, without any first quickening. It has also been found that there is another group of doses between the two with contrary actions, which has both actions. My readers will be

good enough to remember that all doses having double or opposite actions belong to these intermediate groups. Experiments in health have now abundantly proved that each drug has, at least, one group of larger doses having only one action in one direction, and another group of smaller doses having only one action but in the contrary direction to that of the larger doses. These contrary actions of larger and smaller doses are not double and opposite, but single and opposite. To make what I mean very plain let me give an example:—Everyone knows that there is a large dose of *Belladonna* which *dilates* the pupil, and as regards the pupil does nothing else—this is not a double but a single action. There is also a small dose of Belladonna, as I found many years ago by experiment on myself, and as we see confirmed in Mr. Richards' experiment in this Essay, which *contracts* the pupil, and as regards the pupil does nothing else—this is not a double but a single action. The actions of the larger and the smaller doses are contrary to each other, they are not double and opposite, but single and opposite. To this contrary action I have also had the presumption to give a name, and have called it *Antipraxy*. This conclusion has, like the first, been perseveringly criticised and objected to. There are some minds whose reasonings I suppose are intelligible to themselves, but I confess myself quite unable to understand them; and there are other minds who draw inferences from my statements that, to me, are surprisingly irrelevant. My consolation is this, that neither obscure reasoning nor false inferences can alter the facts, and that these are so plain that I cannot but believe that, sooner or later, they will be seen and acknowledged.

17. A third conclusion may, I think, now be added. It has reference to *the repetition of the same dose*. The experiments now recorded, it will be seen, relate to the small doses which have only one action in one direction, and they show clearly that these doses tend to *repeat that action each time the dose is repeated*. A dose of *Aconite* which slows the pulse and does nothing else, will continue to slow it however often the dose is repeated. The characteristic of the repetition of such doses is uniformity or continuity; this, then, is a law-fact—a third conclusion. If apparent exceptions to this law occur, it will be because obstacles have arisen to prevent the

natural action taking place. It will be noticed that the intermediate groups of doses having double and opposite actions, are not included as subject to this law. I have no experiments showing by what law the repetition of such doses is governed. This, however, is not important, as no dose with double and opposite actions is fit to be prescribed as a remedy. When taken in health the organs these doses act upon must be torn in pieces first in one direction and then in the opposite, and when given to patients, if they do good at first, must do harm afterwards.

These experiments on the repetition of the same dose have also another voice, which proclaims a caution. They tell us that a dose having only one action in one direction may be repeated till it threatens a fatal termination. The lesson to be learned from this testimony is a serious one, it is this—that medical men ought to watch their patients while they are continuing to take the same dose of a medicine, and ought not to allow the repetition of it beyond what the successful treatment of the case renders imperative. This is a corollary to the third conclusion which will surprise many, but a thoughtful study of the experiments seems to make it unavoidable.

18. If small doses can kill, how many must be killed by large ones! It may be that the number of deaths thus caused is diminished by the custom of mixing several drugs together, which partially antidote one another by "fighting together in the dark"; but even in these cases the delicate organs in which the battles are fought must suffer very much.

19. We have, therefore, now three law-facts. The first, that of the local or organic action of each drug. The second, that consisting of two halves—the contrary action of a group of larger and a group of smaller doses of each drug—and the action in one direction only of these groups of doses, with the action in two opposite directions in succession of the intermediate group. The third, that of the uniform or continuous action of the repetition of the doses which act in one direction only. This third law-fact seems to be distinctly proved with reference to small doses, by the experiments detailed in this Essay.

My most grateful thanks are now respectfully offered

to each of my co-workers in this supremely interesting field of labour.

20. It may be worth while to remark that the question, what *intervals* are to be allowed between each repetition of a dose cannot be answered in this Paper. It is a difficult question, because each drug will have its own character in this as in so many other particulars, and each, therefore, will require to be experimented with by itself. A vast expanse is here brought into view for more than one generation of doctors.

21. In works on Medicine we read a great deal about the "re-action of the organism" against the action of medicines; and also about the "toleration of the organism" of their action. Of course, these are subjects demanding attention, but they are outside the limits of the present inquiry, the object of which is to learn the natural action of drugs. It may, however, be noticed that many effects are attributed to these causes —re-action and toleration—which are really the effects of *the changes in the working of different doses* of the same medicine. These changes in the effects of different doses of the same drug are a lesson taught by all my experiments on doses; and it is a lesson so important that I would fain hope that it will not be neglected by my medical brethren. On the other hand, the continuity of the same action by repetition of the small doses, as seen in this Essay, is also important; we ought to learn from it that if we wish to increase the effect of a small dose, we are to seek to obtain it by repeating the same dose, not by increasing the size of it. Small doses of drugs act like small drops of water—drop—drop—drop —wears away the stone.

Reflections.

22. The first reflection which offers itself is so obvious and striking that it can require no illustration to explain it, nor any argument to enforce it. Every one knows that the object Hahnemann proposed to his Profession, by his experiments with drugs in health, was that each drug may be prescribed as a remedy for such symptoms of disease as are *similar* to those which the drug causes when taken in health. His "Provings" contain many thousands of these symptoms; and when the "totality of the patient's symptoms" can be found among those

of the drug, that drug is the remedy. The object proposed in the experiments described in these Essays is directly the opposite of this. It is that each drug may be prescribed as a remedy for diseases, when its kind of action, so far as both can be ascertained, is *contrary* to that of the disease. For this to be possible, the seat of action of both disease and remedy must be *the same.*

23. This contrary action of the drug is the action of a group of its small doses, learned by experiments *with these doses* in health. A second reflection, therefore, is that the labour and suffering of those who undertake these experiments are much less than those which Hahnemann expected from his co-workers. Our knowledge of the effects of larger doses of the same drug may often be useful, but it is not essential. It is essential for us to know what the small doses can do, if they are to be prescribed with confidence.

24. At this moment the improvement of therapeutics is earnestly pursued by four different methods. There is the old method of Empericism, which can never, except by chance, either improve our present treatment, or give us remedies for the many diseases for which we have none. There is the Chemical method enthusiastically pursued by many able men, in forgetfulness that the living chemistry going on in man's body cannot be approached in the dead chemistry of the laboratory. There is the Microscopical method just now so conspicuously handled by Dr. Koch; of this the latest development seems to be the transfusion of the blood of rats into the veins of the unhappy sufferers from consumption; of this method all that it becomes me to say is that I have no desire to join in its pursuit. And there is the method of these Essays.

25. It is objected to Antipraxy that it is imperfect—it has yet no remedy for tubercle or melanosis. This objection is a two-edged sword, it cuts both ways. If ever the *tu quoque* argument is a legitimate one, it is so in this instance. Imperfection is stamped upon every method of healing, and the older the method is the greater must the imperfection appear. Antipraxy is very young.

26. A letter reached me on March 23, 1891, from a native physician in India, highly complimenting the

'Essays on Medicine,' but containing a criticism of the six Essays (from 45 to 50), published together in a volume. "The name of the book should have been 'Homœopathy and Hahnemannian system reformed,' not 'Therapeutics founded on Antipraxy.'"

27. I have frequently acknowledged, and am glad to acknowledge again, my obligation to Samuel Hahnemann for attracting my attention to two things—the value of experiments with drugs in health, and the necessity of experimenting with, and prescribing, only one drug at a time. But here we part company; for, not being able to accept his dogmatic teaching, I am driven from his presence by his own uncompromising judgment—it is quoted from Dr. Dudgeon's 'Biography of Hahnemann'—"He who does not walk on exactly the same line with me, who diverges, if it be but the breadth of a straw, to the right or to the left, is an apostate and a traitor, and with him I will have nothing to do." It has been said before that the Homœopathies of other men I have not undertaken to investigate, time would have failed me for that, but it will not escape notice that, notwithstanding all the unbounded praises they lavish upon Hahnemann, if they differ from him "the breadth of a straw," they are, equally with me, under his condemnation as "apostates and traitors."

28. The conclusions arrived at in these Essays, so far from being a reform of Homœopathy, are a direct contradiction of it, especially in these particulars:—Hahnemann, in his Provings, gives us more than enough of symptoms, but he sternly rejects Pathology. My experiments have led me to found Therapeutics on the local or organic, that is, on the pathological action of the causes of disease on the one hand, and on the local, organic, or pathological action of drugs on the other hand; so that the seat of the disease and the seat of the action of the remedy given for it shall be the same. Hahnemann, in his Provings, takes no notice of, and gives no information about, doses. Whereas, to me the dose is as essential as the drug. My experiments teach that the actions of different doses of the same drug may be arranged in groups, each group having a characteristic action of its own, and that the contrary action of a group of small doses is the best adapted for the curative purposes of medicines. Of these contrary actions

Hahnemann knew nothing. Hahnemann believed, and others have contended for years, that every dose has two actions—"double and opposite," "primary and secondary" actions. "There are always two actions from one and the same dose. This is a universal law." The experiments recorded in these Essays prove that there are groups of doses which have only one action, or action in one direction only, and experiments on the sick prove that the small doses with this action in one direction only are the most successful remedies.

29. It is pointed out, in the early part of this Essay, how unsatisfactory Hahnemann's experiments with drugs in health are in the form in which he has given them to us. There is one feature in this form of them, which is extremely perplexing, and which has not obtained the attention it deserves—namely, in all the provings of the more important drugs, conditions the most opposite in character are described without the smallest indication as to what may have been the causes of these contrary conditions. If, for instance, we take one of his shorter provings and read that of *Veratrum* in his 'Materia Medica Pura,' we shall find, among the 400 symptoms, the following :—

Eyes—"Contraction and dilatation of the pupil."

Face—"Pale face, dark red hot face; cold face, extreme redness and heat of the face."

Appetite—"Nausea and vomiting, hunger and great thirst."

Stool—"Frequent diarrhœa, chronic constipation."

Urinary organs—"Scanty urine, diuresis."

Larynx and chest—"Dry cough, cough with profuse expectoration."

Moral symptoms—"Silence, he does not talk, extremely lively."

It is probable that these and other opposite conditions arise from one or the other of two causes—the difference in the doses of the drug, or the Idiosyncrasy of the experimenters; but there is nothing in the provings to show to which of these causes any of these contrarieties are owing.

30. Let me once more impress upon my readers that it is entirely misleading to classify drugs under such heads as Stimulants, Narcotics, Aperients, Astringents, &c., &c. Is not *Opium* in small doses a Stimulant to the

brain, and in larger doses a Narcotic? Is it not in small doses an Aperient and in large doses an Astringent? And so of all the rest. This mode of classification is a long established custom, but it is a false and deceptive one, and it is high time that it should be banished from all works on Materia Medica and Medicine.

31. Mr. Richards' experiment is a most instructive and valuable one, but even this, careful as it is, is an illustration of the last paragraph, and it requires to be divided into two. During the first six days, one minim of *Belladonna* 1 was taken three times a day. Throughout these days the action on each organ, so far as it could be observed, was continually increased or intensified. On the two following days four minims were taken in each dose, and in one of the functions there was a great change—the bowels had before been confined, now they were violently relaxed. It is universally recognised that a change of the drug is a change of the experiment, but in future it must also be recognised that a change of the *dose* is a change of the experiment. A dose of one minim is not responsible for what another dose four times as large may do. In Hahnemann's provings of Belladonna, (he gives 1440 symptoms), diarrhœa is conspicuous; this must have been caused by larger doses than the one hundredth part of a minim of the strong Tincture. Among the conditions described by Mr. Richards are depression; contracted pupils; pale throat; deficient secretion of the kidneys; deficient action of the bowels. These effects are so many distinct indications—not that Belladonna—but that *this dose* of Belladona may be confidently prescribed for an excited brain; for dilated pupils; (and *ab usu in morbis* for some form of defective sight); for an inflamed throat; for some cases of diuresis; and for some of diarrhœa. What an amount of practical teaching has been given us in this one experiment! Thanks to Mr. Richards.

32. A short time ago I was endeavouring to persuade a medical man to prescribe only one drug at a time, when I was silenced by his curt reply:—"We eat bread and butter together." It puzzled me to imagine what analogy could possibly be seen between food and drugs, the former being so necessary to life that if deprived of it we soon die, while the latter are poisons differing

chiefly in the degrees in which they have power to kill us. And like the dreamer of Bagdad (in the "Visions of Mirzah"), "I fell into a profound contemplation on" one aspect of "the vanity of human life"—the lack of thinking with which even educated men are content to go through the round of their daily work.

33. We hear of Research Societies, Research Fellowships, Research Funds, and Government allows the Royal Society four thousand pounds a year to promote such researches; to some all this is only hearsay. My work has been carried on for more than forty years, not only without encouragement, but in the face of incredulity, ridicule, silent neglect, and violent opposition, both from orthodox and homœopathic physicians; howbeit the world's history testifies that the truth and value of work done is not thereby made one whit less certain, and we know One who has said, "Wisdom is justified of her children."

HORTON HOUSE, RUGBY;
April 24, 1891.

BY THE SAME AUTHOR.

ESSAYS ON MEDICINE.

ESSAYS XLV—L.

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