

An examination of the evidence in regard to infinitesimal doses / by W.W. Rodman.

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The Evidence

IN REGARD TO

INFINITESIMAL DOSES.

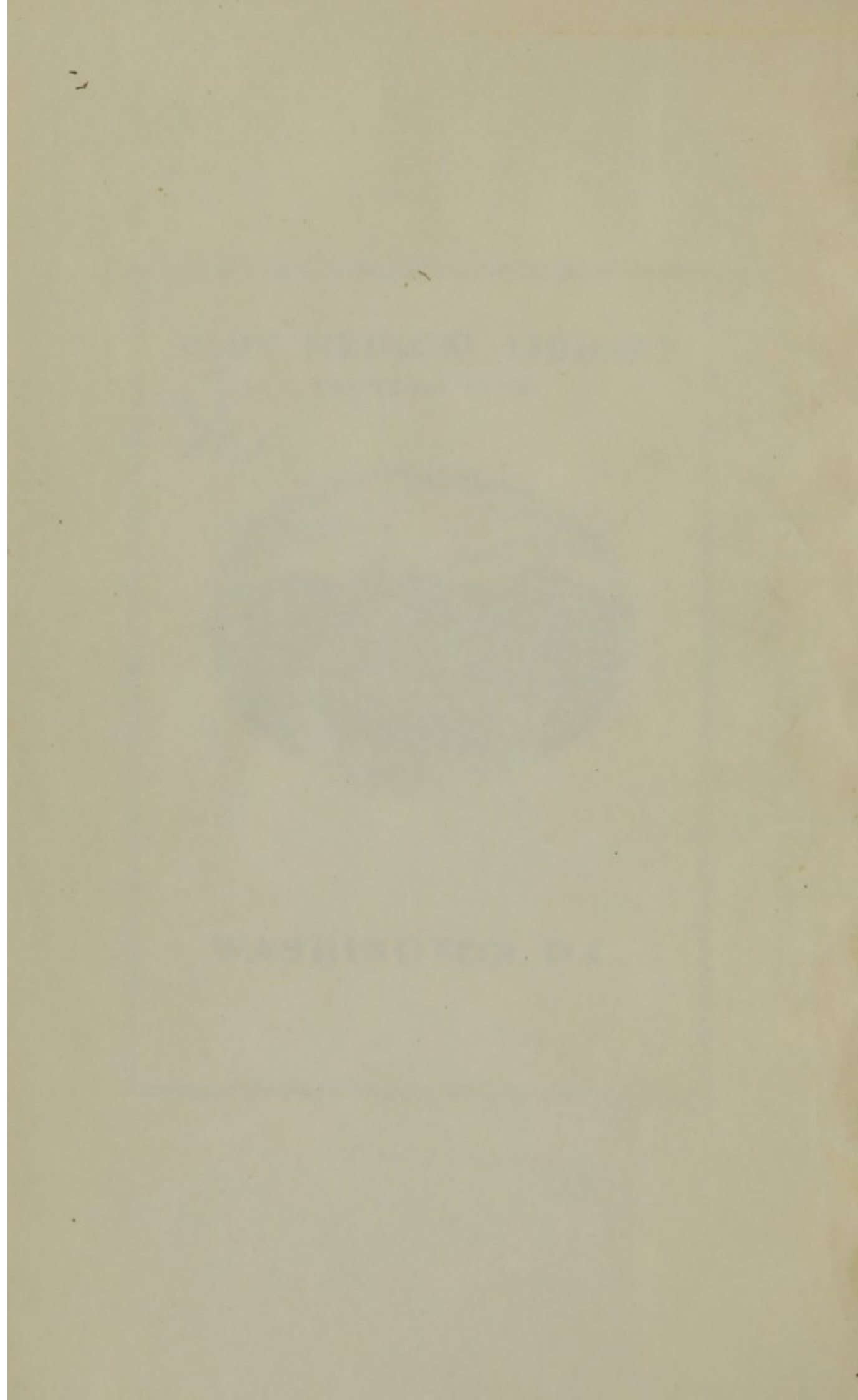
BY

WM. W. RODMAN, A. M., M. D.

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WATERBURY, CT.,
WILLIAM PATTON.
1851.

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NOTICE.

Although in adducing facts to establish the several positions herein taken, I have generally referred to standard Allopathic authorities, and only quote from Homœopathic works when it is expressly stated that they are such, it should be remarked that it is in the latter class of works alone that the subjects are fully treated. To one disposed to pursue the enquiry, an unlimited amount of material is accessible, much of which bears directly on the different doctrines and positions which in the following pages are incidentally brought to his notice.



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EVIDENCE

IN REGARD TO

INFINITESIMAL DOSES.

INTRODUCTION.

It would seem to be a maxim among physicians, that those holding conflicting opinions should not come together to compare their several views, and modify their practice. Nay, that they must be an exception to the rule that "it becomes all men to maintain peace and the common offices of humanity and friendship in the diversity of opinions." If this is so, it is a duty which all intelligent men owe to themselves and to others not to be deterred from entering the difficult labyrinths of medical evidence, but to listen to their respective claims, and sit as umpires between the discordant doctors. From such, and especially from the medical profession of every school, I would ask a candid decision on the following pages. The effects of medicines and the treatment of disease are subjects in which all men have a personal interest. The principles of medical science are not so settled, the honest opinions of its professors are not so united as to make it improper for any man to examine any mode whereby it is proposed to allay pain or cure disease.

Statement of the question.

The question I propose to examine may be thus stated. Certain practitioners assert that medicines have valuable properties in doses much smaller than has usually been

thought possible. That these properties are distinct from the cathartic, emetic and other disturbing effects of the remedy. That in doses much smaller even than those whereby are usually obtained the "alterative or deobstruent" effects of medicines, there is an alterative power having definite relations to the effects of the same remedies in larger doses. To this power they give the name Homœopathic, and the doses in which it is claimed to exist are often so small that the term "infinitesimal" has been adopted to characterise them. *Has this power a real existence*, and if so, can it be made available in the cure of disease? Such a question it would seem might easily be settled.

The advocates of these doses moreover avow the belief in certain theories, which with the evidence on which they are based, constitute, as they claim, a system of medical science. The truth of the theories will depend first on the truth of the asserted facts, and secondly, on the correctness of the reasoning upon them. As the character of a process of reasoning depends on the accuracy, and is liable to the fallacies, of human judgment, I shall pay but little attention to the mere theoretical claims of the homœopathic system, and shall content myself with examining the truth of their assertions on the single point, viz: that their medicines in the doses in which they use them are effective as medicines.

Definition of Infinitesimal.

In using the term infinitesimal it is hardly necessary to say that I do not have any reference to infinity in the absolute or in the mathematical sense. Nor do I include the extreme dilutions used by some homœopathists. Among themselves it is an interesting question, how far the division of the dose may be carried, but as the present issue is with others, such enquiries are irrelevant.

But I do mean, 1st, such doses as, according to ordinary views may be fairly styled infinitesimal. Such as would not admit of explanation on any commonly received principle. Such as are many times smaller than the smallest allopathic dose of the same remedy. 2d. That they shall, at the same time, be such doses as homœopathsists are agreed upon as furnishing simply the homœopathic effects of drugs, thus excluding, on the one hand, doses so large that some appreciable disturbance of the system would usually result from their administration, even if the homœopathic effect might be obtained, and on the other, the high dilutions already referred to. Many homœopathsists have never used these, and therefore do not claim to have any decided opinions in regard to them.

By thus limiting the term infinitesimal to the doses commonly used by homœopathsists and recommended in their books, we secure a definiteness to the enquiry which, in the discussions on this subject, has not always been maintained. Whether such doses can ever so far affect the human system, as to excite or assist its natural restorative powers to the cure and palliation of disease, is the question at issue.

Negative alternatives threefold.

Those who deny the efficacy of infinitesimal doses in specific cases must do so on one of the three following grounds :

1st. This efficacy is absolutely *impossible* or incompatible with certain demonstrated truths.

2d. It is so *improbable* as to amount to a moral impossibility, in other words, there is so much evidence against it that no attainable evidence can prove it.

Or, 3d. It is so improbable that the evidence adduced in its favor does not prove it.

That twice two is equal to six cannot be true, that the sun will not rise to-morrow cannot be proved, that a mode may be discovered whereby light and heat can be obtained at an expense which compared with previous methods, might be fairly styled infinitesimal, has not been proved. These examples illustrate the above distinctions.

The efficacy imputed to these doses not an absolute impossibility.

I am not aware that the first of these positions has ever been assumed by any one. The opponents of homœopathy frequently speak of the effects imputed to its doses as being impossible. These effects have even been said to involve an absurdity, similar to the claim that the part is greater than the whole. No one, however, seriously means I presume, that such effects involve a contradiction with any ascertained truths. No demonstration, certainly, of the impossibility of the asserted facts has ever been produced. Were this once done, it would be conclusive, rendering all argument superfluous. The "possibility" is yielded when facts and arguments are appealed to, as the proofs of the inertness of homœopathic doses.

Not a moral impossibility.

The second position, viz : that the *improbability* of these doses having remedial power is so great that the supposition cannot be true, is the one usually taken, or at least acted upon by the profession. When the subject forces itself upon their attention it is declared to be incredible because it is irreconcilable with undoubted facts and principles. The published attacks upon homœopathy, so far as I have met them, are attempts to settle the question by establishing a conclusive *a priori* presumption against the system. So complete is the incredibility supposed to be, that it is not thought necessary to furnish detailed experiments, or any analysis of the testimony of

homœopathists, nor to produce the statements of such witnesses as give proof of being competent, by showing that they were acquainted with the homœopathic system. Professor Lawson, in the *Western Lancet* (Feb. 1846) says, "No one who has the slightest acquaintance with the laws that govern the human system and the effects of medicines, can for a moment believe that infinitesimal doses of medicines, such as properly belong to the homœopathic system, can under any circumstances exert the slightest influence in the cure of disease." Dr. Worthington Hooker says, "The homœopathist attributes palpable results to doses of medicine which are so small that they cannot produce any perceptible effect except by a miracle."* Dr. John Bostock, in his *History of Medicine*, dismisses homœopathy with the single remark that "no medical testimony is sufficient to establish a fact which is itself incredible, and that this previous incredibility can only be ascertained by an extensive and accurate knowledge of the functions and properties of the living body, both mental and corporeal in all its modifications, and under all circumstances, and by a correct and careful generalization of the knowledge thus obtained."†

To show that this position cannot be maintained, I would urge the following considerations :

For, it is not miraculous.

It may be extremely improbable that medicines can have valuable properties in so minute doses. No mere improbability, however, can be so great as to justify us in declining to examine so important a subject, by experiment, and by fairly weighing the testimony of others. No statement whatever, the evidence against which, is that of moral probabilities merely, can be so incredible

* Lessons from the history of Medical delusions. New York, 1850. Page 25.

† Cyclopedia of Practical Medicine. Philadelphia, 1845. Vol. 3d, page 228.

that "no testimony is sufficient to establish it." At one stage of our knowledge we can never be certain of the amount of probability when that knowledge is increased. Had some of us been told some years since, that a mode could be devised whereby thought could be instantly transmitted to a distant city, we might have rejected a truth for its extreme improbability. That a homœopathic dose should produce a perceptible effect, is no *miracle* unless it is "opposed to universal experience." The experience of those who have tried these doses may perhaps be found *not* to be universally against them. But if it really is a miracle, it may even then be proved by testimony. Even Hume, in his famous essay on this subject, admitted so much as this. He says, "I beg the limitations here made may be remarked when I say that a miracle can never be proved so as to be the foundation of a *system of religion*: for I own that otherwise there may be miracles or violations of the usual course of nature of such a kind as to admit of proof from human testimony."

Or incredible.

But I claim next that the improbability in this case previous to experiment and testimony is not so great as the preceding suggestions would imply. Such improbability can only be proved by showing, 1st: that Analogy is entirely at variance with the claim that medicines in infinitesimal doses affect the bodily organs, and 2d: that the peculiarities of the homœopathic system, and collateral circumstances furnish no presumptions in their favor. Should either of these suppositions be erroneous, the claim would be brought within the limits of credibility, where a certain amount of evidence would establish our proposition, and were this unattainable, where a less amount might render it probable, or at least show it to be possible.

It is not without Analogy.

The argument from analogy, appears to furnish the strongest objections which are urged against infinitesimal doses. The efficacy imputed to them is commonly thought to be utterly unlike all the ascertained properties of material agents. Language is said to be inadequate to express the absurdity of believing in this efficacy. The claim in their favor is thought to be "a gratuitous outrage to human reason." It is important to determine wherein the force of this argument consists, and how much weight is due to it.

That the efficacy imputed to these doses is extremely improbable previous to evidence in their favor, no one denies. But we daily see extreme improbabilities give way to evidence, and what was regarded as impossible, is often found to be true and rational. The opposers of infinitesimal doses do not, I think, usually state clearly why these should be an exception, and the evidence in their favor should not even be examined. So far as I can see, the argument would be thus stated: It is highly improbable because unlike all the ascertained phenomena of nature, that power sufficient to affect the vital functions can exist in quantities of matter, which are infinitesimal in weight and bulk. This improbability is increased, if "appreciable" quantities of the substance do not produce such overwhelming results as would be expected if the minute amount has any effect. The improbability becomes moral certainty, if large quantities do not usually manifest any effect on the vital functions, or if we are familiar with the common properties of the substance without finding any thing in them to indicate such powers. If this is not a fair statement of the grounds on which the conclusion is based, I think the argument needs re-investigation.

From various sources.

On it I would observe, 1st. It is not absurd, though it be improbable, that effects should be produced by means which, so far as is yet known, are altogether inadequate to their accomplishment. The records of science are little else than a catalogue of such marvels. As a general rule, the formative and the remedial agents of nature are those which would have appeared to be quite incompetent to the purpose. The insect, the snail, and the animalcule are vastly better architects than all the monsters of the sea and land. The gentle breeze, the sunshine and the dew are the restorative and developing powers, while those immensely more active, the tornado, the earthquake, and the flood, are fit merely to disturb and to destroy. We are just beginning to learn how effective are agents quite as immaterial as the doses of the homœopathist, and of whose very existence we possess no other evidence than the phenomena we extort from them, and as their properties are unfolded, each year brings its host of discoveries, which astonish us both by their immense results and the apparent feebleness of the means.

2d. It will not do to take it for granted that the power of physical agents is always proportioned to their weight and bulk. The amount of matter, necessary to produce a given result, will depend on circumstances, one of which would be the state of aggregation of the particles, according to which it would be more or less diffusible. It would also depend on the delicacy and susceptibility of the thing to be acted upon. That susceptibility would only be blunted, if the keenness of the agent was not proportioned to its own. If the result aimed at was to overthrow a mountain oak, the thunderbolt of heaven might be requisite, but if it was to restore to right action the delicate vital force, the result would hardly be pro-

portioned to the amount of power used. It might also depend on circumstances of which we know but little. The next breath might bring to us a hidden poison utterly inappreciable to our tests. A slight current of air might do more to sicken than the gale of yesterday.

3d. It is not inadmissible that a substance with which we are familiar should have latent properties of a very energetic character. We may never have tested them in precisely the circumstances in which these properties would show themselves, or if they have occurred, we may have erroneously attributed them to some other cause. The seed might furnish us food for a long period, and if we knew nothing of its germinating power, we should never suspect that such a force lay dormant within it. Much might be said to disprove it. If, as you say, one seed will produce its hundred, and each of these its hundred again, and so on—you would soon have the world filled with it alone, there would be no room for any thing else. Again, each one of your first crop would have but a hundredth part of the original vital principle peculiar to it, and thus they would diminish in power at so rapid a rate as soon to exhaust themselves. Your statement is absurd: it is an outrage to human reason to ask us to plant a seed and try your experiment. The bar of iron in our hands may be a magnet and we know nothing of it. It may even have the strange power needed for making new magnets *ad infinitum*, without losing its own potency. We might see metals rusting under the action of acid, all our lives, without appreciating the galvanic force.

The ascertained results of vaccination prove: 1st, that matter may have latent properties only to be learned by specific experiments on the human system; 2d, that these properties may be not at all proportioned to the weight and bulk of the vehicle, as it is almost a matter of

indifference whether much or little of the virus is used ; 3d, that what would appear to be a process of dilution is not necessarily such, as no perceptible diminution of the peculiar efficacy results from continuing the process from individual to individual, though thereby, but an infinitesimal portion of the original agent can be retained ; 4th, that there is a diversity in the susceptibility of different persons to such an agency, only discoverable by experiment ; 5th, that important changes may be going on in the system without any evidence thereof being exhibited during at least part of the process.

Such analogies, it will perhaps be said, are too "loose to form the foundation of a system of medicine." Very true, but they should at least teach us caution in the argument.

In what we know of medicines.

In prosecuting the argument from Analogy, we are now prepared to enquire what light is thrown upon the subject by what we know of the action of medicines upon the human system. A little reflection will show us, I think, that we know so little of the effects of remedies previous to experiment with them, that the powers imputed to even infinitesimal doses may be credible. That, for example, a man may avow the belief that "a mere atom" of arsenic or belladonna has remedial properties in certain states of the system without forfeiting his character for soundness of mind or honesty of motive.

If this is denied, any argument against it must be based on its being utterly unlike all that we know to be true either of the remedy itself or of the human system in health or in disease ; including of course such phenomena as result from their mutual relations.

If the position is taken that what we know of the remedy shows it to be unreasonable that so minute a portion of it should exert a medicinal action, my reply would be that however different it may be from our ac-

customed views of these articles, though there be no link whereby we can connect it with what we do know of them, it is not incompatible with that knowledge. We know that a certain quantity of arsenic or belladonna will poison. We learn this not from their chemical or sensible properties, but because they have poisoned, and we have found that if the dose be diminished within certain limits it will no longer do so. But we could not infer from these facts what their properties would be in still smaller doses. That either of them had valuable properties as a "tonic" or "narcotic" could only be proved by experiment, and experiment alone would show that when the dose was still further diminished these useful properties would be lost. Even smaller doses might be valuable in appropriate cases for all that we have learned to the contrary, and nothing but experiment in those cases could either establish or disprove it. A minute dose of ipecac is said to quiet a sick stomach under certain circumstances, though too small to produce any appreciable effect upon a healthy one and although a larger dose would have directly opposite effects.* The question would depend on the evidence, and could only be settled by experiment. That matter retains its identity when "infinitesimally" divided, and that in this state it is capable of affecting the human organism is well known,† and is illustrated by the extreme division which odoriferous substances must undergo, and by their effects on the susceptible—causing at times fainting and convulsions.‡ Experiment, *did we only know where to make it,*

* Billings' Principles of Medicine.

† See a valuable article on the minute division of matter, by Dr. Joslin, in Silliman's Journal of Science, Jan. 1847. Also Principles of Homœopathy, by Dr. Joslin.

‡ Dr. Hooker says (Med. Del. p. 66) that such analogies "do not touch the point at issue unless it is proved that an infinitesimal amount of any substance, musk for example, can be made to emit a stronger odor than a large quantity of it." This appears to me to be a mistake. All that it is necessary to prove is, that an infinitesimal amount of the substance will produce more effect on one person under one set of circumstances than a large quantity would on a different person under very different circumstances.

might lead us to remedial doses which, compared with our present ones, might be termed infinitesimal. Some criterion of the degree of probability would be found in the sensibility of the system, or of parts of it. If we knew that in any given morbid condition there was complete insensibility to other agents, it would be improbable that therein the system would be affected by very minute doses of medicine. But if we knew that it was acutely susceptible to some impressions so slight as ordinarily to have no effect, it would not be unreasonable to infer that there might be medicines which would have effects though quite inappreciable in other states of the system. I see not how the conclusion can be avoided, that we know nothing of medicines which, previous to experiment with infinitesimal doses, shows it to be incredible that they may affect remedially the human organism.

Or of the vital functions.

Is it not equally evident that we know nothing of the human system—its healthy or its morbid functions which renders this supposition unreasonable?

In health.

Many individuals apparently in good health, are exceedingly susceptible to certain medicines even in very minute quantities. A breath of air in which was floating an "inappreciable" amount of powdered ipecac has repeatedly brought on an attack of asthma. A millionth of a grain of the exhalations of our "poison sumach" must be sufficient to cause a severe erysipelatous inflammation, as some are thus affected by merely riding near it. These, and other cases of "idiosyncrasy," are as improbable, previous to their being established by evidence, as the assertions of the homœopathists. Nay, they are more so: for we know from repeated experiments, that these substances do not ordinarily produce these effects, but we

do not know from actual trial, anything which renders the effects of similar doses on an appropriate patient improbable.

In disease.

Again, we know that disease alters the susceptibility of the system to medicines as well as to other agents. A particle of food, a spoonful of water may disturb an irritable stomach. A touch, a breath of air, a ray of light may wound an excited nerve. A weak solution of borax will sometimes cure a diseased mucus membrane, though it, or a larger quantity, produce no appreciable effect on a healthy one. A diseased organ is in a state of unnatural excitement, and unusually susceptible to all external agents: especially is this true of agents which have a tendency to affect that very organ. If infinitesimal amounts of matter ever affect the system in health, it would be probable, rather than otherwise, they might often do so in disease. The increase in our knowledge of medicines and of the human system, has been constantly introducing doses hitherto unused. If a *new* field of experiment were opened to us, it would not be absurd to enter it because we were told that it would lead us to far smaller doses than those we have heretofore used. Should such power exist, we should never know it except from experiment. Experiment might prove it. It would be an addition to our present knowledge, not contrary to it. No theoretical considerations, certainly, can justify us in denying its credibility.

It is therefore important to observe, that these claims are confined to a field almost unexplored except by one party in this discussion. The homœopathists find their indications for the use of a remedy in its effects upon a healthy person, and prescribe it for symptoms similar to those which itself would tend to produce. The profession at large, have never considered this a reasonable

field of enquiry. It is, for example, one of Pereira's *objections* to homœopathy, that its medicines would only aggravate *the diseases for which they are given*.^{*} This impression has prevented adequate experiments on the effects of medicines, for such symptoms as these medicines are known to produce. That the objection is not well founded, might be suspected from the well known principle, that the action of medicines is different, often opposite, in large and in small doses, and also when other circumstances vary. The opposite effects of large and small doses of opium, the efficacy of small doses of some of the cathartics in diarrhea, the fact that turpentine in large doses causes and in small doses cures inflammation of the bowels,[†] that camphor, cubebs, &c. cause and cure irritation of the urinary organs, are familiar illustrations of this principle. Whether effects might not be obtained from still smaller doses, (of turpentine, for instance,) provided we select from the cases, in which it would be serviceable, such as have other symptoms resembling those produced by it, is a fair subject for experiment. If we have been in the habit of prescribing a drug for a certain disease, experience may have taught us that it was useless to give it in very small doses. But this experience cannot decide the question whether this drug may influence a different, much less an opposite set of symptoms. Nor does it make it unreasonable to suppose that therein the system may be far more susceptible to it, and consequently that far smaller doses will have effect. *How far* the division of the dose might be carried it is not necessary to decide. The great susceptibility of the human system to occasional irritants, shows that the limit might not be easily found. But it will be time enough to enquire whether effects can be

^{*} Materia Medica. Philadelphia, 1843. Vol. 1st, page 153.

[†] Eberle's Practice of Medicine.

obtained from *decillionth* dilutions, after we shall have settled the question in regard to those which are less remote from undisputed ground.

Other objections.

The considerations drawn from analogy include, moreover, certain objections to infinitesimal doses which are thought to be insurmountable, and hence to furnish a conclusive argument against them. So far as I can see, these objections may be reduced to the following heads:

1st. That these effects are incredible, because the same medicines may be given in ordinary doses without benefit for the very diseases for which they are administered homœopathically.

2d. That homœopathic medicines may be taken without producing any effect on the system, even in large quantities and habitually.

3d. That we cannot explain or understand the *modus operandi* of these doses.

4th. That the admission of the efficacy of these doses involves certain disastrous consequences to "scientific medicine."

In regard to the first of these objections, I would observe, it is not unreasonable to suppose that the small dose may be more efficacious than a large one would be, for—1st, the latter may produce effects which counteract the remedial effects of the small one. The "tonic" powers imputed to aloes or rhubarb,* the "alterative" effects of calomel or bloodroot, may be lost if the dose is too much increased. Vomiting, sweating, or any disturbing result may interfere with the remedial action. 2d. It is possible that the minute subdivision of matter may develop properties heretofore latent. The chemist assures us that this is true of its chemical properties, the homœopathist that it is true of its medical. It

* Pereira's Mat. Med. on these articles.

appears to me no more rational to reject one statement without examination than it would be the other. Is it wisdom to believe that there is latent heat in a drop of water or a lump of ice, but folly to believe that a piece of silex may have latent medical properties? Previous to experiments, there would appear to be as strong a presumption against the statement of the chemist that a piece of charcoal is identical, in its chemical composition, with the diamond, as there is against that of the homœopathist, that this charcoal or diamond, if subdivided so minutely as to be absorbable by the minutest capillary of the system, may have valuable remedial properties. Where such a subdivision exists in certain natural preparations, there is found to be an activity far greater than their chemical analysis or physiological effects would lead us to expect. The cod liver oil contains but a trace of iodine and bromine, and therefore somewhat resembles the preparations of the homœopathist. Some of the most highly prized mineral waters contain so little "active medicine," that their effects cannot be accounted for on any commonly received principle. 2d. The curative effects imputed to these remedies may be prevented, when given allopathically, through neglect of certain cautions, which those who advocate these doses consider quite important. A mode of preparing medicines, than which none has ever been devised apparently so well calculated to develop their properties, the administration of the remedies uncombined with one another, and the avoidance on the part of the patient of anything which might be supposed to interfere with the remedial action; these peculiarities are *claimed* to afford a reasonable answer to this objection to our doses.* 4th. The objection makes an assumption which may be unfounded. An acquaintance with the homœopathic

* See Dr. Joslin's work, already referred to.

principle, and a comparison with it of the effects of remedies may show that, under the ordinary treatment, cures do result in accordance with it more frequently than those suppose who do not attend to it. Homœopaths assert this and urge an investigation of the subject. It may be found that many orthodox cures are due to the homœopathicity of the remedy.

Another objection to these doses is frequently urged, viz: that homœopathic medicines may be taken in much larger quantities without producing any perceptible effect. To this I reply, 1st. No one can deny that medicines may produce curative effects in small doses, though in large ones there may be no such disturbance as would account for their remedial properties. The effects of sarsaparilla and other remedies popular with those who urge the objection, sufficiently illustrate this principle. 2d. Infinitesimal doses are not claimed in this essay to have any effects, except where the precise indications for them exist, though I think it could be shown that they do affect the healthy, when sufficient care is used in the experiment. 3d. In an occasional occurrence of these indications, the remedial effects might be lost by the very excess of dose, and by the neglect of indispensable cautions. 4th. Occasional results which might occur, might not be recognized by those who will not admit their possibility, but would be referred to accident or anything else rather than the dose. 5th. We can draw but uncertain inference as to the remedial effects of a medicine by the habitual or careless use of any article. Should we decide as to the remedial powers of tobacco, opium, or even arsenic, from the effects of these agents on those who are habitually or occasionally under their influence, we might be led into serious error. But, 6th. This objection and the previous one, take for granted certain questions, in regard to which those who

urge the objections are at issue with homœopathists. The latter assert that aggravations of disease and its symptoms are occasionally the consequence of their doses, and frequently result from medicines if used in larger doses when they happen to be homœopathic to the symptoms. That in the nearest approach which the objectors ever make to infinitesimal doses, these aggravations are conceded, being urged as a valid objection to the remedy, while they are merely so to the doses in which they are given. Instances may be selected almost at random. One physician (Dr. Waller) recommends a medicine very highly for some disease, (for example, belladonna for whooping cough,) another (Dr. Theophilus Thompson) objects, because in some cases where it was given, "he had found that the poisonous rather than the curative effects of the remedy developed themselves, even though the doses administered were remarkably small."* The accidents which have occurred with ordinary doses of tincture of aconite, have led some to reject it altogether as being a dangerous remedy. For a similar reason medical men have often been led to avoid testing the value of remedies in the cases in which we use them, because they "would only increase the original disease." The two objections are inconsistent, and thus neutralize each other. It is begging the question to conclude on *a priori* grounds, that there cannot be a dose so small as to avoid the dangerous effects of the remedy and yet large enough to secure its beneficial action.

The objection urged against these doses, that we cannot understand how the system can be affected by them, is hardly relevant to the point at issue. The question is as to the amount of evidence, and those who reject more direct presumptions would not be likely to listen to the

* London Lancet, Jan. 1845.

explanations we offer. Homœopathists suppose, in common with others, that most symptoms are but indications of the efforts of nature to remove disease, and hence that it is the duty of the physician to act in accordance with them: that where nature needs assistance, perhaps these remedies excite the *reaction* of the affected organ by giving it a slight impulse, analagous to that which the disease has given. This is offered as a possible explanation; still others are suggested which are worthy of the attention of honest enquirers after truth. That the objection is not a fatal one, can be seen by any one who will undertake to give us an explanation of the action of sarsaparilla. If, as Pereira says, this article “has little taste and no smell,”—if “by the ordinary use of it, it produces but very slight if any obvious effects,”—if, moreover, “in the hands of some, it has failed to relieve or cure diseases in which others have found it effectual,” what reason can be given why British and American surgeons should admit its therapeutic power and deny that of homœopathic remedies.

The objections drawn from the *consequences* of admitting these claims, so far as they bear on the question at issue, evidently belong to a different part of the investigation. Such objections, however, are but remotely relevant to this question. Our opinions as to the causes and consequences of any general truths are very apt to mislead us. The truth or falsity of disputed facts may often be reached with entire certainty, where all such speculations in regard to them may be altogether delusive.

Such objections as those implied in Dr. Bostock's excuse for not examining homœopathy, (quoted page 9,) too plainly beg the question to require further examination. Instead of knowing the properties of the living body under “all” circumstances, it is plain that, at least

under one set of circumstances, he had never investigated them and therefore did not know them.

These considerations, without the necessity of proceeding further, appear to me to prove that the *second* position in our trilemma is untenable; in other words, that there is no conclusive presumption against these doses—nay, that there are fair analogies in their favor. If this is so, the question is to be referred to the evidence.

First: that drawn from the circumstances amid which the doses present themselves.

Second, and chiefly: to our own experiments, and the testimony of others.

But if this inference is not correct, if it be true that there is nothing in the ascertained phenomena of nature which is analagous to the activity imputed to infinitesimal doses, and that of all the known properties of medicines, not one resembles those under consideration, a supposition than which nothing appears to me more extravagant, even then we would undertake to prove their efficacy, and claim that the evidence is so full, the presumptions so fair, and the testimony so conclusive, that no man with it fairly before him could reasonably reject the conclusion which we urge upon him.

INDIRECT EVIDENCE.

If it is possible that infinitesimal doses of medicine may act remedially, we are prepared to examine the probabilities in regard to it. It comes before us a question of fact, to be determined by the evidence. Although, from the nature of the subject, demonstration is unattainable, I hope to show that the preponderance of proof is strongly in their favor,—evidence of precisely the character which, in the ordinary concerns of life, forms the belief and determines the conduct.

Presumptions in favor of Homœopathy.

In the first place, a slight examination of the homœopathic system would seem to furnish indirect evidence that there must be some truth connected with it. But it can safely be assumed, that there can be no truth in any part of the system if not one of its doses can have effects—in other words, if an infinitesimal dose can not be remedial.

The very remoteness of its claims from the commonly received views, affords, I think, some presumption that its medicines must sometimes have effects. An assertion which widely differs from what we have hitherto believed, is much more narrowly scanned than one which nearly coincides with it. It is therefore to be expected, that the attention of every one would be awakened to the results of this treatment. If some mysterious process were gone through, which might at least affect the imagination; if, by some disturbing treatment, the patient might be persuaded that the relief which he felt after its effects were over, was produced by it; if some electric shock, or even an unpleasant taste was communicated, there might be room for doubt, but it is almost incredible that patients should be cured, or persuade themselves

they were cured, by absolutely no treatment, and that too, under circumstances calculated to awaken their suspicions.

It is not a system of quackery.

This system has not the usual marks of imposture. It does not claim to cure by secret or patented nostrums, but by means which any one can make trial of. Hitherto it has been thought that it was only necessary that an "empirical remedy" should be disclosed, to insure it the fate it merited.* Homœopathists invite others to acquaint themselves with their remedies, with an assurance that if they use them with proper care they will succeed with them.

Homœopathy is not founded in ignorance and dependant for its success on the ignorance of its dupes. "Hahnemann was undoubtedly a man of genius and a scholar; a man of indefatigable industry and undaunted energy."† His system embraces, where justice is done to it, the entire range of the medical sciences. Every fact, in each of them, finds its proper place in modifying its principles and its treatment. Its patrons are, "for the most part, the refined, the learned and the wealthy."‡

To appreciate duly, however, the presumptions which the homœopathic system furnishes in regard to infinitesimal doses, we must examine more particularly its characteristic features.

State of the controversy.

The history of the controversies on this subject shows that it is necessary to restrict such enquiries within definite limits.

Homœopathists enunciate certain principles which they claim to be based—some of them on facts, others to be deductions therefrom by processes of reasoning more

* Blane's Medical Logic, Section viii.

† Forbes' Review of Homœopathy. Brit. and For. Med. and Surg. Review for 1846.

‡ Hooker's Medical Delusions, pp. 54 and 79.

or less direct. Most physicians indiscriminately refuse to admit these claims, on the ground that they are opposed to established facts and principles, which they think furnish so strong a presumption against homœopathy as to justify them in refusing to examine the facts alleged in their favor. Thus the two parties do not come to an issue, and the controversy has been mostly a wearisome and too often an irritating attempt to establish and refute certain positions by argument. The difficulty has been increased to endless confusion by the constant introduction of collateral issues.

It is necessary to select some one question which may for the time be the rallying point in the discussion, and to which it may be confined. Such an issue is to be found in the question, "Have these doses the efficacy imputed to them?" and no argument is worth taking into account which has not a palpable bearing on the naked question, whether infinitesimal doses produce remedial effects.

I shall not undertake, therefore, to defend the leading dogmas of homœopathy. These may be true or may be false, without determining this precise question. Those who feel inclined to make an assault upon other parts of this system will probably find some of its defenders ready to meet them on any ground they may choose for the attack. At present I shall merely try to show, that there is enough in some of its principles, which is demonstrable and conceded, to furnish a presumption in favor of its doses, and therefore (especially if analogy leads to no conclusive presumption against them) that medical men have no valid excuse for not bringing this question to the test, upon which homœopathists have always insisted.

Peculiarities of Homœopathy which bear upon the question.

Several peculiarities of homœopathy appear to me to afford strong presumptions in favor of its doses.

A new method of studying the Materia Medica.

First.—Its physicians believe they have a method of learning the properties of medicines by a deductive process. It had heretofore been taught, not merely “that all attempts to ascertain the properties of the articles of the materia medica from hypothetical principles were of no effect,” but “that the only method which promised any success, was to collect from the best sources of information the most authentic facts, and afterwards to arrange the facts thus obtained in a scientific form.”* While this was the prevailing doctrine, the idea was started, or revived, that the true way of learning the properties of medicines was by observing their effects on healthy persons. “With a firmness of purpose and spirit of perseverance that scarcely admits a parallel, Hahnemann and some of his followers submitted an immense number of the most active articles of the materia medica to the test of experience.” “In conducting these experiments, the most patient and unwearied attentions have been bestowed by them, not merely in guarding scrupulously against all sources of contingent influence, but likewise in noticing with the greatest exactitude the multifarious changes produced in the several functions of the system.”† By such a process, thus conducted, may be elicited the pathogenetic effects of a newly discovered substance, while those of our old remedies may be confirmed or corrected. This method makes available all that is positively known of the medicine—all that accident, as in cases of poisoning, may make known of its effects.

Is there not the same presumption that this precise study will lead to useful discoveries as exists in any other case where facts are gathered and studied? Or is

* Bostock's *Materia Medica*, in *Edin. Encyc.* p. 397.

† Prof. Edw'd Geddings. *Amer. Jour. of Med. Sciences*, 1830.

it supposable that those who pursue these investigations should themselves reject whatever useful result they might lead to by using their medicines in doses altogether inert? Dr. Hooker, as do others, objects that the results thus obtained are often trifling.* This objection is unworthy of the student of nature. Would he deem it a good rule for the chemist, the meteorologist, or the astronomer to note no results but those that are *important*. So thought once the disciples of other natural sciences. Now the opinion is confined to one of them alone.

Whether this claim of the homœopathists is well founded or not, and whether the results obtained from it can be made available in the cure of disease, by means of that great law which is the basis of homœopathic therapeutics, it is not necessary to determine, but there are some things connected with it that are not at all doubtful.

If it were once acknowledged to be true, it would be thought to be reasonable and important. In any other natural science, some such process is considered indispensable to its progress. If a newly discovered substance is presented to the naturalist, he immediately commences examining it, and without delay can refer it to its proper place. Its properties, so far as they relate to his science, can be to a great extent detailed, and its analogies determined. Whether it be a new planet, or a new flower, a gas, a mineral, or an ore, observation and experiment are at once resorted to, and the inductive process requires but a short period to furnish additional proof of the wisdom and beneficence of the Creator. But if a new medicine is discovered, how different is the process. Used by the vulgar or the "quack," until its merits compel the profession to adopt it in spite of their prejudices, it at last lends lustre to the name of him who is so fortunate as to

* Med. Delusions.

take the right moment to introduce it to its new friends. The peasant, the monk and the Indian have each been important contributors to the cause of science. "Fanciful analogies in the form and color of the remedy," or at best, experiments on brutes, have helped make up the *materia medica* of those who deride the experiments of homœopathists. Pereira says of *nux vomica* it is "one of the few remedies the discovery of which is not the effect of mere chance."* Even the development of medical properties which cases of accidental poisoning occasionally lead to, has not enabled the profession to see that herein is a process which might be used deductively. The roadside, the meadow and the prairie are filled with "weeds" from which the homœopathists are claiming that their *materia medica* is constantly being enriched. The mineral and even the animal kingdom, are continually furnishing us valuable material which we are keeping in store for our elder sister when she shall be induced to own us, and which, we assure her, is worthy to rank with the discoveries she has made in anatomy, physiology and pathology.

This method of studying the *materia medica* is evidently looked upon with increasing favor by the profession. Some† prominently advise experiments on the healthy, who look without interest upon the results of like experiments which have been conducted with the greatest care for more than half a century. It would be well if they would undertake to furnish so faithful a pathogenesis of at least a single drug as to prove by the contrast that that which the homœopathists offer is indeed "an incongruous and ridiculous farrago."‡ A short time since an allopathic medical society at Vienna

* *Mat. Med.* Vol. 2, page 356.

† *Prof. G. B. Wood's Introduct. Lecture.* Phil. 1840, page 27.

‡ *Dr. Hooker.*

adopted the method of investigating the *materia medica* which we have now been considering. Their first published report, so far as it goes, corroborates the accuracy of homœopathic experiments made many years since.* One of their members, however, admits that it is not very obvious "what practical medicine can gain by these observations." Perhaps he may yet find a rule whereby, in his own language, "the physiological proving of the remedy may be harmonized with its employment at the sick bed."†

It has made discoveries.

A *second* peculiarity of the homœopathic system, is the claim that this method of observing the effects of medicines has led to valuable discoveries in regard to their specific properties.

In the use of remedies.

1st. It offers new indications whereby the use of old remedies may be guided. However highly he may estimate those indications which now lead him to the appropriate remedy, no candid physician will deny that they are often imperfect, sometimes contradictory, and dependant on no general law or fixed principle. Whether the claim of the homœopathist, to have more precise distinctions, be well founded or not, the very existence of the claim furnishes a presumption in favor of their doses. It affords great facility in testing the efficacy imputed to them. When Pereira recommends belladonna in whooping cough, &c., ipecacuanha in hæmoptysis, arsenic in fevers and periodical diseases, or nux vomica in dyspepsia, he gives us but few indications to guide us to the application of either to appropriate cases. It is not strange that their proper value in these affections should be undetermined. But when the homœopathist

* British Journal of Homœopathy Vol. 6, page 265.

† Page 277.

offers his doses of the same remedies, in some forms of these diseases, he accompanies his recommendation with far more precise distinctions whereby its truth may be verified.

Of many new specifics.

2d. The homœopathic system proffers to the profession many new *specifics*. I need not say that the very offer excites at once, a strong prejudice. But we do not mean by the term specific, either prophylactics, as citric acid for scurvy—though at least one such is very extensively accepted—viz: belladonna for scarlatina, nor is it meant by specific, a remedy always appropriate to a single disease, as mercury, cinchona and sulphur are often thought to be for certain affections. Such use of remedies is utterly irreconcilable with homœopathy. But the claim is that, with proper attention to the indications, these remedies may be expected to cure certain forms of disease pointed out by *definite groups of symptoms*. In this sense, though to a far less degree, many of the most valued “alteratives” are specifics, and are spoken of as such by the best writers. The search after specifics (in this sense) has always occupied a large share of the attention of the profession. Progress in therapeutics must chiefly be made here. Prof. Alison in anticipating the sources of future improvement in medicine, ranks the discovery of specifics as one of the most important means whereby the science is to be advanced.* How they are to be found, he does not tell us. The search has hitherto been but poorly successful except in one direction. M. Marchall, one of the “Faculty of Medicine,” at Paris, had the candor to say at the public examination of the students, in July 1847, that with regard to specifics and their action, all that we know we owe to the works of homœopathists. In those of Physicians

* Cyclop. of Pract. Med. Vol. 3d, page 270.

commonly called legitimate, from Hippocrates to our own time, we find absolutely nothing.*

It has anticipated some "discoveries."

3d. Again, many of the discoveries in regard to the action of medicines announced as new in late medical journals, were already familiar to homœopathists who were indebted for them to their trials on the healthy. The experiments and discoveries on aconite of Dr. Alexander Fleming† had been long rejected with disdain by those who are too orthodox to doubt their own infallibility. When once baptised into the true faith, they are found to have sufficient merit to entitle the propounder to a gold medal from the Edinburgh College of Surgeons. Will it be said that, though the homœopathists may have anticipated the discovery of the therapeutic power, Dr. Fleming first taught in what doses to make it available, that by some accident they stumbled on the discovery, but had not sagacity enough either to learn how to use it or to know whether they secured its effects or not? It is at least a curious coincidence that medicines like aconite, arnica, belladonna, &c. should now be used in cases for which homœopathists have for many years recommended them, while the same medicines have been in the hands of physicians for centuries, and these most important uses never thought of till now, and that no allusion to these properties should be found in standard works on the materia medica published within the last half dozen years.

A law of therapeutics.

A *third* peculiarity of the homœopathic system is the claim to be able to refer the specific properties of medicines to one general principle. Homœopathists believe that medicines tend to cure diseases and to remove

* Brit. Journal of Homœopathy. Vol. 6, page 124.

† Lond. and Edin. Monthly Jour. of Med. Sciences, July, 1845.

symptoms similar to those which the same medicines would tend to produce. It is sufficiently accurate for illustration to say, that to cure diarrhea they would use small doses of cathartics rather than astringents; like doses of emetics to *check* vomiting, &c. The aphorism *similia similibus curantur*, purports to express the relation between the action of a medicine on the healthy body and its specific effects in disease. If true, it furnishes an explanation of the action of medicines, and a rule of art which guides the practice. Therapeutics is thus made almost independent of the diverse speculations on the ultimate nature of disease, and on the mode of action of each separate remedy, both of which are indispensable under all other treatment except that which is merely empirical. Guided by the law of similars, the symptoms of the patient can be compared with those produced by the medicine, and that selected which furnishes the most perfect resemblance.*

I am not about to take it for granted that this principle is established—so far as to entitle it to become the basis of therapeutics—nor is this a suitable occasion either to exhibit the proofs of it, or to educe the important conclusions thence arising.

Which is true to some extent.

I would simply urge: 1st. It is evident that it is true to a certain extent. In innumerable cases it is found that there is a correspondence between its results and the properties of medicines long recognized. To say the least, a simple and beautiful hypothesis is offered whereby anomalies are accounted for, and neglected facts classified.† The stimulant effects of small doses of opium would be suspected from the narcotic effects of

* See a valuable paper on this subject, by Dr. Scott, of Glasgow, in the *Homœopathic Journals* for 1848.

† Fletcher's *Elements of General Pathology*.

large ones. Dr. Chapman need no longer express surprise that a remedy so efficacious in asthma as ipecac is found to be, should cause paroxysms so similar,* nor Dr. Graves his wonder, that in accordance with the homœopathic principle, sulphur should prove valuable in some cases of tympanites.† This list might be indefinitely prolonged. Standard works on therapeutics abound in instances where the excessive effects of drugs can hardly be distinguished from similar symptoms which they are known to cause.‡ Indeed, most of the so called alteratives are found to approach more or less closely to homœopathicity in their action. Even those who utterly deny the efficacy of infinitesimal doses, are constrained to admit that this theory is true within certain limits. Dr. Oliver Wendall Holmes tells us that “there are well ascertained facts, known from the earliest periods of medicine, showing that under certain circumstances the very medicine which, from its known effects, one would expect to aggravate the disease, may contribute to its relief.”§

And is worthy of study.

2d. The extent to which this law is applicable is a fair subject of enquiry. In other sciences “any hypothesis which possesses a sufficient degree of plausibility to account for a number of facts helps us to digest those facts in proper order, to bring new ones to light, and to make *experimenta crucis* for the sake of future enquirers.” As such merely, it would be worthy of the attention and study of physicians. But it may be more. Before the time of Hahnemann it had never been investigated. Since he published his essays, the mass of the profession

* *Materia Medica on Ipecacuanha.*

† *Clinical Lectures*, 2d Edition, Phil. page 233.

‡ For numerous instances see *Homœopathic Journals*, and especially the writings of Hahnemann.

§ *Homœopathy and Kindred Delusions*, Boston, 1842.

have avoided rather than sought experiments in regard to it. Had they been disposed to experiment on the subject, they would have been unable to do so satisfactorily from not having before them the effects of medicines unmodified by disease. Pereira tells us that experiments on the healthy alone will furnish us with the pure effects of drugs.* Those only, therefore, who had used this method of studying the *materia medica*, were prepared to estimate aright the extent to which the law of similars is true. It may be found that its applicability is much more extensive than has heretofore been conjectured. It is, as is well known, the characteristic feature of homœopathy. To it all other questions are merely subordinate. They might each and all be disproved without finally deciding the merits of this; while whatever independent evidence they severally possess, goes to swell that which, to our minds, furnishes almost demonstration of its truth. Some such therapeutic law would seem necessary to any adequate improvement in practical medicine. Without it "the progress of therapeutics during all the centuries which have elapsed since the days of Hippocrates, has been less than that achieved in the elementary sciences of medicine during the last fifty years."† A belief in its truth as a general principle has prompted to the most laborious investigations amid difficulties and discouragements almost without a parallel, and has given what seems to be vitality to a system which, if this law be false, has at least nothing else to sustain it.

And affords a presumption in favor of the doses.

3d. If this hypothesis be admitted but to a small extent, it removes much of the *a priori* objection to the extremely small dose. If, as we have already seen to

* *Materia Medica*, Vol. 1, page 119.

† Dr. Forbes.

be true, the human system is ever susceptible to extremely minute amounts of matter, it might be expected to be so in the circumstances which this principle implies,—itself, morbidly excited by disease, and the excitement of the very character as that which the medicine would tend to cause or increase, no circumstances can be imagined, previous to positive evidence, wherein an *idiosyncrasy* might be expected, if not here. If, in what seems to be perfect health, the system is ever susceptible to agencies vastly less than those which are ordinarily necessary to produce effects; and if in disease it is even more impressible by appropriate stimuli—just so far as the law, *similia similibus curantur*, is true, so far may infinitesimal doses be expected to cure. This presumption becomes certainty, if we shall find that it is confirmed by experience.

State of the Argument.

Such are some of the presumptions which prepare us to examine the direct evidence in regard to infinitesimal doses. The system of medicine which furnishes them ought to receive a respectful attention. It has still other, and to us no less cogent claims on that attention, but as they do not so directly bear on our subject, their place in this discussion is comparatively unimportant. Their truth or falsity has but little to do with the precise question before us. They may be false—wanting so much as a show of reason—without materially invalidating the evidence which experiment would furnish with regard to infinitesimal doses. Whether they are so or not is a matter of opinion, which can best be decided after the present question is settled. The same may be said of a variety of presumptions, for and against homœopathy, which are usually made prominent in discussions as to its merits. Whatever light the history of medicine and the ascertained principles of its elementary sciences

may throw on the questions at issue, between the two systems, however plausible may be the various considerations drawn from analogy, and from the peculiar dogmas of homœopathy, their value, in the present issue, is limited almost entirely to the question, are the effects imputed to these doses *credible*? The ultimate decision of the question must be referred to the most direct evidence which the subject admits of.

This brings us to the question, whether there be any *testimony* in regard to infinitesimal doses. On other subjects testimony is the most direct evidence attainable, except the results of our own observation. On such subjects it is often surprising how a little plain testimony easily outweighs the most decided opinions, and the strongest probabilities. Even if the testimony should prove to be defective, or scanty, or conflicting, these defects are within the reach of our faculties, and we can estimate what weight is due to it, and then, perhaps, procure or furnish better. But the deciding disputed matter of fact questions by arguments, however logical, or by authority, however weighty, has far too often perpetuated error.

DIRECT EVIDENCE.

The subject a suitable one for testimony.

In examining the direct evidence in regard to infinitesimal doses, it should be observed that the subject is a suitable one for testimony. The facts asserted can be appreciated by the senses of the witness. It is perceived that an individual has a certain symptom, *e. g.* hæmorrhage, ulceration of some exposed surface, fever. The physician administers something, and the symptom (or group of symptoms) after a time, ceases to exist. What such a statement is worth, is not now the question. Whether it furnishes any evidence will depend on the way in which the blanks in the formula are filled. It

may be that the ulceration has continued for months, the hæmorrhage for hours, and that the one ceases in a few days, the other in a few minutes. It is possible for him to know it if it be so. He may indeed be mistaken in attributing the change to the supposed remedy, and this liability limits the value of the testimony. We must judge on other grounds as to the amount of evidence it furnishes. Should he undertake to say that it was the medicine that cured the disease, this would not be admissible, for he could not know it from the direct evidence of his senses. He can testify only to what he thus knows.

Not involved in a process of reasoning.

Moreover, the facts asserted by the witnesses may include, by a direct implication, the question at issue in this discussion. No prolonged process of reasoning is necessary to connect them. The witness may assert that dysentery, or any other disease, disappeared soon after the administration of an infinitesimal dose, or in connection with certain manipulations with metallic tractors, or after taking calomel, lobelia, or some other remedy. Surely now, if the inference is not inconsistent with some general fact or natural law, if there is not so strong an *a priori* presumption against it as to render it incredible, if it is a part of a system which supplies some independent evidence in its favor, the statement does furnish some evidence that the means used did occasion the change, that the "post hoc" was really a "propter hoc." However slight this presumption might be, it would be increased by a similar statement of the same or another witness. If then, the witnesses possess the qualifications necessary to make them credible, there might be enough such testimony accumulated to put the question beyond all reasonable doubt. Moral certainty might be reached that the doses were really the "physical cause," if it could be shown that in other ways the results could not

be fairly accounted for. Whether this can be done, it will be proper to determine after we shall have examined the nature of the testimony and the character of the witnesses. In doing so, it will be seen :

The testimony is precise and circumstantial.

First. That the statements of the homœopathists are as precise and circumstantial as it is possible for medical facts to be.

All the circumstances which can be enquired about are stated if required. Every thing which might aid the judgment in explaining the results is given or offered. The specifications are frequently so minute as to excite the derision of others. These details can be compared with unquestioned facts, and their real value estimated. It is possible that these circumstances may admit of no explanation except through the action of the doses. If this should prove to be the case, the minuteness of the circumstances detailed, is such as to admit of constant contradiction and detection, if the testimony is false. The importance of this precision may be seen by comparing it with the evidence, lately revived, in favor of certain exploded modes of treating disease, of which Perkinism is made most prominent.* I have not the means of examining the original evidence in favor of this system. Those, however, who now appeal to this evidence, do not give us a precise report of a single case thus treated. Of course we know not what evidence would be supplied by an ample number of such cases—much less what would be testified to by those who should treat all their cases with “metallic tractors.” Evidence free from such deficiencies, is abundantly furnished by homœopathists.

It is direct and positive.

Second. This testimony is direct and positive. Not

* See the works of Dr. Holmes and Dr. Hooker, already referred to.

indeed, that infinitesimal doses do produce effects, for the nature of the case does not permit this, though homœopathic physicians assert these effects as confidently as they could the remedial effects of medicines in any doses. They assert, however, that their own convictions are unhesitating, and as the basis of them testify to certain facts without conditions or limitations which so directly involve the efficacy of these doses as to convince most of those who observe the facts and listen to the testimony, that the conclusion is true. The evidence may be sufficient to exclude all reasonable doubt: that it should be so strong as inevitably to produce conviction, would of course be impossible.

This testimony is positive as to several particulars.

As to a certain amount of success.

1st. That there is a certain amount of success under homœopathic treatment. Reports of hospitals, dispensaries, and other public charities are given regularly. These establishments are open to the public, and any considerable inaccuracy may be detected. They are, as it is well known, watched with no little care, and in no friendly spirit. They include, one with another, probably every variety of disease which is ever made the subject of medical treatment. The reports vary in regard to the success attained: sometimes it would appear to be very striking, at others less so. On the whole, however, they assert that a greater success is attainable under this treatment than under any other which the attending physicians know about. These representations have induced others to try the same means, and they, with hardly an exception, testify to the same facts. Several thousand physicians in private practice now unite in testifying that in inflammations, fevers, diarrhœa, dysentery, rheumatism and most forms of acute and chronic disease, a success is attainable under

the administration of homœopathic remedies in infinitesimal doses, which they would not know how to obtain otherwise.

One of the most recent witnesses in favor of these doses is M. Tessier, physician of one of the hospitals annexed to the Hotel Dieu, at Paris. This gentleman has, it seems, been treating his patients homœopathically for some time past, and the result of his trials has been his own conversion to it, and an endeavor, by a publication of his cases of pneumonia and cholera, to interest others in the subject. He says that after studying the writings of Hahnemann and his disciples, "I proceeded to verify the efficacy of infinitesimal doses. I devoted six months to this verification at the bed-side of such acute and chronic patients as could not by any possibility be injured by the experiments I was conducting. The evidence that the doses acted was complete in a few days, but I continued the experiments on this point for the whole of six months, and it was only at the expiration of that time, that I sought to ascertain the therapeutic value of the new mode by applying it as rigorously as I knew how."

"I ventured, in the case of a patient" with pneumonia, "in whom I had produced the remission by bleeding, to substitute phosphorus for the tartar emetic usually administered. He recovered without accident, and I repeated the experiment several times with the same success." "These happy results, however, might very properly be attributed to the energetic antiphlogistic means pursued at the beginning of the disease, and I could only conclude from them, if I had done no good, I had at least done no harm. I then resolved gradually to diminish the number of bleedings, and to introduce the Hahnemannian method before the usual remission was produced, reserving always the right to resort to the or-

dinary treatment if the amendment did not appear with sufficient promptitude. I omitted one, two, three, four bleedings in the next patients that presented themselves, thus bringing the administration of the new remedies gradually nearer and nearer to the first onset of the malady: beginning with a dose of aconite, followed after an interval of twelve or twenty-four hours by bryonia, to which phosphorus succeeded. The less I bled the more grateful was the action of the infinitesimal doses to the suffering patient, until I decided to bleed no more, and employ the method of Hahnemann from the commencement. Aconite seemed to be of little service after having been administered a few hours. Bryonia appeared to be very energetic in its action, and phosphorus seemed to be useful in local inflammations, threatening to pass to suppuration."

"I cannot express the anxiety which attended these first experiments. Notwithstanding my express directions to resort at once to bleeding if the patients became worse, and in spite of repeated personal attendance at their bedsides, I could not escape the constant feeling that some catastrophe was about to happen. But nothing of the kind occurred. The patients who were first submitted to the treatment recovered without an exception, and the condition of others was rapidly improved. For two years I have had but a single death" (from pneumonia); "two others have died, but they entered the ward in the agonies of a pneumonia already suppurated, and if they figure in my statistics, they should not be taken into account in a discussion of my therapeutics. Since then I have employed the same method in many cases of pneumonia, and by degrees my fears have vanished."*

The report of his cases is full in its details, giving

* North American Hom. Journal. New York. Feb. 1851.—p. 103 and 109.

every particular which might aid us in judging as to the effects of his doses.*

There are now in operation several large homœopathic hospitals. They publish regular reports and invite examination. Among them none perhaps has excited so much attention as that of the Sisters of Charity at Vienna, under the charge of Dr. Fleischmann. His reports have claimed so much success, that Dr. Balfour of Edinburgh, went to Vienna apparently to detect their falsity. He wrote from that city in 1846, in the following words: "During the first appearance of cholera here, the practice of homœopathia was first introduced: and cholera when it came again renewed the favorable impulse previously given, as it was through Dr. Fleischmann's successful treatment of this disease, that the restrictive laws were removed, and homœopathists obtained leave to practice and dispense medicines in Austria. Since that time their number has increased more than three fold in Vienna and its provinces." He also says: "No young physician settling in Austria, excluding Government officers, can hope to make his bread unless at least prepared to treat homœopathically if requested."†

Dr. Fleischmann's reports may be found in the British and American journals of Homœopathy. Of that for 1845, Dr. Forbes says: "No candid physician will hesitate to acknowledge that the results there set forth would have been considered by him as satisfactory if they had occurred in his own practice." Of Dr. Fleischmann he says: "He is a regular, well educated physician, as capable of forming a true diagnosis as other practitioners, and he is considered by those who know him as a man of honor and respectability, and incapable of attesting a falsehood."‡

* For additional particulars see Am. Jour. of Homœopathy, May, 1851.

† British and Foreign Medical Review. 1846.

‡ Review of Homœopathy.

The Protestant half orphan asylum, New York city, has been "for the last five years under homœopathic treatment. For years previous its inmates were afflicted with Ophthalmia of the most virulent kind, and all the counteracting effects of allopathia, even to the inconvenient expense of keeping the little sufferers in the country, were made in vain. The malady gained strength until seventy-five of the children, about one-half of the whole number, had to lie prone on the floor to avoid the light, and extra nurses had to be employed to wipe their eyes! At this extremity homœopathy was called in to try her powers," and in about six weeks almost every eye was made sound.*

In this institution an opportunity has been afforded to demonstrate the superiority of homœopathic treatment in various other affections, acute and chronic.†

Homœopathic journals abound in instances where physicians, who held previously at least a respectable rank, announced their conversion to homœopathy, and assert as the reasons for the change, that they found this system afforded them the means of curing disease more effectually than the other. Testimony similar to that of M. Tessier is unlimited in amount.

Innumerable coincidences.

2d. This testimony is positive as to there occurring under homœopathic treatment, innumerable coincidences where symptoms, often of the most serious character, and frequently of long standing, disappeared almost immediately on the administration of certain remedies, which they assert were strictly in infinitesimal doses. A case may result fatally, and yet in its progress afford indubitable evidence of the action of the doses by the control which the remedies have over some of the symp-

* Report of Trustees of Homœopathic Dispensary for 1849.

† See Dr. Wright's report, Homœopathic Examiner, vol. 3, p. 347.

toms. These "accidents" occur, according to the testimony of the witnesses, with remarkable precision, and may be expected in certain groups of symptoms with as much uniformity as any similar results of medication with larger doses. They report thousands of cases treated in this way, and assert that they give all the particulars which might explain the results. In such groups of symptoms as the following, these coincidences occur:

1st. Hot, dry skin ; full, quick, frequent pulse, &c.

2d. Inflammation of throat and tonsils, shooting pains, scarlet redness of the surface.

3d. Violent vomiting of mucus, continuing after the throwing off the contents of an overloaded stomach.

4th. Diarrhœa, with great thirst, vomiting, prostration, burning pain.

5th. Continued fever, with disorder of stomach and liver, debilitating sweats, &c.

6th. Cough dry, fixed pains in chest, catching pains on inspiring.

7th. Violent pains in the bones, after excessive use of mercury, with languid nervous prostration.

Such a list might be extended without limit. Hundreds of witnesses assert that they have many times seen these groups of symptoms disappear on the administration of certain remedies, with a precision and promptness which would have surprised them under any treatment. They confirm each others' testimony by fixing upon the same remedy for each well-marked group of symptoms. On the supposition that they are all alike inert, one remedy would be no more likely to have effects than another.

Many of these diseases and symptoms are not known to have a constant tendency to sudden changes where nothing is done. They are known under other treatment to yield much better to some courses than to others.

Yet homœopathists assert in almost every instance, that these best methods are at least equalled by homœopathy in its power of furnishing "accidental" coincidences. They offer to prove it to any one who will examine their results in a friendly manner, and if he denies their conclusion, they offer to put into his hands the means of making good his denial. Here are the remedies, and here are the indications for them—whether results follow or not, is simply a matter of fact.

To give any adequate view of homœopathic evidence by narrating cases, would require volumes. To judge satisfactorily of the weight due to them, the original reports must be examined. I shall therefore content myself with referring to sources easily accessible, where such reports can be found; merely giving two cases, one acute and one chronic, to show what evidence is so unceremoniously neglected or so scornfully denied.

Case of croup. The patient, a girl of thirteen, had been two days suffering from catarrh and sore throat, for which the usual homœopathic medicines were administered, when from reckless exposure of herself during rain, she became more seriously ill. About 7 in the evening she retired to bed and remained quiet, and as was supposed, asleep. But between 9 and 10 one of the family passing her door, was startled by hearing a rough, hoarse, barking cough. Aconite and spongia were administered, each of them in the 5th dilution. There seemed to be a rapid progression of the disease, the cough becoming more deep and hoarse, the flush of the face deepening, first into crimson, then into purple, and the contortions of the whole form showed the agony with which every breath was won.

Such was the condition of the patient at midnight when the physician arrived, too late it was feared, to be of service. His account is in the following words: "I

found the patient under a harsh, dry cough, the respiration stridulous, the expression of the countenance anxious, the cheeks puffed and livid, pulse frequent and small, extremities cold—great restlessness and jactitation. On looking into the throat I found the fauces highly inflamed and swollen. I dissolved six pellets of the kali bichro. of the 6th dilution in a tumbler half full of water, and administered a dessert spoonful of the solution. In ten minutes thereafter there were pauses in the before incessant cough—in thirty minutes the child had sunk into a quiet sleep, and the difficulty of respiration subsided.” The patient slept with little interruption for several hours, and it was not till nearly morning that the cough returned with any persistence. The medicine was repeated—half a spoonful of the water in which it had been dissolved being given—it gave immediate and enduring relief. The patient had no return of cough or hoarseness.*

Case 2d. “Miss B. W., aged fifty-two, of scrofulous diathesis, formerly much subject to bilious affections with sick headache: sixteen years since became partially paralytic in the left fore-arm and hand, which was then supposed to arise from enlarged glands about the neck from scrofula: fifteen years since, during a severe attack of what appeared to be sick headache, became apoplectic, the sequel of which was nearly a complete paralysis of the left hand and arm. For the last fifteen years, the arm has been useless, quite pulseless, nearly bloodless, numb, prickling, with occasional turns of severe pain of the whole arm, which, from their severity, were nearly insupportable—the limb flabby, diminished in size, so utterly destitute of strength and firmness, that in endeavoring to execute some trifling motion, the thumb, fingers, and sometimes even the wrist, would,

* Abridged from the American Journal of Homœopathy. Vol. 1st, p. 232.

as it were, fall out of joint. Disposition fretful, easily discouraged, and inclined to weep, mind weakened; general health in many respects delicate. Nervous, excitable, unhappy.

On the first of July I gave her *Rhus radicans*, three pellets of the third dilution—the same on the morning of the second. She passed a somewhat restless night, with much severe aching of the back of the neck, shoulders, instep and toes of the left foot, with severe tingling and prickling of the left arm and hand. Through the 2d, 3d, and 4th of July, the pains continued severe, and extended to the whole left arm and hand, which seemed swollen, and the veins of the left limb, which had rarely been susceptible to the sight or feel, became full, blue, hard, painful in the extreme. All this was attended with dizziness of the head, slight turns of faintness, great irritability and complaining, less than usual appetite, insecure feeling when walking, or unsteadiness of gait, free motions of the bowels, and abundant discharge of pale urine.

On the 5th the unusual symptoms began to abate, and in about three days, she was free from any considerable suffering, with a quite perceptible, yet rather feeble pulse, where for fifteen years, none had been detected even by physicians, veins remained filled, giving the limb nearly the appearance it had before being paralyzed.

There is still (August 1st) daily increase of power in the limb, with improvement of the general health, rarely feels slight pain in the limb, is more cheerful, less irritable, has more ambition and fortitude. She is yet resting from all medicines, but follows strict rules of diet. I design to repeat the medicine at some future period if improvement ceases short of perfect restoration of the diseased organ.”*

As a student of homœopathy, it was my privilege to

* American Journal of Homœopathy, Vol. 1, page 100.

sit by the side of the gentlemen who report these cases, in their attendance at the New York Homœopathic Dispensary, and thus form an opinion of their system from personal observation. As case after case came before them, I listened to the patients' own account of their sufferings, and often of the fruitless result of previous modes of treatment. I took notes of the symptoms and of the prescriptions. At their succeeding visits, I compared my record with the subsequent statements of the patients. I examined the symptoms and watched as carefully as I knew how, their progress. Such of the dispensary cases of one of the physicians, as were unable to leave their homes, I was permitted to visit with him, and I assert, that to the best of my judgment, no success which at an earlier period of my medical education I had witnessed in attendance upon Hospitals and other public charities, under the common treatment, would favorably compare with that which was thus obtained with infinitesimal doses: that coincidences as striking as those which I have alluded to, were so frequent as to be not the exception, but the rule—that similar results attended the prescriptions of other gentlemen connected with this institution, and that I saw nothing which led me to think there was deception in the diagnosis, or in the use of remedies—or made me suspect that the results were due, to any unusual extent, to mental influence, or to the unassisted restorative powers of the human organism.

Accordance of the coincidences with the law of similars:

3d. Homœopathists assert that the coincidences to which they testify, occur with a regularity proportioned to the correspondence between the symptoms of the patient, and the results of the homœopathic principle, or law of similars.

There is an infinite variety in this correspondence.

In some instances the similarity between the pathogenic effects of the remedies and the symptoms of the patient, is as exact as possible. In others, less so, and in some cases, no remedy very closely homœopathic can be found. There is also a great variety in our knowledge of the application of this principle. As on other subjects, we find constant occasion to extend and correct this knowledge. Some of us are but beginners in the practice, and are conscious of doing it but very imperfect justice. Moreover, our own failures to appreciate duly at one interview with our patient, symptoms which we learn more accurately at another, furnish us a test of the varying fitness of the several remedies. The witnesses assert that the coincidences to which they testify, correspond in frequency to the homœopathicity of the remedy. They assert that they not merely find a difference whether they give one remedy, than another, but also whether they give one preparation of the same remedy rather than another, and whether they take pains to exclude agencies which might interfere with its action, such, for example, as chemical antidotes.

To appreciate duly the force of this argument requires some knowledge of the homœopathic materia medica. Those only who have in mind the pathogenetic symptoms of a remedy, can adequately judge what would be the indications for it according to the homœopathic principles. A sufficient approach to this, however, can be made by any candid enquirer who will make a comparison between the prominent effects of these medicines on the healthy, as laid down in this materia medica, and the symptoms of the reported cases, or, if he cannot trust these, of those symptoms which himself shall detect in the patients of the homœopathic hospitals and dispensaries. Such a process can be conducted without any confidence either in the accuracy of the standard of

comparison, or in the efficacy of the remedies, or in the truth of the law by which they are selected. Such a comparison, carefully made, would furnish a conclusive demonstration either of the truth or of the falsity of these several claims.

To illustrate the force of this position by comparing the pathogenetic effects of remedies, as laid down in the homœopathic materia medica, with the curative effects of the same remedies when administered in infinitesimal doses, would be an easy matter. As, however, the fidelity of this pathogenesis might be questioned, I will compare the prominent symptoms produced by belladonna, as determined by the experiments of the allopathic society of Vienna, alluded to on page 31, with a case treated with this remedy in infinitesimal doses. I select the case almost at random; certainly the only limit to the number of similar cases, which might be furnished, would be the small number of remedies of which we possess an orthodox pathogenesis.

Case treated homœopathically. Symptoms caused by belladonna.

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| <p>"A man, aged 30, of strong constitution and choleric temperament, was, in consequence of taking cold, suddenly taken with chills, heat, anxiety and restlessness, boring and throbbing pains in the head, soreness and heat in the eyes. The attending physician found him in the following condition: he could not sleep a moment at night; complained of vertigo, particularly if he attempted to sit up, and often fell into the most violent delirium; he stormed and cried out, attempted to leave his bed, and resisted powerfully all attempts to keep him there. In his more lucid inter-</p> | <p>"At one time increased, at another, diminished pulse," "feverish symptoms."</p> <p>"Restless sleep, sleeplessness or torpor."</p> <p>"Vertigo, reeling as if intoxicated."</p> <p>"Confusion of intellect and hallucinations."</p> |
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vals, he complained of heaviness and confusion of head, of a sense of fulness, and horrible, fixed burning and aching, at times piercing pains in the same. His eyes looked wild, were rolled from side to side, were painful, reddened, and very sensitive to light; the pupils were contracted, and he saw bright sparks and flames before the eyes; he was very sharp of hearing, very sensitive to noise, had roaring and tinkling in his ears. His face was hot and red, and at times, covered with a sticky moisture; pulse was very quick, hard and spasmodic; skin was dry, hot, and red; respiration anxious, sobbing, at times interrupted. His voice was hoarse, and speech at times difficult; his lips very red, hot and dry, and the cavity of the mouth also; the tongue was red, and thinly coated with tough mucus; violent thirst, sensation of contraction in the throat, and frequent urging to swallow; entire loss of appetite; all food disgusted him; nausea and bilious, slimy vomiting, constipation, red, burning urine.

In this state of things, belladonna, one drop of the tenth dilution, was administered.—Twelve hours after there was decided improvement, the next day still more, and in 4 days the patient left his bed with no other aid from medication except the belladonna, and subsequently bryonia of the fifth dilution.”

[Homœopathic Examiner, for 1840, page 223, and Ruckert's Therapeutics, page 36.]

“Palpitations” of the “carotid and temporal arteries, with heat and redness of the face;” “congestion in the head.”—“Pressure, general and partial headache.”

“Humming, or ringing in the ears, sensitiveness of eyes, weak sight, diplopia.”

“Heat and redness of face.”

“Difficulty of moving tongue, impeded speech and deglutition;” “uncomfortable continued dryness of lips, of the buccal cavity and fauces; with desire for drinks, and only temporary relief by drinking; redness of the mucus membrane of the fauces, eructation; furred tongue, diminished appetite, disgust, nausea.”

[From the British Journal of Homœopathy, for April 1848.]

Several things are to be noted in regard to the above.

- 1st. The case was treated and the report was published several years before the society published their provings.
- 2d. The experimentors only furnish to us such symptoms as were produced on all, or almost all the "provers"; of course we learn no more from those who, being most susceptible to the action of belladonna, would have furnished some of the most characteristic and well-marked symptoms, than we do from those less susceptible. These might have been given, and some mode devised of pointing out the comparative frequency of their occurrence.
- 3d. The experiments were made with "full doses" only: whereas ours are made with those also which are gradually reduced in amount until the "provers" perceive no decisive effects. We find in the report the following words: "It should be remarked that the above phenomena" "bore no fixed relation to the increase of dose, for by the use of larger doses, symptoms which had formerly occurred lessened or vanished entirely, without others taking their place." It would seem to us, in these circumstances, to be very natural to diminish rather than increase the dose, and thus determine if doses still smaller did not lead to symptoms which those used did not elicit.
- 4th. The correspondence between the most marked symptoms of the patient and those produced by belladonna is so palpable, that no man, having the homœopathic law of similars in mind, would have a moment's hesitation in selecting belladonna from those proved and reported by the society, and we claim that the consequent recovery was a coincidence which has been repeated so often and occurs so constantly, as to be conclusive evidence of the action of infinitesimal doses.
- 5th. I think no one will refuse to admit that were he to administer belladonna to a patient with the symptoms recorded above, he would be careful to do so

in very small doses—that Pereira would find in it an illustration of his objection to these remedies, that “they would only increase the disease for which they were given.” Yet, for such symptoms, probably every homœopathic physician would be led at once to belladonna.

The reports to which I have referred on previous pages, furnish abundant additional examples of similar coincidences. If more are wanted, they may be found in reports of cases of pneumonia, acute bronchitis, croup, &c. by Prof. Henderson, of the University of Edinburgh,* and said by him to be all the cases of these diseases occurring in his practice since the publication of his “Inquiry into the Homœopathic Practice.”† If these reports are not satisfactory on these points, it would be interesting to see some medical reports which might serve us as a sample. Still more may be found in Dr. Wielobychi’s cases of puerperal convulsions,‡ Dr. Black’s cases and treatment of headaches,§ reports of cholera, dysentery, fevers, diseases of the kidneys, liver, &c. spread over the several journals of homœopathy, already referred to. These reports must either be rejected altogether, or admitted to prove the success of the treatment, and the occurrence of constant coincidences such as are claimed. They furnish an amount of evidence of which those have no conception who deny the conclusion which seems to us so plain. Their very existence is probably unknown to them: so that the effect produced by this evidence is not at all proportional to its amount. Probably, if the testimony in favor of homœopathy could be suddenly doubled, or reduced to half its present limits, either change would be entirely unappreciated by most allopathic physicians. In the present state of the controversy, what is wanted is not

* British Journal of Homœopathy, 1849 and 1850. † New York, 1846.

‡ Brit. Jour. of Hom., 1847. § Ib.

so much an increase of evidence, as the removal of those considerations which now prevent an examination of that which is already abundant.

Positive as to the doses being "infinitesimal."

4th. The witnesses assert, with great unanimity, that the only aid rendered to the restorative powers of nature, in most of these cases, arises from such doses as are the subject of this essay. True, it must be admitted, that now and then a physician practices according to the law of similars, with doses but little removed from those ordinarily used, and that a larger number use, in part or altogether, doses still smaller than those which homœopathists usually prescribe. But there is nothing necessarily conflicting in this. It only proves, as it seems to us, that while the homœopathic principle is all important, the dose is quite a subordinate consideration. Fortunately for our present purpose, however, we have witnesses enough without seeking them from either extreme. Such will be needed by our opponents, and to them we will leave them. The one class will be their most efficient means of proving homœopathists inconsistent and dishonest, and the other will be needed in the attempt to show that we impute palpable results to doses which imply *supernatural* power. The great majority of our witnesses assert that their results were obtained with doses so small as to be quite inappreciable to the senses; as to cause, usually, no disturbing effects, —several times smaller than the smallest doses of the same remedies directed in standard works on the *materia medica*: doses to which either no allusion is made in the common treatises on therapeutics, or, if they are noticed at all, are considered worthy only of derision and contempt.*

* Those who wish to learn more in regard to the mode of preparing and administering these remedies, are referred to the work of Dr. Joslin, alluded to already.

There are thus three distinct sets of phenomena to be accounted for. The cures of disease which take place under homœopathic treatment; the sudden changes in the state of the system, occurring in connection with the administration of infinitesimal doses, and the correspondence of most of these to the law of similars, which correspondence proves the reality of the law and the powers of the remedies. The progress of a single case may furnish evidence which would belong to one or two or all of these classes. In the report of cases, it is not possible, nor is it important, to keep these several statements always distinct. But in examining the testimony of homœopaths, justice to the subject requires that they should all be taken into consideration.

It should be observed, moreover, that while homœopaths do not claim that their treatment is uniformly successful, or that the coincidences to which we have referred may always be expected; while they confess that curable cases are sometimes uncured; that pain is at times unrelieved, and failures and disappointment will sometimes arise; that even with infinitesimal doses in their hands, they find the imperfections incidental to all human attainments: they do claim, and assert positively, that favorable results occur as often and with as much uniformity as is necessary to the argument. Their opponents themselves being judges, these claims are so strong as to appear extravagant. It is, accordingly, no uncommon thing, when statements in favor of homœopathy are made, and cases detailed, for the reply to be given, "this is claiming too much." A zealous friend of orthodox medicine goes so far as to say, that he knows of "no *nostrum* whose virtues are more absurdly or impudently vaunted" than these remedies are. If a greater success was claimed, or more certainty in the occurrence of these coincidences was asserted,

this objection would be still more plausibly made. It would, therefore, be inconsistent to claim that homœopathic testimony is *deficient* in these respects. If it is attempted to be invalidated at all, some other point must be selected for the attack.

It is intelligent.

Third. This testimony is intelligent. Most homœopathic physicians are “regularly” educated, and by a previous experience in a different system, acquire sufficient knowledge of the usual progress of disease and the ordinary effects of remedies. If it is remembered that this treatment is resorted to in cases the most violent, and of the most tediously chronic character, from the unconsciousness of infancy to the decrepitude of age, it surely cannot be believed, that men otherwise intelligent should be unable to distinguish whether the medicines they use have any effects. It is not incredible that disease should be cured through the influence of the imagination, or by nursing, or by the unaided powers of nature; but it is incredible, that educated men should persuade themselves that these agents, however adroitly used, are superior to all the means which the medical profession has accumulated for alleviating pain or prolonging life. But, if it really be otherwise, they might at least be supposed to have sagacity enough to give intelligent testimony. If they cannot appreciate the effects of remedies, they might testify as to the facts. The disappearance of pain, cough, vomiting, &c. contemporaneously with the administration of a remedy, only give us cases enough, may be decisive as to its effects, even if the witness does not know what the symptoms mean. Those who impute incompetence to homœopathic practitioners, are bound to show at least plausibly how this success is to be accounted for. If they are not intelligent, it is strange that they should so effectually

have gained the confidence of others, where the presumptions appear to be so much against them.

It is honest.

Fourth. This testimony is honest. It should be admitted to be so unless the contrary is proved, and the proof must be independent of its own improbability, for it would be arguing in a circle to infer the dishonesty of the witness from the incredibility of his statements, and then use this dishonesty to invalidate his testimony.

The presumptions are in favor of honesty: for this honesty is generally admitted, and has never been, so far as I can find, attempted to be disproved. The dishonesty of homœopathic practitioners is only occasionally insinuated, when, apparently, no other alternative can be found whereby to account for some of their statements.

This honesty is probable from the circumstances. Homœopathic practitioners exhibit a strong confidence in their own treatment. They assert this confidence, and often inspire it in others, where before there was strong doubt and prejudice. It must be admitted that they often trust it where others would not dare to do so without some confidence in it. No man can seriously doubt, no one, so far as I know, has ever claimed, that homœopathic physicians do not believe their treatment to be a very efficacious one. If they are not honest, they must either have extraordinary confidence in the unaided powers of nature, or they must deceitfully use other means under the homœopathic guise.

There cannot be general dishonesty among homœopathic physicians without there being constant fear of their being exposed by each other and detected by others. One very natural result would be, the dread lest those who now oppose them by argument and ridicule, should at last be tempted to leave these indirect methods and

resort to a much shorter and more direct method of proving them to be impostors. It would be interesting to enquire if, in the search after evidence, homœopathists betray any suppressed anxiety to draw their opponents away from direct experiment with their doses.

It cannot reasonably be said that they trust so much to the powers of nature as not to care for the effects of remedies. It is incredible that they should undergo such an amount of labor as they are known to, merely to maintain an appearance of consistency. What possible motive can be suggested for their accumulating and studying such a mass of symptoms unless it be to aid them in their practice—not to acquire the appearance of scientific investigation, for Dr. Hooker will tell us it has no such aspect—not to gather a mass of learned phrases, for it were easy to show by as good authority that these descend to minutiae beneath contempt. Homœopathists are as susceptible to ridicule as other men. Ridicule is the very thing to be most dreaded in a system such as theirs. Is it supposable that they themselves should furnish so much material for it without some plausible object? Again, in the application of these gathered symptoms to the practice, there is an immense amount of study requisite for the selection of the remedy. That they really go through this, is evident from the reported cases. These cases show that labor is necessary, or at least is undergone, in applying the remedies to definite groups of symptoms. All this is useless, on the supposition that those who prescribe them know that their remedies are without effect.

Nor is it reasonable to suppose, that homœopathists are in the habit of using other remedies in a concealed form. This cannot be done extensively without it being notorious. Active medicines cannot long be used in concentrated forms without being repeatedly detected

by chemical analysis, and constantly betrayed by their effects on the patient.

Nor have our opponents any excuse for thinking, as they seem to, that the occasional use of ordinary doses is presumptive of dishonesty. Unless this is done in a disguised form, or unless evidence gained in this way is offered as proof of the action of infinitesimal doses, the charge is gross injustice. Homœopathic writers have constantly reiterated the statement that their practice does not essentially consist in using small doses, but in prescribing medicines according to the homœopathic law of cure, and that the question of dose is merely secondary, about which there is room for much diversity of opinion. Moreover, most of its practitioners become such by a gradual change from a different system. The occasional resort to such means as they were formerly in the habit of using, is, in the circumstances, no proof of dishonesty. Indeed, differences of opinion among themselves, when in this transition state, as to the comparative merits in the details of two different modes, are to be expected and are evidence rather of honesty than otherwise, which an entire and immediate conformity to one standard would not furnish. If the statements of a medical witness in regard to any medicine or dose are invalidated, because he or some of his brethren use other medicines or other doses, who but Drs. Brandreth and Townsend can give competent testimony? The question at issue is not whether ordinary doses can be altogether dispensed with, but a very different one—whether infinitesimal doses can ever cure disease. When the time arrives in which the present controversies on therapeutics shall have been decided, it may be found that each of the now rival systems has contributed to the stock of truth something which the others had rejected. The question is not whether

either of them shall then be triumphant, to the exclusion of all others, but whether one shall have added nothing to the common fund.

Nor would it be sufficient to prove that some homœopathic physicians are dishonest: it must be shown that so large a proportion of them are so, as not to leave witnesses enough. If "many of them have been, and are, sincere, honest and learned men,"* they may unhesitatingly admit, that some who are not so have crept into their ranks.†

It is abundant.

Fifth. This testimony is abundant, and is constantly increasing. On other subjects, the amount of evidence is usually thought to be proportioned to the number of witnesses. But the discovery has of late been made,

* Dr. Forbes.

† Dr. Hooker's work on Medical Delusions has frequent reference to homœopathy. But little of it, however, has a direct bearing on the evidence, for or against its doses. There is one exception, wherein he impeaches the honesty of its practitioners. He makes a sweeping charge, "that the stealthy use of medicines in ordinary doses is no uncommon thing, while they are occasionally openly guilty of a virtual relinquishment of their principles." His only specification is the use of camphor in Asiatic cholera, (page 84.) As I have no disposition to discourage investigation into the evidence on this subject, I shall neither attempt to refute nor deny, that homœopathists are inconsistent when they use tincture of camphor in drop doses in Asiatic cholera. As it is done without any attempt at concealment, it is hardly presumptive of dishonesty. Those who are interested in the subject would do well to read the Report of a Committee, appointed by a Homœopathic Society in Cincinnati, on this subject. (See Quarterly Homœopathic Journal, 1850.) To Dr. Hooker I would recommend that he continue the examination into the evidence for and against homœopathy. I would even take the liberty of suggesting a few topics, which his reference to the use of camphor in cholera has brought to my mind.

1st. Will he please tell us, whether or no camphor, in drop doses of the tincture, is a valuable remedy in this formidable disease?

2d. If he thinks it is so, will he please to tell us on what principle it acts, and to whom we are indebted for a knowledge of its appropriateness?

3d. If it is useless in cholera, will he tell us why Hahnemann laid himself liable to the charge of inconsistency with so fruitless a result?

4th. If there are some cases in which it is valuable and others worthless, will Dr. Hooker tell us briefly how to distinguish them?

5th. Will he tell us, lastly, in what doses homœopathists recommend and use other remedies for cholera, when, according to their principles, camphor is not indicated, and with what success?

I think Dr. Hooker will find that these topics will help elucidate the evidence on this subject.

that no amount of testimony is sufficient to establish a new system of medicine. Thus it is said, that the history of medical delusions shows, that abundant testimony may be brought forward in favor of the efficacy of supposed remedial agents, which shall yet prove to be worthless. The inference is drawn, that the great amount of evidence which homœopathy furnishes is not conclusive of its truth.

Of the instances on which this conclusion is based, none is so striking, and none is so much appealed to as Perkinism.* If the history of the "metallic tractors" does not invalidate the evidence in favor of homœopathy, that of "tar water," and other similar "delusions" cannot do it.

To make the application valid, it is necessary to show,

1st. That Perkinism had as much collateral evidence in its favor as homœopathy has. "The credibility of testimony greatly depends upon confirmation by collateral circumstances, and on analogies supplied by the aid of reason, as well as mere experience."

2d. That the testimony would bear analysis, and was found to be as complete in every respect.

3d. That the downfall of Perkinism was conclusive evidence of its being a worthless system, and that it was not crushed unjustly by the authority and ridicule and neglect of unbelievers.

But after this is done, and Perkinism is shown to equal homœopathy in the evidence it furnishes; to have been as probable from analogy; to have had as satisfactory presumptions in its favor; to have had as positive, intelligent and honest witnesses; and, finally, when it is shown to have been all imposture or delusion, and thus to have deserved its fall, our opponent has still more to do. He is bound to tell us why, if homœopathy is equally

* See the works of Dr. Holmes and Dr. Hooker, heretofore referred to.

worthless, it has not shared the same fate. The same year in which Perkins promulgated his discovery, was also marked by Hahnemann's first essay. Dr. Holmes tells us, that in fifteen years the tractors were almost forgotten. If it takes so short a time to demolish one false system, there must have been sad mismanagement in the treatment of the other. Its witnesses are already spread over a period of more than half a century. They are found in every civilized country on the globe. In those countries where, ten years ago, homœopathy was said to be already declining, its practitioners have, in the interval, increased many fold.* The establishment of hospitals and dispensaries, of homœopathic professorships, and quite recently of their own medical colleges, are further evidence of progress during that period. With some slight fluctuations they are still increasing, and with a rapidity which has never been equalled by any radical change in medical opinions. The testimony they have given, has been faithfully adhered to amid ridicule and persecution, and, in some cases, where arbitrary laws have required the giving up the practice of medicine, or the renunciation of homœopathy; yet I know of no man who, being once recognized by homœopathists as a homœopathic practitioner, has been led by any motives to renounce his belief in it. The assertion may be safely hazarded, that no statements have ever been made on any subject, sustained by testimony so numerous, so positive, so intelligent, and, apparently, so sincere, and yet proved to be false. Until such an instance can be found, no appeal can properly be made to the "uncertainties of medical evidence," as an excuse for neglecting to use the only means of attaining certainty.

As disinterested as possible.

In considering this evidence, the mind is doubtless of-

* See a list of Homœopathic practitioners in Great Britain, in the Brit. Jour. of Hom. for 1850. Also, abundant evidence to the same effect in the various Journals of Homœopathy.

ten biassed by the feeling, that it is less worthy of confidence, because it is not *disinterested*—being the testimony of those already committed to the system. This consideration is certainly entitled to considerable weight. Indeed it would appear to be the principal reason why medical testimony carries with it less force than that on other subjects. The witnesses usually have a personal interest on the side for which they testify, and are tempted to exaggerate their success, and to diminish their failures. By its aid, any supposable mode of treatment is thought to be able to obtain any required amount of testimony. It is, therefore, important to determine how far homœopathic evidence is invalidated by it.

I observe, then, 1st. This consideration applies at least as much to all other modes of treatment. If they can prove that their medicines ever cure, as well as disturb their patients, homœopathists can prove effects from theirs, provided their testimony is competent in other respects.

2d. Testimony is much less likely to mislead us through interested motives on the part of the witness, than opinions would be. The statements of the witness can be examined and cross examined. His specifications can be separately tested. We can judge as to his motives and competency; can be aided by his manner, and we can compare the statements of the different witnesses. Those who offer to us, as the ground of belief, merely opinions, but imperfectly furnish such tests.

3d. This evidence is the best which the nature of the subject admits of. No better evidence is attainable on disputed medical questions, than such testimony as we offer, unless it be our own observation, which we cannot impart to our opponent in any other way.

4th. This testimony is less objectionable on this ground than that of some other systems. There was a

time in the history of most homœopathic physicians, when it could not have been plausibly said, that it was for their interest to admit these effects. Almost every day some member of the profession gives in his adherence to the new system ; but his testimony, which before would have been admitted in regard to the effects of medicines, is immediately branded as interested, and rejected as worthless. Some of them assert, that they commenced their experiments to prove the doses inert. Many of them declare that interest, prejudice, pride of opinion, their hopes of success, the authority of the wise and the sympathy of friends, all united, in restraining them from such enquiries, and from yielding to their results ; and yet unhesitatingly assert that which, if disproved, (as it might be, if false,) would ruin them. To rebut such testimony, an overwhelming amount of opposing testimony, as unexceptionable in all its particulars, ought to be accumulated.

OPPOSING EVIDENCE.

Such is an imperfect exposition of the evidence in favor of infinitesimal doses. I can see but two modes in which the conclusion to which it appears to lead can be avoided. It must either be shown that the evidence is rebutted by that of an opposite character, contradicting what I have adduced, or that there is some other way in which the results admitted to take place, can be accounted for, without referring them to the apparent cause, the infinitesimal dose.

It is not unfrequently said, that the efficacy imputed to these doses is disproved by their being utterly inefficient in the hands of all but homœopathists. It is, therefore, important to enquire—

First. What evidence would be necessary in order to disprove, by experiment, the efficacy of infinitesimal doses ?

Second. Has such evidence been furnished ?

Third. If not, what substitute is offered ?

What would be needed to establish the negative.

In order to disprove the efficacy of infinitesimal doses, it would be requisite—

1st. That the witnesses be at least as numerous as is usually thought necessary to settle other medical controversies, where the points at issue are questions of fact.

2d. That their reports be precise and circumstantial ; such as will enable us to judge of their fairness ; to see that the medicines are rightly selected ; minute details as to the circumstances, would be demanded as indispensable.

3d. That there be a considerable range of disease, lest the experiment be confined to one set of cases, and those selected ones.

4th. That a sufficient number of the remedies be tested to show, not merely that some of them were inert, but that most of them were so.

5th. That the experiments be spread over sufficient space and time.

It is not possible to fix, in advance, the extent to which these considerations would lead. The limits as to the number of witnesses, the number of cases treated, the variety of remedies, the length of time, &c. would depend on circumstances—one of which would be the amount of evidence in favor of the doses ; another would be, should there be, at times, some appearance of success, which would render it necessary to extend the trials and guard more carefully against other influences. The experiments should be continued and carefully reported until the conclusion should be fairly deducible from the evidence, that the doses were inert.

If this is not possible, if sufficient evidence cannot be accumulated within reasonable limits of time, space and

labor to convince the unprejudiced enquirer, that infinitesimal doses are inert, it ought to be so said, and the present claim abandoned. But no such ground is taken. Such evidence is said to be easily attainable. It would seem that hardly more pains is necessary than to determine the value of a single medicine, iodine for example. We ought not to be asked to accept of less where the merits of a system of medicine are in the issue. If we are offered less than is plainly demanded by the nature of the subject, it would be a fair conclusion that no better can be obtained. No principle is more fully settled than that which requires, that on disputed points the best evidence of which the nature of the case admits, should always be produced. In the present instance, every possible motive would prompt to the furnishing such evidence.

What has been furnished.

Second. Our next enquiry is, has such evidence been furnished? Pereira, in his *materia medica*, has a short notice of homœopathy. He gives what he thinks conclusive reasons for rejecting it. The principal one is thus stated: "Homœopathy has been fairly put to the test of experiment by some of the members of the "Academie of Medicine," and the result was a failure. Andral tried it in one hundred and thirty or one hundred and forty patients, in the presence of the homœopaths themselves, adopting every requisite care and precaution, yet in not one instance was he successful."* He does not quote for us any of the cases, and makes no allusion to any other trials.

Dr. Oliver Wendall Holmes, in a lecture written expressly to disprove the claims of homœopathy, appeals to this trial of Andral as if it were conclusive. The only additional evidence which his account of it would

* *Mat. Med.* Vol. 1, p. 153.

furnish, is found in the following passage: "It deserves notice that he experimented with the most boasted substances, cinchona, aconite, bryonia, belladonna. Aconite, for instance, he says he administered in more than forty cases of that collection of feverish symptoms in which it exerts so much power, according to Hahnemann, and in not one of them did it have the slightest influence, the pulse and heat remaining as before."*

Dr. Holmes says that "Dr. Fleury, a most intelligent young physician, treated homœopathically more than fifty patients suffering from diseases which it was not dangerous to treat in this way," with "every precaution," and "found not the slightest effect produced by the medicines." He says that nine of these cases—which, by the way, he does not quote—"indicate the absolute nullity of aconite, belladonna and bryonia, against the symptoms over which they are pretended to exert such palpable, such obvious, such astonishing influences."†

Dr. Holmes also appeals to two trials of homœopathy where the prescribing physicians were homœopathists, one at Naples and one at Paris, in which the results were altogether unfavorable to homœopathy; but he does not think it necessary, or cannot afford space to quote any of the cases. We must trust to his opinion that they were fairly selected, that the remedies were properly applied, and that the results indicated that no effect was produced by the doses. We learn, at least, that the process of proof is simple and easy.

Dr. John Forbes concedes that medical men have not adequately put homœopathic doses to the test of experiment; although he thinks such experiments as far as they go, must be considered unfavorable to the claims of homœopathy. "At present we have no such experiments conducted on a sufficiently large scale to render the result valid."‡

* Homœopathy and Kindred Delusions, p. 57. † Ib. p. 57—59. ‡ Review of Homœop. p. 23.

Dr. Hooker's late work, written to guard against medical delusion, and having more space devoted to homœopathy than to any other single subject, does not allude to such evidence.

A number of reviews and essays written expressly to disprove homœopathy, have fallen under my observation, but in none of them have I found anything which would add materially to the above evidence. Either no allusion is made to such evidence, or the above trials are appealed to as decisive.

This is not sufficient.

In regard to this evidence, I would take the following positions :

1st. It is not sufficient to decide the point at issue. The witnesses are not numerous. Their reports are not given with sufficient detail, or else they are very badly brought before us. There is no evidence that the cases were fairly selected or properly treated. Nor are they spread over sufficient extent of space and time to settle a medical question, even if their evidence was altogether unexceptionable. I have never seen Andral's report. Homœopathists say that it shows he did not understand the system he was experimenting with. If this is a feigned excuse, it can be shown to be so. Our books and principles are before the world, and reported cases can be compared with the indications pointed out in them.

2d. The only appearance of proof which these statements furnish is that in regard to the action of aconite in "that collection of feverish symptoms in which it exerts so much power, according to Hahnemann." This, and perhaps that in regard to belladonna and bryonia, are the only specifications in the whole testimony. On this it should be observed, that about ten years after this statement was made, the antiphlogistic properties of aconite were admitted by the Edinburgh College of Sur-

geons, and a gold medal was bestowed on the *discoverer*. Again, Robert Liston, an authority as good as Andral, recommends aconite and belladonna for similar symptoms in his *Elements of Surgery*,* and lectures: in the latter, giving credit for them to the homœopathic principle, and expressly stating that he had witnessed their good effects when “prepared according to the homœopathic pharmacopœa.”† Perhaps if we had more specifications we might gain more light on the subject.

3d. It is plain that the evidence against infinitesimal doses is not that of experiment. We have before us, if not all the evidence attainable, at least all that is usually appealed to. If such evidence was abundant, it would doubtless be furnished. I have searched through the limited number of books and journals to which I have access, and have yet to find the first case reported by an opponent of homœopathy, and appealed to as a failure, in which the symptoms are detailed so particularly as to indicate what remedy would be appropriate. If such cases can be found and reported by honest experimenters, they are so extremely rare as to furnish but slight evidence on this side of the question. If infinitesimal doses are ineffective, there need be no difficulty in accumulating positive evidence to that effect. A physician may or may not be bound by his duties to his patient, to furnish such evidence. This is a question with which we need not interfere. But he is under obligations to do so in controverting and ridiculing the advocates of these doses.

The statements of homœopathists, if true, can be easily and repeatedly verified by experiment by all who will undertake it—if they are false, their system is at the mercy of any one who chooses to adopt it. Its falsity

* *Elements of Surgery*, by Robert Liston. Phil. 1842, page 58.

† *London Lancet*, 6th and 13th Feb. and 16th of April, 1836.

can be proved by any one who will take the trouble to make it the subject of thorough examination. He who shall so far make himself acquainted with it, as to detail cases treated according to its formularies, with the symptoms, so particularly noted as to preclude all fair gain-saying: such a man, if homœopathy is all delusion, has reserved for him a niche in the temple of Fame as yet unoccupied. By prompting others to like experiments, by contrasting them with the reported cures with these doses, by accumulating evidence, such as the nature of the subject calls for, and an amount of it having some slight reference to its importance, rather than suggesting doubts, objections and ridicule, he can consign homœopathy to the same oblivion in which Perkinism and medical delusions were so speedily entombed.

What substitute is offered:

Third. When an opponent of these doses is urged to furnish such evidence, he generally offers as an equivalent, the uniform and decided opinions of the medical profession against this and other features of homœopathy. The claims made in their behalf are spoken of as being "an outrage to human reason," as being "possible only by a miracle," as being rejected by all "but a small fraction of the medical profession." Such expressions as these: "we are agreed," "all physicians know," "every sensible person must see," are frequently used as being a sufficient reason for not giving more direct evidence. No consideration appears to have so much influence as the weight of authority which such opinions carry with them. Their most substantial basis is usually thought to be the argument from analogy; next in order would appear to be the considerations which arise from the supposed unreasonableness of the homœopathic doctrines, which, if it were altogether true, would be no decisive argument against the efficacy of the doses. Nor

would it be enough to show that these opinions are, for all that appears to the contrary, founded on adequate reasons. Most men feel their incompetence to judge of the merits of medical theories—to decide for or against the absurdity or validity of medical dogmas—they might perhaps judge as to the credibility of testimony, and other direct evidence, but where this is denied them it is natural to enquire, on what principle their assent is expected.

Hence it is important to determine if the opinions of allopathic physicians, in regard to homœopathy, constitute a valid argument against infinitesimal doses, and if so, how much weight is due to them.

The offer of opinions is unphilosophical.

1st. It is opposed to the principles of the inductive philosophy to offer opinions, however general and apparently well founded, as decisive of important and disputed questions of fact. It is both the basis and the glory of that philosophy, that every opinion must be held subject to the evidence furnished by observation and experiment; that “though a *single* well attested fact should overturn a whole system, that fact must be admitted.” The history of medicine, as well as that of other sciences, should teach us great caution in adhering to opinions, as such, after it becomes possible to confirm or correct them by observation. We learn from it that a most fruitful “source of medical delusion is to be found in the disposition to theorize instead of encountering the labor of strict observation.” The prevailing system of medicine has, at every period of its history, been in an attitude of hostility to some views, afterwards admitted to be true. The history of sects, and of individuals, illustrate the positiveness of medical opinions, and the change which they may undergo. One after another of the heresies has been permitted to modify the standard of orthodoxy.

Dr. Hooker admits, that even Thomsonianism may have some truth in it: possibly homœopathy may be permitted to take its turn next.

But they are not uniform.

2d. These opinions are neither uniform nor consistent. Of those physicians who have brought these opinions to the test of experiment, a great majority have yielded them, and adopted the contrary. Still more of them may do so. There is no absurdity in supposing that such changes may go on till the weight of authority shall be equally divided. Some physicians admit the point at issue without becoming homœopathists. Others concede it by claiming that homœopathic remedies are dangerous, even in infinitesimal doses.

Milligen, in his "Curiosities of Medical Experience," admits the efficacy of these doses, and is quite earnest in his appeal to his brethren to give the system a trial.

Pereira says, "the doses in which these agents are exhibited are so exceedingly small, that it is difficult to believe they can produce any effect on the system." At the same time, he virtually concedes their powers, by urging that if administered according to the homœopathic principle they would only increase the disease.* If they are large enough to produce this effect, must they not have some effect on the system?

In an article in the British and Foreign Medical and Surgical Review,† we find the following passage: "However much there may be of fiction and delusion in the statements of the homœopathists concerning the specific adaptation of their infinitesimal doses of particular medicines to forms of disease denoted by especial symptoms, we are inclined to believe that legitimate medicine may, in this respect, as well as in some others, have cause to admit that Hahnemann has done good service to the healing art."

* *Materia Medica*, vol. 1, p. 153. † Jan. 1846. p. 166.

Prof. Brera says, "with respect to the dose, apparently so minute, there is not a physician of any practice or experience, who ought to reject it as altogether erroneous and inefficient in every case."*

In some instances we have an implied admission of the power of these doses, from those who would doubtless refuse to avow such a conclusion. The following may serve as a sample of such: "Let the hydropathist praise his water-cure—the homœopathist his concentrated and infinitesimal doses—the Thomsonian his steam and lobelia;—their remedies were yours long before their systems originated."† Such expressions can hardly be reconciled with so positive a conviction of the inertness of infinitesimal doses as excludes all doubt.

In other instances we have an approximation towards the position held by homœopathists which it is well to observe. It is no uncommon thing to hear such statements as these: "We all use far less doses now than formerly." "I now use a sixteenth of a grain where once I used a quarter." Some go so far as to say, that they believe medicines are efficacious in doses very much smaller than those generally used. How they can avoid seeing that this is a fatal blow to their own therapeutics: that, taking their present position as a starting point, the same evidence, which has brought them to it, would as certainly carry them still further; and that no greater additional change than this indicates, would plant them upon homœopathic ground, I am at a loss to imagine. It would require some assurance to ask for any admission which would more inevitably yield our claims than this.

It would be easy to show, were it relevant to the point at issue, that most of the other features of homœopathy

* *Anthologia Medicele*, 1834.

† Address to the graduates of the Med. Inst. of Yale College, by J. G. Beckwith, M. D. 1848.

are conceded by many physicians who reject the system as a whole.

Still, it must be admitted, that a great majority of the profession are unwavering in the opinion, that there is no truth in homœopathy, and, consequently, that there can be no efficacy in its doses, and as we cannot prove in advance that this preponderance of authority will ever be removed, it is necessary to determine more definitely the weight which is due to it.

Nor are they authoritative.

In judging of the weight due to opinions, the principal questions to be considered, are :

1st. What are these opinions ?

2d. Are they based on a full examination of the evidence ?

3d. Are they disinterested ?

The answer to the first question is not easy. Medical men are not always consistent with each other, and with their subject, in expressing opinions on homœopathy, even leaving out those who admit part of its claims. Fortunately the work of Dr. Hooker will enable us to see with sufficient accuracy what those opinions are. Its manifest ability, its avowed object to guard against medical delusion, and its partial sanction as the prize essay of a State Medical Society, give it unusual authority.

They are neither probable nor consistent.

It is the general opinion of the medical profession, that there is no truth whatever in homœopathy. "There is absolutely no truth whatever in this system."* To make this still more emphatic, we find it in contrast with the statement that "there is some truth, a little in Thomsonianism." But it is thought not merely that there is no truth, but that there is no evidence in its

* Med. Del. page 54.

favor, not even an appearance of it. "The almost universal rejection of homœopathy by medical men is to be referred, not to a hatred of new things, but to the absence of even a semblance of proof in its favor."* "The so-called arguments advanced in defense of Hahnemann's system, are made up of the most flimsy plausibilities, and the loosest analogies."† "Its founder fails to command our respect by defending his theories with talent and ingenuity." "His deficiency in what may be termed scientific acumen, is palpable. The writings of his followers, for the most part, show the same deficiency."‡ "An accomplished homœopathic observer is simply a skillful maker of farragoes." "A belief in homœopathy, after a full examination of its doctrines and evidences implies an obliquity of mind which incapacitates for a just appreciation of truth."§ "While all other theorists have added something to the stock of medical knowledge and experience, Hahnemann has literally added nothing. His very mode of observation forbids any such additions; it establishes nothing. It leads to error, and to error only. It accumulates a vast and incongruous medley, the great mass of which is irrelevant, while that which is relevant is small, almost homœopathically small, and even that is valueless, because it cannot be separated from the rest."|| Its treatment consists in leaving disease to the "curative power of nature," while its followers are referring the results to their infinitesimal doses, either ignorantly, or from cunning devise." "The stealthy use of medicines, in ordinary doses," is thought to be "no uncommon thing with homœopathic practitioners," while "they are occasionally *openly* guilty of a virtual relinquishment of their principles."*** It appears also to be the opinion of unbelievers in homœopathy, that though at some past period it was

* Med. Del. p. 77. † *Ib.* p. 87. ‡ *Ib.* p. 86. § *Ib.* p. 87. || *Ib.* p. 52. ** *Ib.* p. 84.

making some progress, it must have already passed its culmination, and be now on the decline. Thus Dr. Holmes told us nine years since, that "it was going down in England, Germany, as well as in Paris." It "was in its prime in Paris in 1836 and '37, and since, it has been going down." It is generally spoken of as going down everywhere except in the speaker's own neighborhood.

Such is an abbreviated, but I believe, a fair statement of the opinions of most medical men in regard to homœopathy. If opinions differing from these are sometimes ventured, even by those whose views on other subjects are received with the utmost deference, they have but little weight with the mass of the profession, because they are thought to be unwarranted concessions. This is strikingly illustrated by an editorial in a late medical journal,* which pronounces "language inadequate to express its condemnation of Dr. Forbes, for his concessions to homœopathy," whereas, in the writer's language, "until he penned them, he stood first among his fellows in education, in intelligence, in honorable feelings, in practical sagacity."

They are not intelligent.

2d. Are these opinions the result of a full examination of the evidence? Without such an examination, we should not feel warranted ourselves to form a decisive opinion on such a subject. Far less can we be expected to trust to the opinions of others without it.

I shall confine the enquiry to a single point, which, however, is quite a comprehensive one. Are Dr. Hooker and his friends warranted in the conclusion that homœopathy has less truth in it than Thomsonianism has. To come fairly to this result, he ought to have a thorough theoretical and practical knowledge of both systems, and to take into consideration the evidence which each fur-

* New York Annalist, Vol. 1st. p. 166.

nishes—or, more strictly, the evidence which Thomsonianism furnishes for homœopathy he thinks has neither proof nor a semblance of any. In conducting such an examination he ought to enquire, if previous to their announcement, Thomsonianism would have had more probabilities in its favor, and if subsequent researches showed its analogies to be less “loose,” and its presumptions more plausible than those of homœopathy. He ought to learn if its founder by education, energy and talents was more likely to establish a valuable truth; if his mode of investigation was more earnest, laborious, and in accordance with other scientific researches. He should enquire if Thomsonianism had been more distinguished in its discoveries than its rival; if it more naturally assimilated and more duly estimated the orthodox discoveries in anatomy, physiology, pathology, chemistry, &c.; if its disciples were better educated, and indicated superior “scientific acumen;” if it made more converts from the regular ranks, and oftener secured the confidence of those, who on other subjects, seemed best judges of truth: if the success of its treatment was greater, as shown by his own trials of both modes, or at least by observing the treatment of each system in its own hospitals and dispensaries, aiding his judgment by the published reports of both parties. It would not be irrelevant to enquire if Thomsonianism had less the appearance of “quackery” in rejecting the aid of patent laws, and in throwing out to the world its processes and results for its candid estimate of their value and its unrestricted use. Nor would it be amiss to enquire if Thomsonianism was working “more fundamental changes in the healing art” than homœopathy: if the changes which the science and art of medicine had undergone for the last half century indicated an approximation to Thomsonianism, rather than to homœopathy: and if

its claims were more in accordance with other discoveries of the age: such as the development of the heretofore unknown powers of the imponderable agents, and their mysterious influence over the human functions. Such an examination if it led to such results, would, on this point, be conclusive—but though I am not in favor of trusting very much to mere opinion in medicine, I will venture one—that the standard, even if it be called orthodoxy, which should lead to the conclusion that there was more truth in Thomsonianism than in homœopathy, must itself be a crooked one. That the opinions of allopathic physicians, even the wisest and most candid of them, are not founded on a careful series of experiments with infinitesimal doses, and therefore are not intelligent on the question now at issue, is, I cannot doubt, sufficiently obvious.

They are not disinterested.

3d. A consideration no less important in judging of the weight due to opinions is, whether they are disinterested. However uniform these opinions may be, however intelligent may be the reasons for them, however firmly they may be held, if they are not disinterested they cannot be offered as competent evidence.

An assent to the efficacy of homœopathic doses, from a physician in the ordinary practice, requires a laborious examination amid circumstances little calculated to prompt to such an undertaking. His pre-conceived opinions, the dread of ridicule, the difficulties of the subject, the “uncertainties of medical evidence,” are a few of these circumstances.

It would require an admission of having been previously in the wrong, that his opinions had been unfounded, that his teachings were false, his ridicule and opposition unjust, and his practice based on error.

It would usually involve or ultimately lead to great

expense, and the sacrifice of much which had already been incurred ; it would involve the partial loss of business, which would be immediate and certain ; any compensating influence would be uncertain, prospective and dependant on circumstances, one of which, perhaps, might be, whether the new views should prove to be valuable truth or no.

It would involve a loss of influence, and a part of that respect and confidence which he had previously held. In some localities it would induce a loss of the usual professional courtesies and sympathies, and might bring upon him ridicule and opposition. If he was a member of a medical society, it might lead to a public, and possibly, "dishonorable" expulsion.

Henceforth the toils of professional life must be undergone under the blighting influence of an evil name. His success, should he attain any, will be explained away, instead of being admitted as evidence in his favor, as none but his unfortunate cases are considered competent to decide the merits of his treatment. He must be cut off from fellowship with his former associates, conscious that the presumptions in the minds of men are against him, and embarrassed by the knowledge that they suppose that the means on which he relies are unworthy of trust.

These are but samples of the considerations which must be overcome or faced if an allopathic physician should assent to homœopathy.

It should be observed that the question is not whether now and then there might not be a physician, obscure, inexperienced, and unable to gain respect or employment, for whose interest it would be to give credence to these doses, but the point is, whether it is for the interest of the wise, the sagacious and the successful, those whose opinions, if admitted, would be entitled to some weight in the

argument. It will not, I think, be questioned that it is for the interest of such to believe infinitesimal doses to be inert.

This consideration alone is sufficient to preclude their opinions from having much weight in the discussion. Homœopathists are not allowed to offer their opinions as evidence, but are obliged to resort to far more direct sources of proof. There can be no reason why those of their opponents should fare better, especially as they are not uniform, are fast giving place to different views, and are not based on a full examination of the evidence.

Partial summary.

It appears to me that the following positions have been established in the foregoing pages :

1st. That the human system is at times extremely susceptible to the action of medicines. So much so, in fact, as to receive impressions from amounts which correspond in minuteness to the doses we usually style infinitesimal.

2d. That on mere *a priori* grounds this susceptibility would more reasonably be expected under the circumstances in which homœopathic medicines are given, than under any others which can be pointed out in advance.

3d. That our opponents admit that an unusual susceptibility to doses which they consider very small, is to be expected in those cases in which the medicine is homœopathic to the symptoms.

4th. That there are certain facts and principles yielded by allopathists which afford a sufficient presumption in favor of infinitesimal doses, to make their efficacy credible, provided the evidence furnished by experience preponderates in their favor.

5th. That experience is decidedly in favor of the doses, whether we consider the number of the witnesses or any of those qualifications which would make them competent to give testimony on this matter.

6th. That there is no such evidence against these doses as the nature of the subject calls for: whereas, if the objections which are thought to exist against them are really valid, there need be no difficulty in furnishing direct evidence to that effect.

7th. That, therefore, the principle which requires men to yield their assent in that direction where is found the preponderance of evidence, makes it incumbent upon all candid minds to admit these doses to have remedial efficacy, and that whatever conclusions this may lead to, in regard to the theory and practice of medicine, should be fairly dealt with.

EXCLUSION OF POSSIBLE ALTERNATIVES.

The state of the controversy in regard to homœopathy makes it necessary to enquire, what disposal those who deny the efficacy of infinitesimal doses make of the facts and arguments which have now been advanced. To some extent this question has already been answered. There is neither uniformity nor consistency among our opponents when they undertake to account for the evidence which has been accumulated in favor of our system. Some reject it altogether, as unworthy of serious notice; others attribute the results to the unaided powers of nature; others refer them to the rules for diet, regimen, &c.; some appear to think them due to the influence of the imagination and the encouraging efficacy of faith and hope; while others are satisfied with the assurance that the practitioners must be dishonest either in the use of remedies or in the report of cases. These, in various combinations, include, I believe, all the methods whereby homœopathic evidence is attempted to be set aside. The attitude of our opponents, rather than the merits of the question, makes it necessary that some of these positions should be further examined.

Here the burden of proof should be on the other side.

I would therefore observe : It is unreasonable to require this of homœopathists. They have furnished a sufficient amount of evidence and offer more. No medical testimony is more uniform or more conformable to the rules of evidence in regard to the remedial efficacy of any medicines, calomel, sarsaparilla or lobelia, or of any processes, vomiting, bleeding, or "packing," than that in regard to the remedial action of infinitesimal doses. Those who refuse to admit it are bound to show, not that some other alternative is possible, but that such alternative is unavoidable. While we are examining one point, our opponents might, as in fact, they do, fly to another, and never come to an issue with us. Even if they can be justified in their refusal to put these doses to the test of experiment, they ought not to require of us to furnish evidence which shall necessarily exclude all doubt ; far less ought such a demand to be made on a question which they admit is so easily settled by experiment.

Nor would it by any means be sufficient to show that in some one of these ways it would be possible to account for any fact proved by homœopathists. It should be shown that such an explanation was probable. It would, for example, be contrary to all fair reasoning to explain away any of the cases by imputing dishonesty to the practitioner, unless this dishonesty is previously shown to be probable. The presumption that any single fact which admitted of two explanations, was due to one rather than the other, might be very slight, while the conclusion that a great number of such facts must, at least in part, be due to one of these causes, might be irresistible. It may be quite a matter of doubt whether the latest death among our acquaintances was owing to disease or to accident, or to want, but a matter of en-

tire certainty that each of these causes of death has a real existence.

Alternatives examined.

When a case of disease is put under homœopathic treatment, it is either,

1st. Entirely unaffected thereby, growing worse or better, according to circumstances, just as if left to nature : or,

2d. It improves, as a consequence of the treatment : or,

3d. It is made worse by the treatment.

If a great number of cases, through a long period of time, are left to this treatment, some tendency would be found to a common result, which would indicate that the treatment was either inert or beneficial, or injurious. The evidence heretofore detailed, sufficiently shows that the treatment is not injurious. Those whose views I am controverting, will not be so inconsistent as to claim that homœopathic treatment is injurious.

The vis medicatrix naturæ.

The position that disease progresses under this treatment, just as if left to nature, cannot be maintained, for,

1st. This would preclude allopathists from claiming to have any agency in benefitting their patients. The evidence goes to show that the results of homœopathic treatment are as favorable as those to which they are contrasted. If the evidence is all on one side, that is not the fault of homœopathists. The witnesses testify that this treatment succeeds as well as the other. If that is worth any thing, this must be also. The improvement of the patient is generally considered the surest evidence of a beneficial efficacy in the medicine. This is asserted to be at least as decisive in regard to infinitesimal doses as to any others.

2d. This position is opposed to the statement of the witnesses, that in the progress of this treatment, innumerable coincidences arise which are so striking as to preclude all possibility of doubt as to their connection with the remedies.

3d. It is irreconcilable with the claims of a large number of our opponents, who admit the cures, but attribute them to the effects produced on the imagination, and to dieting, and hygienic means generally. Prof. R. M. Bird says of this treatment :* “It is but an elegant modification of the practice, the ever fanciful and ever varying practice of Moral Medicine, the *Mentis Medicinæ*, which has been in vogue among regular physicians in what they consider the proper forms of disease (for they cannot believe it to be adapted to all) since the days of Hippocrates.”

4th. This view is opposed to the position of those who magnify most strongly the “*vis medicatrix naturæ*.” Those who reject the use of drugs altogether, find it necessary to make the most of such means as are not strictly medicinal. Attention to dietetics, exercise, bathing, fomentations and nursing are avowedly a necessary antecedent in their cures. They attach so much importance to these means as to conclude that thence homœopathic cures result. Dr. Isaac Jennings says of the success of homœopathic treatment : “Its evidence is full and complete.” Of its practitioners he says : “their present practice is, in my apprehension, nearly faultless.”†

It is frequently said of homœopathic treatment that it leaves disease unaided at all, that its cases do just as if left to nature. I think these considerations show that this is a very reckless assertion. It is opposed to almost

* Introd. Lecture, Philad. 1841, page 11.

† Medical Reform, by Isaac Jennings, M. D. 1847, pages 247 and 268.

all the evidence on this point, and to the claims of every school of medicine. It is none the less opposed to the evidence in regard to the powers of unaided nature. The history of cholera, small pox, and other serious diseases, furnishes very striking evidence of the great superiority over unaided nature, of even the most rude and imperfect means whereby the natural "vis medicatrix" is assisted in curing. To persuade men that disease would be removed as well without medical aid as it is under homœopathic treatment, would lead to important changes in their estimate of the value of medication.

If, then, the conclusion is fairly reached, that disease is in some way removed as a consequence of homœopathic treatment, it must either be through the action of its medicines, or by means of what is called the

Expectant method,

which consists in leaving disease to the restorative powers of nature, aided by nursing, dieting, regulating the regimen, and attending to obviously injurious external agents, and at the same time exciting the hopes of the patient by giving him the impression that he is taking medicine. The progress of our examination brings us to the enquiry, can the results which take place under homœopathic treatment be accounted for by referring them to the power of the expectant method?

On this question I observe: this supposition does not materially affect the evidence of the success of the treatment. A candid decision on the evidence would give it the credit of curing disease as effectually as the more orthodox mode, with all its disturbing appliances. Certainly if, as a general rule, medicines can be dispensed with, and yet the physician and patient persuade themselves that better results cannot be otherwise obtained, our opponents are bound to exchange theirs for the expectant practice, and determine by a careful ex-

amination if, after all, their drugs are of any service to their patients.

But this supposition is inadmissible because it does not reasonably account for the facts.

There are three sets of phenomena to be accounted for. First: such of the recoveries occurring under homœopathic treatment as are not fairly attributable to unaided nature; second and thirdly: the many coincidences where changes in the bodily functions arise on the administration of the doses; such as the checking vomiting, diarrhea, cough &c.—the occurrence of sweating and the restoration of suppressed evacuations, in connection with the use of these doses, and in accordance with the law of similars. The cures are admitted by the view under consideration, and an hypothesis is offered whereby their occurrence is explained without aid from the doses. But there can be no pretence that any considerable proportion of the coincidences, of which we have precisely the same evidence, can be accounted for by the aid rendered nature by dieting, nursing, &c.—another hypothesis must be resorted to for their especial benefit. Accordingly they are explained by referring them to mere accident, and to the influence of the imagination. Now it is hazardous to resort to two explanations to account for cotemporaneous occurrences where one will do it. It is unreasonable to do so unless there is conclusive evidence either in favor of each hypothesis, or against the single one. It is almost an absurdity to do so where the single explanation accounts for the phenomena, and is more likely to be true than either of the others.

To determine whether the explanations offered whereby to account for homœopathic evidence are reasonable or not, we may observe:

1st. It is not probable that so much success would arise

from the entire expectant treatment as is proved by homœopathsists, making every allowance for exaggeration and accident. This mode of treatment, removing obvious causes of disease, regulating diet, and inspiring hope, is not new. Occasionally instances of its success have not been very unfrequent, and have usually been considered quite striking from their singularity. But the conclusions of physicians have not on the whole been favorable to it in any considerable range of cases, much less in all cases. The evidence in regard to it so far as I can find, is quite limited, and is far from being decisive in its favor. Dr. Hooker considers it a "frivolous" mode of treatment. Prof. Alison, of the University of Edinburgh expressly admits that this practice, in the hands of *homœopathsists*, has been "more frequently successful in inflammatory diseases *than could have been expected* ; i. e. the practitioners who have thought themselves justified, by that theory, in trusting more than we do to the powers of nature, aided only by regimen, for the cure of such diseases, have had fewer deaths and better recoveries than we should have expected."*

2d. Fortunately we have in one instance a series of experiments on a scale large enough (as is usually supposed) to test the value of a new mode of treatment—one which has always been considered authentic and decisive. In these trials even the homœopathic diet and regimen were followed "with every necessary precaution." "Medicines" were administered which no one can claim to have had either remedial or disturbing action. The patients had the advantage of having their hopes awakened by the inspiring attendance of one of the most talented and sagacious of physicians. Yet "in not one instance was he successful."† Nay, more :

* Edin. Month. Jour. of Med. Science, Aug. 1850.

† Pereira's Mat. Med. Vol. 1, p. 153.

in those groups of symptoms in which, according to the views we are considering, "accidental coincidences" and apparent results are most likely to occur, not one appeared, and the "absolute nullity" of the "expectant method," was shown conclusively. It can no longer be claimed that under this treatment the success is *so great* as to embarrass our judgment as to the effects of remedies. If homœopathic testimony will bear to be so pruned and analyzed as to satisfy the demands of medical logic, and yet prove a greater success than that of Andral,* few of our opponents can maintain their consistency and deny that sometimes homœopathic medicines have effects. Whether it will or no, I leave others to judge.

3d. Some of the successful results which have been gathered up in regard to the "expectant practice," have evidently been acquired from homœopathic sources. This we have already seen in the quotation from Prof. Alison. I have already referred, page 86, to the treatise of Dr. Jennings, wherein he advocates the efficacy of this practice, and the uselessness of all medication. In recommending homœopathy to the consideration of allopathic physicians, I have been answered by an appeal to his position as proof that the evidence for which I was pleading was not conclusive. He gives us but few cases whereby to judge of the effects of his method. He makes no pretension to report all his cases of any one disease. We cannot judge what his success was, nor what ours would be should we adopt his views. At least part of the success and evidence on which he bases his conclusions would appear to have been acquired under homœopathic treatment. Speaking of homœopathic practitioners, he says: "I uniformly advise my friends to employ them."† To appeal to evidence gained in this way

* Pereira's Mat. Med. and Dr. Holmes' work quoted p. 68. † Jennings' Med. Ref. p. 268.

as proof of the success of the expectant treatment, is simply begging the question.

4th. The disciples of Priessnitz have experimented on the largest scale in treating disease without medicine. Their testimony is important to aid us in judging as to the progress of disease without medication, though owing to the activity of their treatment, we cannot, in an enquiry as to the curative powers of nature, take their results as a standard of comparison. Many of them admit and testify to the efficacy of infinitesimal doses,* and some of them, very properly, as I think, seek to aid their own treatment by openly combining with it the occasional use of homœopathy.

5th. It is unreasonable to impute the success of homœopathic treatment to dietetics, &c. for there is nothing in the directions usually given by homœopathic practitioners which would lead to such results. Their great object, as is evident from their books and published directions, is to keep away from the patient all disturbing impressions. They are satisfied if they can exclude those circumstances which might interfere with the action of the remedies. Accordingly, the restrictions are very numerous, while but little else is enjoined. They use less of the expectant appliances than any other practitioners. In many cases of acute disease the only directions are, take the medicine and avoid such things as might be injurious.

These considerations appear to me to prove that homœopathic cures are not referable to the expectant means. The supposition, however, does not solve the real difficulty. It does not get rid of the evidence of its being a very successful mode of treatment. Our opponents do not speak of homœopathy as being a mere harmless system, as this supposition would imply—it is said

* Gully on the Water-Cure in Chronic disease, p. 45.

to be "either true, or a dangerous, fatal error."† Were it true that the powers of nature so little needed aid from medicine, it would prove both systems useless—but of the two, homœopathy should even then have the preference.

Nor is the second hypothesis—that which attributes the striking coincidences which occur under homœopathic treatment to mental influence and accident, any more rational.

Those who adopt this explanation, ought to tell us whether the homœopathic physician is conscious that this is the agency in his success—whether he is an impostor, or like the patient, is groping in doubt. Such a claim is so improbable, that it ought to be accompanied by precise specifications and overwhelming evidence, either of dishonesty on the part of the practitioner, or of such gross ignorance as to render it impossible to judge of the effects of remedies. That neither of these can be done has been, I think, sufficiently shown.

But it cannot fairly be claimed that homœopathic results are, *to any unusual extent*, due to this cause, for there is nothing in the treatment especially adapted to excite the imagination and awaken confidence. The presumptions of the senses are altogether against these doses. Probably no system of medicine has ever been devised which is so little likely to inspire confidence, apart from its merits. We should expect patients to be discouraged by it, as we know they would be if they were in doubt whether we were not administering to them bread pills, or any other mere placebo; that at least, whenever they found themselves seriously sick, they and their friends would become alarmed at sight of such inappreciable remedies. It cannot be denied that under other treatment there are many circumstances besides the mere remedial effects of the medicines, which tend

* Dr. Holmes.

to keep up the confidence of the patient. The taste, the smell, and the sight of "appreciable" agents, the very pains caused and disturbance created, the vomiting, blistering and purging, the perception of immediate results, all contribute to inspire hope. Will it be pretended that these various aids to faith can be dispensed with and the confidence of the patient as well maintained? If it seems an outrage to human reason to ask a physician to prescribe infinitesimal doses, is it directly the reverse to ask a patient to trust them? Even if the homœopathist owes his success to the expectant means alone, it might be expected that he would do better with doses which had the appearance of being potent: that otherwise he would often have injustice done him by the impatience of those who were induced to make trial of his method. That where it failed to meet their expectations, it would make them active and efficient opposers. It would take a long period of success to so far equalize the presumptions, in the minds of men, in regard to the rival systems, as to give homœopathists a fair chance in the strife.

If, notwithstanding these considerations, it is still held that homœopathic physicians really cure by regulating the diet and habits of the patient, and inspiring confidence in doses of sugar; if results can be thus secured which so effectually rival those obtained by the learning and talents of orthodoxy itself; if this success is such as to secure the confidence of the "intelligent, the refined, and the wealthy," it must be admitted that allopathic physicians are really in an embarrassing position. Debarred from the use of "sugar and rain water," or "metallic tractors," or even "tar water," when, if their consciences would only permit them to do so, they might cure their patients by merely inspiring confidence in these harmless agents, we at least see clearly why all

the "knaves" are driven into the homœopathic ranks, to the great relief of legitimacy. If, as Dr. Holmes tells us, ninety per cent. of the cases which a physician is called to treat would get well without him; and if, of the balance, most cases would do so well on sugar if the physician only had confidence in it, will there not at last be too great temptation for even honest men to resist? If all the distressing effects of medication can be dispensed with, and nobody need be physicked, blistered or bled, if the physician can be saved all anxiety in the diagnosis, and all care in the selection of the remedy, as whatever might be the disease, "globules of sugar" are the only remedial agents—with Dr. Forbes to testify that better results cannot be obtained by orthodoxy itself, and Dr. Jennings to assure him that nature's "vis medicatrix" would not be interfered with by the phantom dose, and even Dr. Hooker to hint that that most important item, the fee bore no ratio to the dose,* is there not danger that these considerations will make heresy too popular? True, it might be suggested that there was nothing in the appearance of these doses to excite the imagination—that the facts would not be accounted for unless the infant draws in faith with its mother's milk, and the delirium of fear is some clairvoyant state wherein credulity is highly exalted—these considerations would be set aside by Dr. Holmes, with the assurance that "on medical subjects the credulity of mankind is boundless," and it will become necessary for Dr. Hooker to "submit it to our friends of the clerical profession," after they have settled his questions in regard to Dr. Bushnell,† to decide whether faith alone is not sufficient in medicine as well as in theology.

* Med. Del., p. 80.

† Ib. p. 94.

CONCLUSION.

I have endeavored to show the credibility of the facts asserted in regard to infinitesimal doses, that they have in their favor fair presumptions and unimpeachable testimony, and that the evidence against them is chiefly speculative. The various alternatives which have been suggested whereby to account for the success of homœopathic treatment, have been seen, if I mistake not, to be irreconcilable with the claims and principles of those who urge them, to be inconsistent with ascertained facts, and to be disproved by experiment. Against such presumptions it is unreasonable to urge the extreme improbability of the asserted facts, and of the conclusions they lead to. The state of the evidence might be such that they would be intrinsically improbable, while their relations to other suppositions rendered them certain. "The improbability of any supposition is estimated from a *comparison* with each of its alternatives. The inclination of the balance cannot be ascertained from knowing the weights in one scale, unless we know what is in the opposite scale." Those who seek to account for homœopathic results by calling attention to the expectant practice, and those who urge the extreme improbability of the action of infinitesimal doses as a sufficient reason for not weighing the evidence in their favor, appear to me to have their attention fixed on one scale to the exclusion of the other.

It has been my aim to confine this discussion to a single topic, not as the most important one in which the two systems differ, for I do not so regard it. But where all the features of homœopathy are alike rejected, as foolishness, our doses are, as all know, the stumbling block which prevents the others from receiving a respectful attention. This question also has the advantage of being more matter of fact than the others are. a consid-

eration which those should duly appreciate who are so much afraid of being misled by speculation and theory. Moreover, much of the evidence which has been accumulated in regard to them has been gathered with infinitesimal doses; so that a determination of their efficacy is important, if not necessary, to decide the other claims of homœopathy. Nor have I undertaken to decide whether the extremely minute doses, which have gained the confidence of many homœopathists, are worthy of adoption. It was not necessary to my purpose that I should do so. I might have failed in the attempt to accomplish this. Such a failure might have been used to invalidate the conclusion that the *lower* dilutions were efficacious. The doses to which my positions have reference, are far smaller than the smallest dose ever thought of, until homœopathy came into existence, far smaller than the smallest now recognized by any other writers on *medicamenta medica* and therapeutics. The settlement of the question whether such doses affect the human system, naturally has a place before the same question is mooted in regard to still higher dilutions, and an acceptance of these doses has, in almost every instance, led to the adoption of homœopathy, and been one of the first steps in the process. If these are disproved, no one would undertake to defend the others; but the higher dilutions may be inert, and the lower have a value beyond all human computation. In this state of things, to urge arguments or excite prejudice against homœopathy because its disciples sometimes or often carry their dilutions still further, would be a virtual admission that this latter position was considered more assailable. It would be to hurl their missiles to either side, instead of aiming at the battery close in front. It would imply that there was some evidence in favor of the doses we have been considering in these pages.

In conclusion, I would respectfully submit to physicians, if they honor this essay with any notice, that its application to *them* cannot be set aside by refuting the arguments, or even by disproving the facts which have been adduced. The question would arise, if this may not be owing to the imperfect manner in which the subject has been treated, or as another expresses it, the want of "scientific acumen;" and of "capacity for the just appreciation of truth" which characterizes the believers in homœopathy. I make no claim to have given a full view of homœopathic evidence, on any one position taken in these pages. Little more has been attempted than to furnish an analysis of the evidence on a single question. If the considerations which have been advanced have no weight, may there not be others in existence which have led so many to adopt these views in spite of the adverse influences brought against them? With physicians, the first question should not be as to the merits of homœopathy, but as to the evidence in its favor. The question at issue is, as to the reality of certain asserted facts; until that is settled, it is useless to spend much time in discussing the causes or the consequences of those facts. Nor is the question whether there is sufficient evidence to warrant the adoption of the new views, but is there any evidence, or enough to prompt to an investigation, or at least sufficient to justify suspending the judgment. It has become customary to explain away this evidence by the strange admission that homœopathy has taught "how much agency the curative power of nature exerts in removing disease." How this absolves them from the obligation to give an unprejudiced examination to the subject, to examine the results of homœopathic treatment in its hospitals, to read its reports and especially to experiment either with its medicines, or with the expectant treatment, it would be diffi-

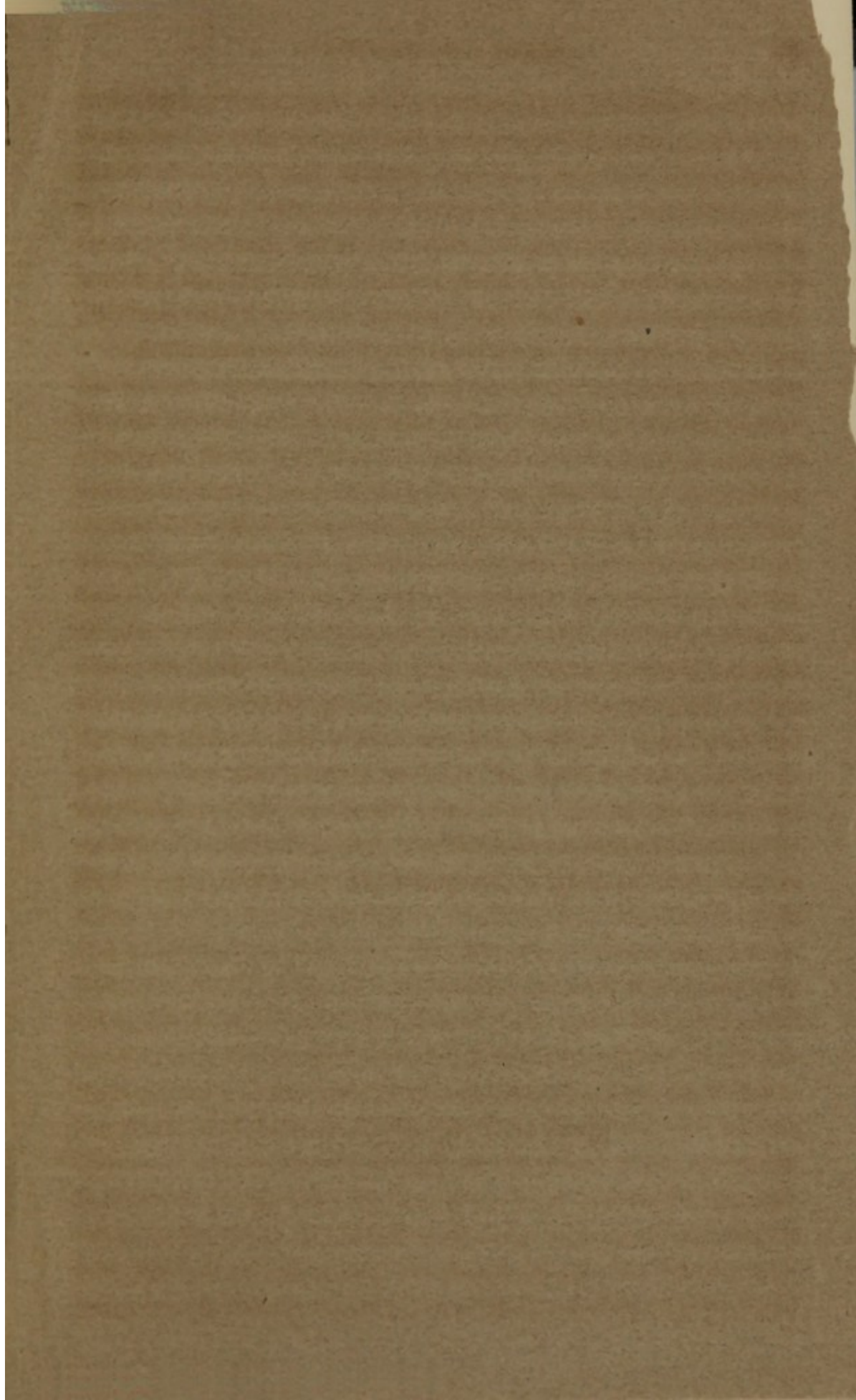
cult to determine. Homœopathy is admitted to have led already to "most important changes in the healing art," to have taught the curative power of nature, to have brought about a far more guarded use of the most active agencies which it found in vogue. After these constrained admissions, is it wise to suppose that the causes which have wrought these changes are yet exhausted? Is it not rather probable that a candid enquiry into the "powers of nature" would lead to still further changes?

Any attempt to escape this conclusion by diverting attention from the question at issue, or by discussing the merits of the rival systems and the reasonableness of their tenets, rather than the evidence in regard to them, and especially the having recourse to ridicule, instead of argument, would too plainly be a confession of the necessity of resorting to such alternatives, to be expected of candid men, conscious of the strength of their position. Not but that they must have opinions in regard to homœopathy. Occasional contact with it they cannot avoid. Some action, which has reference to its merits, they are compelled to take. Members of a profession which claims to be liberal, and avowing that admission into its fellowship is "irrespective of opinions," they will allow one of their number, the only charge against whom, attempted to be sustained is, that he has adopted, practiced and recommended homœopathy, they will see him expelled, and that ignominiously, from their own ranks, without uttering one word of remonstrance to shield him from injustice, or to avert from themselves the shame of participating in the outrage. In their daily intercourse with the sick, in the impressions they impart, and the influence they exert on all who listen with deference to their opinions, they make no exception in regard to the views of which this essay is the result, but treat them as they do the most absurd tenets or the most

notorious deception. It is to be hoped that the convictions under which they act are unhesitating. Motives for caution are all around them. That love of truth which should supersede all attachment to opinions, the charity "which is lenient to the erring while it shows no leaning to the error," their most sacred duties to their patrons, and a regard for their own future fame, in short, every obligation which binds men in a common calling, makes it incumbent upon physicians to assure themselves beyond all doubt, that their opinions on this subject are well founded. But if homœopathy is all a delusion, they have still a duty to perform in regard to it, and one which they cannot always escape. They are bound to furnish the public with the best evidence which the nature of the case permits, that it is really a "fatal, deadly error." For my own part, I can see no intermediate ground which medical men can consistently adopt in rejecting homœopathy, between that which shall prove by experiment the entire inertness of its doses and the absurdity of its principles, and the taking and *maintaining* that which assumes that there is absolutely not merely "no truth in it," but that there is no evidence in its favor, not even a "semblance" of it.



Shawno



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