

Joslin (B. F.)

Discovery of Curatives by Observation.

AN

ADDRESS

DELIVERED

IN THE ASSEMBLY CHAMBER, ALBANY,

BEFORE THE

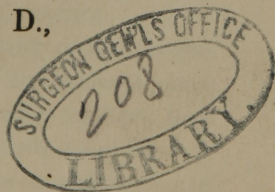
HOMŒOPATHIC MEDICAL SOCIETY

OF THE STATE OF NEW YORK,

AT ITS

ANNUAL MEETING, FEBRUARY 8, 1853,

BY B. F. JOSLIN, M. D.,
OF NEW YORK.



ALBANY:
J. MUNSELL, 58 STATE STREET.
1853.

ADDRESS

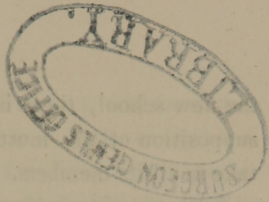
1852

If a time should ever arrive, when Hæmorrhoids shall be proved a delusion, and be generally abandoned by those by whom it had been adopted; if it should become a sheer matter of ancient history, then would mankind regard the former progress and prevalence of this system, as a phenomenon unaffiliated in the history of medicine. And could the wide and extent of the diffusion of this new medical doctrine among intelligent physicians, be now considered by their professional brethren, with the same impartiality as the remote part, few could resist this evidence of its truth.

If any one who is acquainted with the history of the origin and adoption of this system, still considers it so incredible a notion, that he is compelled to believe, he may be most advantageously conclude, that either the most criminal fraud, or the grossest mistake, is chargeable to every member of the largest body of regularly and thoroughly educated physicians, which the world ever saw existing in any one mode of practice.

They have the calm and constant bearing of men in possession of a truth which, from the nature of its evidence, must in due time be generally appreciated. In other matters, they are known to evince as much integrity as their neighbors. I shall be excused from the disagreeable task of obdeseending to refute the charge of professional dishonesty, for it is becoming nearly obsolete, except among the ignorant, or the scold who use the profession as a trade, and having a strong odor of the shop, is repulsive to refined taste.

My argument commences as follows: For those who reject



ADDRESS.

If a time should ever arrive, when Homœopathia shall be proved a delusion, and be generally abandoned by those by whom it had been adopted, if it should become a sheer matter of ancient history, then would mankind regard the former progress and prevalence of this system, as a phenomenon unparalleled in the history of medicine. And could the *mode* and extent of the diffusion of this new medical doctrine among intelligent physicians, be now considered by their professional brethren, with the same impartiality as the remote past, few could resist this evidence of its truth.

If any one who is acquainted with the history of the origin and adoption of this system, still considers it so incredible *a priori*, that he is compelled to assume its falsity, he must inevitably conclude, that either the most criminal fraud, or the grossest mistake, is chargeable to every member of the largest body of regularly and thoroughly educated physicians, which the world ever saw agreeing in any one mode of practice.

They have the calm and confident bearing of men in possession of a truth which, from the nature of its evidence, must in due time be generally appreciated. In other matters, they are known to evince as much integrity as their neighbors. I shall be excused from the disagreeable task of condescending to refute the charge of professional dishonesty, for it is becoming nearly obsolete, except among the ignorant, or the sordid who use the profession as a trade, and having a strong odor of the shop, is repulsive to refined taste.

My argument commences as follows : For those who reject

the new school, there is but one possible alternative; i. e., the supposition of the most criminal fraud or of the grossest mistake in all its members.

That the fraud, if real, is most criminal, none will doubt who reflect on the interests involved. Let those who choose, take this horn of the dilemma.

My chief business is with sceptics who, possessed of more charity and more intellect, take the other horn—the supposition of error. I expect to show the untenableness of this position, and consequently (to those who have any faith in human testimony) the certainty of the homœopathic doctrines.

I hope to prove, so far as time allows, that this system is exempt from the radical defects and fallacies of its rival, and to give such an exposition of its method of investigation, as will enable reflecting persons to perceive the difference between our position, and that of various transient sects, moral and medical.

Our opponents have inconsiderately declaimed about “kindred delusions.” As this in various forms has been frequently and publicly reiterated, I deem it of some importance, not merely to deny the alleged consanguinity, but to give such an exposition of the nature of the principal evidence, as to prove that self-delusion in this case, is impossible.

In order to exhibit the peculiar advantages of the homœopathic principles of investigation, it will be necessary to allude occasionally to other methods. This will not be done with any feeling of unkindness towards a rival school, nor with any intention of rejecting those collateral branches of medical science, of which we are the rightful inheritors. On these points, before entering upon my subject, I ask leave to make an explanation to those who might feel aggrieved. I am unwilling to seem, still less to be, the defamer of that profession of which I have been, boy and man, i. e., old school and new, for twenty-seven years an acknowledged member, and to which I am under so many obligations. I do not here refer to any extrinsic awards, such as an M. D., here, or a fellowship there, though these are duly appreciated, but to that mental aliment, which my medical in-

fancy imbibed from authors which I still respect, and from professors whom I still venerate. From a sense of justice to my benefactors, I may claim for myself, and for these homœopathic brethren (who were also "brought up at the feet of Gamaliel") that the purer portions of that milk of instruction, still continue to sustain our medical vigor, long after the impurities or adulterations have been excreted. As well might a man disown his mother, because some imperfection of her health had rendered his early nutriment less salutary, as the full grown physician of the new school, deny his obligations to the old.

Whatever of truth in anatomical and physiological science, and of skill in surgical and obstetrical art, and whatever knowledge of the natural history of disease, and of the auxiliary branches of medicine, has been bequeathed to us by our grand-sire Hippocrates and his successors, we shall endeavor to cherish for ourselves, and to transmit to our medical posterity, by means of expurgated books and reformed colleges.

Before describing the reformation which our school is introducing into medical investigation, let us glance at a few of the evidences of the necessity of reform.

Every age, including our own, exhibits a succession of attempts to found medical practice on some new theory, mechanical, chemical, physiological or transcendental. Every era, like that of the physiological and world renowned nineteenth century, is satisfied that it is much in advance of its predecessors, and that the medical millenium is approaching; and the more enthusiastic theorists recognize it as already arrived. The theorist introduces some new mode of practice in accordance with his new theoretical views. Now, if any of these new modes should chance to be retained by a succeeding age, do you suppose it would be on account of any remaining confidence in the hypothesis, by which it was suggested to its originator? Such a case is comparatively rare. The medicines and modes which retain, in the highest degree, the confidence of the ablest members of the profession, are those which have been verified by experience; and next in order, are those which can be pressed into

the service of some more recent but often equally fallacious, hypothesis. These are retained, not from any respect to the *past*, but the *present*, theory or conjecture.

A still larger class of methods and medicines are introduced by accident, into popular practice, and subsequently adopted by the profession, on grounds purely empirical. But as the profession aims to be scientific, it endeavors so to modify its doctrines, as to provide a respectable seat for the new visitor. Thus probably, farmers have discovered more medicines than physicians, women more than men, and savages and semi-barbarians more than those who boast of science and civilization. If these medicines persist in curing certain individual diseases, in spite of theory, the profession stigmatize them as alteratives or specifics—a band of outlaws indispensable in guerilla warfare.

This plan, the searching for the properties of drugs by experiments on the sick, is denominated the clinical (i. e. bed-side) method. I shall presently speak of its uncertainty.

The physiological method, or that which bases practice on laws of healthy or morbid action, real or supposed, presents, in general, a problem too complicated for science. The insuperable difficulties inherent in the subject, and not lack of ability in the investigators, has been the cause of failure. I take pleasure in offering this as an apology for the unconverted portion of the medical profession, and in fortifying my position by a quotation from one of their distinguished brethren. Sir Gilbert Blane states, that in a certain portion of his "Medical Logic," his main object is, "to convey an adequate conception of the great difficulties which those have to encounter, who would found practical medicine on a knowledge of the animal economy, and to bespeak a liberal indulgence for the errors of those, who, in attempting this, have had to grope and wander in more dark and intricate mazes," than have "fallen to the lot of any other class of inquirers into the various departments of nature."*

The school which, now, because it has a majority, arrogates

* p. 152.

the exclusive title to science and regularity, may with propriety adopt the remarks just quoted, not only as an apology for themselves, but as a confession for their system, from which, by the very law of its being, such errors are inseparable.

It is creditable to the allopathic school, that volumes of similar confessions might be collected from its ablest writers. Their English Nestor, Dr. Forbes, is dissatisfied with the present, but, like his predecessors, hopeful for the future. He declares that matters are so bad that they can not get worse—that allopathy must either mend or end. Though his junior, I take the liberty to suggest, that more wisdom would be evinced by consenting to its ending, than by attempting any improvement short of a radical reformation. Some of his patches have strength, but the garment is thoroughly rotten, and will inevitably tear all around them.

Yet this is the most respectable of the recent projects for exterminating the school of Hahnemann. It is to the futility of such attempts to galvanize a system destitute of the principle of vitality, that I have on a former occasion referred, in an allusion which will be understood by those who are acquainted with the habits of a certain large, hissing, sedentary, and apparently contemplative animal. The old school, like its anserine prototype, “sits and broods over naked stones, mistaken for eggs, in the fond hope of a progeny which shall one day march forth upon the earth, and drive the young homœopathic chickens back into the shell.”*

Scepticism in relation to the present, and hope for the future, are extensively shared by the profession in our own time and country. About sixteen months since, I was present in the principal medical college of the city of New York, when its ablest professor, with his usual eloquence, and with unwonted enthusiasm, portrayed his conviction of the impending advent of some extraordinary genius, who shall reduce to order the present chaos of medical science—“and,” exclaimed the professor, “what if he should arise within these walls!”

* “Principles of Homœopathy,” p. 22.

The indefinite, yet sanguine hope of the first advent of some radical, yet future reformer of medicine, is entertained by many, who like the children of Abraham, in relation to an infinitely greater Being, are not satisfied of its past realization. I confidently trust, that multitudes of such sceptical friends of medical science, will ere long be consoled with the assurance, that the one medical reformer has actually appeared upon this planet.

The society will pardon a brief allusion to the history of his method, in order that others, who honor this meeting with their attendance, may better appreciate a few principles of medical investigation, which I conceive to afford tests of the genuineness, truth and value, of any proposed method of discovering curative properties.

In the latter part of the last century, a learned German physician, named Samuel Hahnemann, happened to observe an instance in which Peruvian bark produced an intermittent fever, similar to that produced by marsh miasm. He knew that the latter was frequently cured by the same bark. On reflection it occurred to him, that several other drugs produced diseases similar to those which they cured. The longer he considered the subject, the greater the number of similar coincidences presented in the medical experience of the world. Did he then announce a general law? By no means. He merely *suspected* one. His whole course was marked by the most careful and philosophical induction. So far from being satisfied with hypothesis, he was far in advance of his medical cotemporaries, in appreciating the importance of applying the principles of Bacon to medicine as well as other sciences.

What course then did such a mind conceive and execute? Was it to sit down in silent and passive communion with his own thoughts, compare and compound them with each other, and spin out from his own brain a theory, "as a spider does a cob-web from its own bowels?" Did he busy himself with considering whether he could frame a hypothesis in accordance with any imagined internal and occult nature of things in general, or of the human body in particular? He did none of these. He per-

ceived that such speculations had been the bane of medical science, whose logic was far behind that of other natural sciences.

A fact observed and similar ones recollected, had led him to a conjecture. Thus far, his course was like that of Newton. Their achievements were also similar; one discovered a universal law in the world without us, the other in the world within us. Their modes of verification differed, on account of a difference in the nature of the subjects. No mathematical investigation could conduct to a universal law of cure. The problem to be solved by Hahnemann was, whether the symptoms which a drug can cure are similar to those which it can cause. This could be determined in no other way than by two sets of experiments. By long-continued trials of drugs, taken for the purpose by himself and several healthy friends, he ascertained the symptoms which many drugs would severally produce. Thus was discovered the pathogenesis, or disease-producing power. All this was determined by simple experiment, without the slightest mixture of theory.

There is not time to describe the precautions used to secure accuracy, and the immense time and labors bestowed in different countries, in obtaining the results, which fill large volumes. No educated man will deny, that the method is in accordance with the strictest logic of the natural sciences, nor that Hahnemann is entitled to the glory of adding a new branch to the natural sciences, i. e. pathogenesis or the science of morbid properties. But is knowledge, in this case, power? Not in the hands of the old school. To them it is a mere scientific curiosity. How did Hahnemann animate it with potency? By proving the pathogenic, i. e. the disease-producing properties, to be similar to the therapeutic, i. e. the disease-curing properties.

The series of investigations which led directly to this grand result was, like the preliminary series, conducted in a mode purely experimental. When a patient presented a collection of symptoms similar to those produced by a certain drug, this drug was administered, and a cure ensued. After a sufficient number of similar experiments, with different medicines, in various dis-

eases, and with similar results, the great benefactor of the medical art felt justified in announcing the law, *similia similibus curantur, like are cured by like*. The doctrine, and practice in accordance with it, are denominated *Homœopathy*, or in the more euphonious *Latin*, (which those who choose may employ) *Homœopathia*.

This law, like that of gravitation, had been a mere conjecture at its conception; yet in the verification of the medical, as in that of the astronomical law, no trace of hypothesis was allowed to enter the reasoning and vitiate the proofs, by which it was established.

No sound intellect can doubt, that if a sufficient number of successful experiments have been made, the evidence of the truth of this law of cure, amounts to a physical certainty. To this point I shall have occasion to recur. To avoid repetition, I shall consider it in connection with the evidence of the power of small doses.

When I speak of small doses, I do not include those recently adopted by many of the old school, in their attempts to approximate to the new, but to those which, for the sake of distinction from the former, are denominated infinitesimal. Their limits are not settled; but in practice their weight seldom exceeds the millionth of a grain.

Does any one inquire what theory could ever have led Hahnemann to the adoption of such doses? No theory. The reduction of the quantity of medicine, like the law of its selection, was a response of Nature to experimental interrogation. The first doses, employed in verifying the law of similars, acted with violence. On trying a smaller dose, he encountered the same difficulty. After many successive reductions, he ultimately obtained doses which were both safe and efficient. In the use of these, he observed no new disease developed in the progress of the cure, nor entailed in the sequel. A good observer, he could not fail to make this discovery in posology; a conscientious physician, he must apply it in his subsequent practice; a lover of his race, he was impelled to publish.

Do you ask, is not the idea of the medicinal potency of such minute portions of matter unreasonable? It would be easy to show why the pharmaceutic process of Hahnemann is admirably calculated to develop an unprecedented amount of curative power; that the doctrine of dose can be exhibited as a rational deduction from the law of similia; and that both are confirmable by laws of vitality and analogies in physical science. But having on former occasions stated and published these views, I at present limit myself mainly to the inductive features of this science—to the direct evidence presented in the experiments of its founder and the experience of his disciples.

Admit the truth of the law of cure, the genuineness of our pathogenesis, and the sufficiency of small doses, then the conclusion is inevitable, that Homœopathia is of immense practical value.

Now those who have neither reflected upon it as a science nor practiced it as an art, have not even a proximate conception of the facilities, afforded by the peculiar nature of this system, for its verification in all these particulars. Every step in correct and successful practice, simultaneously contributes something to the verification of each of the three doctrines. If in following the rules of the art, the physician observes an improvement or recovery under circumstances which render it impossible to attribute it to the efforts of unaided nature, he must attribute it to his practice; yet this practice must have proved inert, if *either* of the three pillars which sustained it had been essentially unsound. In vain is the law of similars true, if one of the two classes of phenomena which it yokes together have no reality, or if the medical materials, whose application it implies, have no potency. Equally useless is the reality of both classes of phenomena, if the law, which purports to connect them for practical agency, is a mere chimera, or if the curative materials have lost their energy by attenuation. Finally, the drug, however energetic, has but a blind and useless force, unless its administration is guided both by genuine provings of the materia medica and an unerring law of therapeutics.

We challenge the opposers of our system to subject it to this severe and triple test. Does history present an instance of a false system which offered such facilities for its own refutation, and still continued for half a century to be more and more extensively adopted by intelligent men ?

The grand peculiarities of Homœopathia relate, directly or indirectly, to her materia medica, i. e., knowledge of medical materials, or science of the properties of medicines. With this she was able, even in her infancy, to rival her elder sister, who had never been so fortunate as to find the key for unlocking this mysterious cabinet of Nature. Is it improbable that a materia medica could in fifty years outstrip one that had the advantage of starting fifty times fifty years earlier ? This wonder in medical history has been effected by means of a new mode of experimental investigation.

The medicinal properties of any substance are now susceptible of determination. The enunciation of this proposition would excite surprise throughout that portion of the scientific world, in which medical logic has not received any attention. The general wonder would be, not that the proposition should be true, but that its truth should be presumed to be doubted, and such an apparent truism published.

Scientific men, engaged in the cultivation of natural philosophy, chemistry and the several branches of natural history, are not generally aware what a *terra incognita* is the so called science of materia medica. The scientific laity, taking a distant view of therapeutics, have been accustomed to regard it as a science. This illusion has been strengthened by the fact, that real sciences have always been taught in the medical schools, and generally by scientific professors. Chemistry and anatomy, and the mechanical portions of surgery and obstetrics, are sciences. Much of the current physiological and pathological doctrine, is also well ascertained and classified truth. The materia medica of the old school has obtained caste by such associations, rather than by intrinsic merit. The stately colossus of medical education, with a fair proportion of gold and silver in

its head and trunk, has too often been presumed to have a solid foundation; yet its feet are mere pottery, mere clay.

The properties of medicines must be the basis of medication. Whilst these are unknown, therapeutics must remain unscientific and feeble.

In order that the *materia medica* may be established with the same certainty as other natural sciences, it is requisite that the effects of different substances on the human body be ascertained by actual observation. This process must be adopted with every substance prescribed as a medicine. The physician, if his art is scientific, is not at liberty to assume that a mixture of two or more substances, possesses the sum of the medicinal properties of its several ingredients.

As the chemist can not make such an assumption in regard to the properties by which inorganic substances react on each other, neither can the physician in regard to the properties by which inorganic substances act on the living body. It is not, as some suppose, simply for fear of the mutual chemical actions of different elements of a compound prescription, that such preparations are forbidden in our school. There may be mutual disturbance of the vital actions of two substances, without any mutual chemical actions of the substances themselves. We are not at liberty to presume, that because one medicine tends to remove one morbid action, and another medicine another morbid action, therefore the two medicines if administered simultaneously, tend to remove both, or even either of those morbid actions: for the mutual vital reactions of the organism, are no more to be neglected than the mutual chemical actions of inorganic substances. Physicians, in combining in the same prescription different simples having certain properties, real or supposed, and estimating the resultant effect from the separate effects of the ingredients, have resembled an engineer applying different forces simultaneously to different parts of a complicated engine, so connected in all its parts, that the motion of each part influenced that of every other part, and yet presuming that each external force so applied, would have its separate and un-

modified effect, or if in one sense there was a modification, that the resultant force, and even the whole effect on the engine, could be estimated by simple addition.

My object has been to show, that if the medicinal properties of drugs are discoverable, the experimental trials by which these properties are discovered, must be made when the drugs have the same degree of complexity which they are to have in the prescriptions that are to be based on these properties.

If this is true, our art is not a science, except so far as it avails itself of simples, or else of compounds whose properties have been determined independently of any conclusions drawn from the properties of the constituents, and admits only those determined by observed effects. The foregoing considerations aid us in comprehending the rapid advance of reformed medicine, and the tardy and uncertain steps of its predecessor.

No physician who understands the real virtues of medicines will prescribe more than one at a time. The mixture of many in one prescription is called polypharmacy. This has retarded the progress of discovery.

If a man loads his gun with a dozen shot, he will rarely hit the exact point aimed at, in a distant object; and if chance should favor him with such a result, it would puzzle him to divine which particular projectile had taken the requisite course, or in what plane or at what angle it had been deflected, by the interference of its fellows, crowded together with it in the same charge.

After medical experiments of similar complexity, whether successful or unsuccessful, the mind of the prescriber remains in its former darkness, in regard to the properties possessed by the components of his prescription. Or, to exchange simile for metaphor, after such medical firing, the hero, whether victor or vanquished, remains, as to scientific attainments and practical skill, "in statu quo ante bellum."

The rejection of this mode of experimentation, is one of the characteristics of the system I am advocating.

It rejects other methods equally fallacious. The mental en-

ergy of the medical world had been previously squandered in the vain attempt to determine the medicinal properties of substances by various other expedients, one or another of which is still relied on, except among the followers of Hahnemann. It is unnecessary here to refer to researches for such properties as tonic, antibilious, and alterative, and others which are equally general, vague and occult, and which still occupy a prominent position in the prevalent system.

Such supposed general properties as febrifuge, &c., whose names imply a curative relation to extensive classes of disease, whilst there is no evidence of their applicability, except to a small proportion of all the cases of that class, can afford little practical aid to the physician, who has no means of determining to what cases they are respectively applicable. The new school avoids such delusive generalities, and in regard to the medicines which it has proved, has a guide to their particular applications.

In order that any system of *materia medica* may have much value, its list of medicinal properties must be numerous. For the varieties of the properties of disease, if we include the different stages of the various cases, are innumerable, and every morbid property requires a corresponding curative property.

Viewed in these aspects, the *materia medica* of our school presents a striking contrast to all others. How meagre is the list of emetic, cathartic, diaphoretic and other properties, discovered in thousands of years, compared with the number of drug symptoms and consequently available properties, discovered in half a century.

There is a still greater disparity between the number of properties which the new and old methods are *capable* of disclosing in a given *future* period.

This is a test of the relative truthfulness and value of the methods. To any science not yet completed, the discovery of the true method of investigation, imparts life and the power of growing. This aptitude to extension is a test or measure of its vitality.

Apply this *test* to the *materia medica*. A single kingdom of Nature, and one of the earth's geographical divisions which is dear to our hearts and accessible to our observation, afford sufficient materials for this illustration. The enlightened munificence of the Empire State, commissioned eminent naturalists to explore its native treasures. The contents of ponderous volumes attest the perseverance and ability with which the task has been executed. Yet this grand work is, not from any fault in the scientific corps, but from the nature of the objects, superficial. It deals with the exterior of Nature, not with her spirit.

For example, the plants have been described and classified as to their external forms; but who should disclose their internal virtues? Their inmost properties which relate to the laws of human vitality, and thus to the cure of disease, are a sealed volume. Who shall break the seal and open this book of natural life? Its treasures far transcend the mineral wealth of the state, even should it, by future exploration, be found to rival that of California or Australia. Yet but an extremely minute portion of these plants have been examined properly, if at all, in relation to their medicinal properties.

What physician, except a disciple of Hahnemann, can ever even commence such an investigation? Not one. What state society complete it? None but a homœopathic one. If so, the society now assembled is not unnecessary: and as I am afraid I have wearied you with abstractions, I will, as it here comes directly in the path of my argument, use for illustration two societies, old and new, which meet here almost simultaneously. I entreat that this may not be interpreted as an attack on the old one. I have no unkind feeling toward it. I am somewhat interested in its honor; its transactions contain some of my own essays. I value some friends among its members, and appreciate their character. But I return from this digression.

Suppose a leaf, from an unknown tree, none of whose parts had ever been employed as a medicine, were presented to the society now present, and a committee of its members were charged with the duty of discovering its latent powers. After

some months, they would be able to decide to what particular maladies it was adapted, what combinations of morbid phenomena it was capable of annihilating. This they could achieve without trying it in one of these diseases, or even meeting a single case during the whole period of this investigation. In labors similar to this, the society are actually engaged.

Now suppose the same kind of leaf, or any number of tons which might be required, were delivered to a committee of a certain nonhomœopathic medical association, which also holds its annual meetings in this political metropolis. Their members were educated in the same colleges as ours, and they can not allege any inferiority in talent as an excuse for their failure. They are more numerous; and as they would prefer experimenting with large doses, I have allowed them the additional advantage of tons instead of grains.

Does any one imagine that they could make much progress in this discovery in a whole year? They have no rule of proceeding which can ensure success.

Suppose they resort to chemical analysis, in order to detect some active principle, whose medicinal action was already known. There is not one chance in a hundred that such a principle exists in this leaf, nor one in a thousand that it is present in such quantity as to give it a predominant influence as a medicine. There is not the shadow of a possibility of obtaining, by such a method, the true virtues of one of these compounds of Nature, which in medicine are called simples, in contradistinction to the prescriptions of polypharmacy.

The chemical test must be abandoned. Their chance is but little better with the vital. In this the experiments must be either pathogenetic or therapeutic. First what would be the result of their experiments on the healthy? Possibly they might observe one or two of the following effects. A quantity might be swallowed sufficient to secure its expulsion from the stomach or bowels. They would record it as an emetic or cathartic; or with a *quantum sufficit* of hot water, a little of it, or much of the water, might permeate the pores of the skin or kidneys,

when the former would be set down as a diaphoretic or diuretic. We need not complete the list, though it is not extensive. I have selected the pathogenetic properties most generally valued in their system of practice.

Would such results teach the use of the drug? Would the provers know, in what particular cases of disease it might be advantageously or safely employed? Certainly not. In the first place, they have no means of knowing, in what cases these properties are curative; in the second, they have no means of determining, whether this drug could, in any particular case, be substituted, with safety or advantage, for others of the same class.

Having failed in trials on the healthy, they have no resource but to attack the sick. I call it an assault, because, in the premises, confessedly ignorant of the nature of the weapon, they have no prospect of benefiting the patients by the blows inflicted, but are almost sure to injure an immense majority, and to destroy some of them, if the strokes are powerful and reiterated.

This operation resembles the cultivation of anatomy by dissecting living men. May I illustrate the method by an example? When the cholera raged in New York in 1849, no treatment except the homœopathic was successful. Some allœopathic hospital physicians, having exhausted their stock of conjectural remedies, stood waiting for the appearance of a new one. One of their brethren having invented a new compound and anxious to make the experiment in their hospitals, inquired of another physician of the same school, who was supposed to know, whether they would consent to try it? The reply was, "they will *try anything*."

The society which is generally considered an exponent of the collected medical wisdom of the Empire State must also, if they would discover inductively the unknown curatives of a disease, make their experiments on the sick, and with every drug indiscriminately, and with dangerous doses. As in the extension of their therapeutics, so in that of their materia medica, which we have been considering, if they would ascertain what diseases a

certain leaf is capable of curing, they must make their experiments on the sick in all diseases, indiscriminately and with dangerous doses.

Let us apply to this method and that of our society, some other general considerations which, on some reflection, can be appreciated by both schools.

I think it can be demonstrated, that an adequate knowledge of the relations of a drug to vital phenomena can be discovered in much less time, by *primarily* studying the relations of the drug to the *elementary* vital phenomena, than by primarily studying its relations to groups of vital phenomena.

Suppose, for example, that a medicine has seemed to cure a single case of disease which manifested itself by a certain group of symptoms. Any physician accustomed to all the discriminations exacted by the homœopathic system, will readily understand, that another case identical with the former, can rarely, if ever, be expected to occur in his own experience. Some symptoms will be wanting, or some new symptoms present, especially if minute distinctions are made as to the character and locality of the sensations, the conditions under which they arise, their order and respective concomitants. Hence a long time must elapse before there can be any verification of the curative relation of the drug to such a case; and previously to such verification, the recovery can scarcely be regarded as anything more than an accidental coincidence, unless a natural cure of the disease had never been known to occur, and unless also it were known that no medicinal agency, except that of the one drug to which the cure was referred, had been brought to bear on the individual case.

A concurrence of the conditions requisite to any considerable evidence of curative action of a medicine employed for the first time in a particular case, and a minute record of the symptoms of the same case, must be rare in the experience of any physician, who rejects pathogenesis as a guide in therapeutic experiments.

These are some of the obstacles which beset the path of those

who are acquainted with no better experimental method of discovering the medicinal properties of any plant, than by endeavoring to ascertain its relation to a group of phenomena, before they have learned its relation to the *elements* of that group.

I consider this as the grand primary distinction between the allœopathic and homœopathic modes of investigation. As it appears to me to lie at the foundation of that medical reform which I am endeavoring to advocate, I will try to elucidate and impress my doctrine by an illustration drawn from another art.

What new mode of investigation lies at the foundation of the modern reform in agriculture? Our fathers endeavored to ascertain what soils were adapted to the growth of particular vegetables, by making the experiment with the plant as one mass, in the soil as a mass, ignorant of the elements of either, and consequently of their mutual relation. The improved method, introduced by Liebig, is to determine, in the first instance, the elementary components of each; and then a very simple law of relation between the two groups enables the farmer to select the ground for a particular plant, or the plant for a particular field. Without such a change in the mode of experimentation, the art of agriculture must have remained almost as stationary as that of medicine, though not as defective.

To the progress of medicine a similar impulse has been given by Homœopathia, which attends *primarily* to the phenomenal *elements*, or properties which drugs exhibit in their *positive* effects on the healthy man; and next to the phenomenal elements of disease to be cured.

All this is a simple matter of observation, relating at first to individual phenomena. When the pathogenetic picture of the drug is completed by these individual strokes of the prover's pencil, he is then prepared to recognize its likeness to that of a case presented in his professional practice.

He then for the first time in the investigation, compares one *whole portrait* with another whole portrait — one assemblage of phenomena with another assemblage of phenomena.

But even in this stage of the process, he still retains the ad-

vantage of that certainty which attaches to the observation of elementary components. For like a true and cultivated artist, he not only perceives the general resemblance of two pictures, but the correspondence of the minutest particulars which conspire in the general effect.

This stage of the investigation, which in both its branches has been conducted under circumstances so little calculated to admit the intrusion of error, brings the homœopathic inquirer into the immediate neighborhood of the general law, *similia similibus curantur*. Its verification proceeds, *pari passu*, with the clinical experiments.

When, by the multitude of experiments by different observers, the evidence is overwhelming, and the truth of the law of cure established, it then exerts a reflex influence, and re-verifies the results of the preceding stages of the investigation. Thus the law of cure, the pathogenesis, and the doctrine of small doses, are continually affording mutual verifications.

For illustration, suppose that in the random experiments of an allœopathic society, a leaf of the vine *Rhus radicans* had cured a case of typhus fever, they would not from that result, obtain the slightest evidence, that the same leaf was capable of curing a sprain, a rheumatism, or any other disease except that one in which it had been successfully tried. The most they could even conjecture would be, that it might possibly be useful in some other malady extremely analogous to typhus. But a homœopathic society, having proved, on themselves when in health, a leaf from the same vine, and having thus developed symptoms similar to those of typhus fever, and being thus systematically led to its use in that malady, will even from their first cure, obtain some degree of confirmation of the law which had suggested its employment in this fever, and consequently some evidence of the adaptation of the same leaf to the cure of many other maladies, whose symptoms are extremely different from those of typhus fever, but very similar to certain groups of the symptoms of *Rhus*.

Again, would the cure of any imaginable number of cases of

typhus fever by *Rhus radicans*, induce, in an allœopathic practitioner, the faintest suspicion that *belladonna* would cure *scarlatina*? If unacquainted with the law of cure, he would regard any supposition of connexion between such facts, as ridiculous. His cure of one affection by one medicament, never contributes a particle to the probability of genuineness of an apparent cure of an entirely *different affection* by an entirely *different medicament*. He would consequently need to observe more than a thousand times as many recoveries under the use of his medicine, as the homœopathist would under his, before he would have equal evidence that any one of the recoveries was really due to the medication.

As each homœopathic cure tends to confirm the law of cure, and as this law has regulated the selection of hundreds of known remedies, for millions of varieties of cases, which, by different combinations of symptoms, have actually been presented to some thousands of homœopathic physicians, the evidence of the truth of the law is so irresistible, that no sane man of ordinary capacity, will remain sceptical, after having an adequate knowledge of the facts.

Equally irresistible as the proof of the law, is that of the efficiency of small doses. For those who successfully follow the guidance of the former, almost always avail themselves of the instrumentality of the latter. If in any art, there is an indispensable rule which prescribes the particular class of instruments, from which one must be selected in order that a particular work shall be successfully performed, and if the artist or artizan selects one of the smallest specimens, and by its instrumentality performs that definite work successfully, he, by that very act, demonstrates the efficiency of a tool of that particular magnitude, and consequently the sufficiency of the magnitude itself.

By a similar comparison, we may illustrate the verification of the special curative properties of the different articles of our materia medica. If a rule which is found to be indispensable to success in a certain mechanical art, requires for a certain work, a certain class of instruments, for example an auger instead of

a hammer, or a saw instead of a pitchfork, then if the work has been most successfully performed, the result affords evidence that an instrument of the right class has been employed. So in medicine; if the rule of similars requires for the cure of a given group of symptoms, the use of a medicine which can produce a similar group, and if we find such a group in our *materia medica*, under the head of *bryonia*, and the administration of an infinitesimal dose of it removes the disease, then we have a verification of the pathogenetic proving we obtain a confirmation of the *materia medica*, which attributes these properties to *bryonia*, as well as a confirmation of the efficiency of small doses.

Thus we see that the success of a homœopathic practitioner verifies all the great doctrines of his school. So do his failures. For example, if on any occasion he makes a prescription which, on farther study, he finds not in accordance with the *materia medica*, he anticipates a practical failure; and on revisiting his patient, finds the effect either absolutely null, or less favorable than he had on other occasions observed after prescriptions, which previously to any observation of their results, had been more satisfactory to himself, because they had been dictated by a closer study of the *materia medica*.

Such is the cumulative character of the evidence which sustains every branch of Homœopathia, and enables one of its practitioners to obtain an amount of reliable, therapeutic knowledge, and a degree of assurance, incomparably surpassing those which an adherent of any other system can ever realize.

I have endeavored to show, that the system of Hahnemann considered as a science of observation, presents, by the certainty of its methods of discovery and verification, such evidences, that his disciples have the privilege, not merely of believing, but of knowing its verity. They manifest their confidence in the system, by invoking for it the most severe and searching ordeal. They entreat physicians to make direct, simple and safe experiments; they petition legislatures to charter colleges, for giving instruction in its doctrines, and hospitals in which the practice

may be subjected to public observation and professional scrutiny, and its success demonstrated by ample statistics.

If facilities for these means of improving the medical profession, of relieving the afflicted, and of multiplying the external evidences of homœopathic truth, shall be afforded by the legislators who annually assemble in this city, and within these walls, for the purpose of advancing the interests of this great and influential member of the American confederacy, then they will, by such a course, give important aid to the diffusion of science, and of the inestimable blessings of health, and will at no distant day, receive the gratitude of their fellow citizens.

