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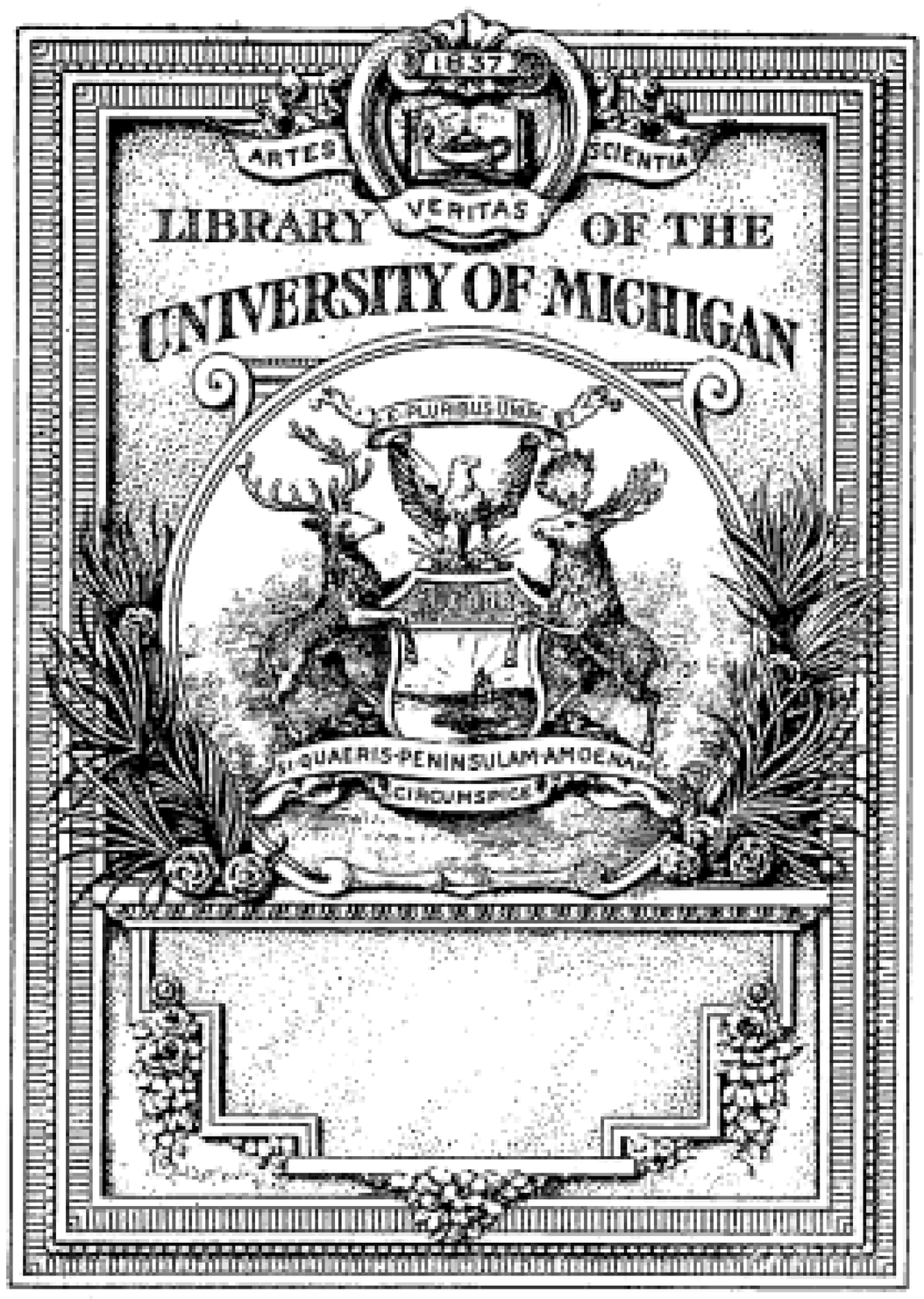

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INQUIRY

INTO THE

HOMŒOPATHIC PRACTICE

OF

MEDICINE.

BY

WILLIAM HENDERSON, M. D.,

PROFESSOR OF MEDICINE AND GENERAL PATHOLOGY, AND LATELY ONE OF THE PROFESSORS OF CLINICAL MEDICINE IN THE UNIVERSITY OF EDINBURGH.

NEW-YORK:

WM. RADDE, 322 BROADWAY.

ALSO: J. T. S. SMITH, 592 BROADWAY.

PHILADELPHIA: C. L. RADEMACHER.

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1846.

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(Fourth Volume, New Series)

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LETTER

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TO

JOHN FORBES, M.D., F.R.S.,

EDITOR OF THE "BRITISH & FOREIGN MEDICAL REVIEW,"

ON HIS ARTICLE ENTITLED

"HOMŒOPATHY, ALLOPATHY, AND YOUNG PHYSIC,"

CONTAINED IN THE NUMBER OF THE REVIEW FOR JANUARY, 1846.

BY

WILLIAM HENDERSON, M.D.,

PROFESSOR OF PATHOLOGY IN THE UNIVERSITY OF EDINBURG.

Extracted from the British Journal of Homœopathy, for April, 1846.

NEW-YORK:

WILLIAM RADDE, 322 BROADWAY.

ALSO,

J. T. S. SMITH, 592, BROADWAY.

BOSTON:

OTIS CLAPP, 12 SCHOOL-STREET.

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LETTER,

ETC.

Sir,—It is not the irritability of an author subjected to a rigorous criticism that prompts me to address to you the following remarks on your late review of Homœopathy, for I can say with sincerity that you have given me, personally, scarcely any ground for complaint. Indeed, both as an author and an adherent of the system which you have reviewed, I can justly pay you the compliment of stating that you are the first public opponent of Homœopathy in this country who has treated it with the courtesy of a gentleman, and the candour, if not of an unbiassed unbeliever, at least of one who does not wilfully assert what is untrue.

Nor is it solely on account of the importance of the omissions and mistakes you have made that I address you at present. Far greater than any you are chargeable with, and deliberate misrepresentations to boot, have been committed by some of your contemporaries, which the feebleness of their influence for either good or bad has rendered unworthy of notice. It is, however, otherwise with you, and the productions of your pen; and though I might, with little anxiety for the result, leave your article on Homœopathy to do the important work for which it is in many respects so well suited, without any comments of mine, it has occurred to me that the inaccuracies and defects to which I have referred may, under the sanction of your name, have more influence with many than they deserve to have, and may thereby retard the progress of an inquiry in which the profession and the public are very seriously concerned. I gladly avail myself, therefore, of the apology for my interference which is afforded by the circumstance of my having a place in your review, in order to supply some of the omissions, and correct the principal mistakes, of that article.

Though I give you full credit for having undertaken, and

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prosecuted, your examination of the subject with a desire to act fairly by it, I am far from admitting that you have succeeded in your object. While there is much in your paper that is just, and a little that will be regarded as even liberal, there is a great deal that is the reverse of both. Some of what comes under this latter designation is, no doubt, the result of imperfect information—of views which, as you acknowledge, have been "suddenly and prematurely" forced from you. A large account, however, remains that cannot be regarded in this light, but which affords some curious illustrations of the psychological infirmity that often leads men to exhibit doctrines which they dislike to as much disadvantage as they can, without absolutely affirming what they know to be untrue.

To this infirmity I must ascribe the suppressing of explanations that might lessen or remove an objection;—the ready admission of whatever appears likely to tell against your opponents; the prompt repudiation of everything like a presumption in their favour; and the recourse to denials or affirmations regarding points on which you are not entitled, by your actual knowledge, to offer an opinion.

Added to all this, there are so many misrepresentations of facts and doctrines, (so plainly stated by Homœopathic writers, that it is difficult to conceive how they can be misunderstood,) that it will be scarcely surprising should many, who do not know you personally, doubt the possibility of their being unintentional.

It is easy to perceive that you started on your inquiry with your mind fully made up on the more important merits of the case; and the following are clearly the "views relating to the general subject which have long occupied" your thoughts. You have been long satisfied that the treatment of diseases, according to the old system, was, for the most part, radically bad,—with some exceptions, simply *powerless* as to the cure of diseases, and in many, if not in most, of these exceptions, worse than powerless, positively injurious; you were familiar, therefore, with the belief that the majority of the supposed *cures* of diseases, including acute inflammations and other dangerous maladies, under the old system, were due to the *power of nature* acting independently or even in spite of the treatment; you had heard not a little of the success of Homœopathy, and the difficulty of conceiving that the means you supposed it to employ could act in any way on the body, suggested an explanation of this success, which chimed in with your estimate of the

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power of nature. The riddle was thus easily solved. The recoveries under the old system are mostly due to nature, *ergo*, the recoveries under Homœopathy can be due to nothing more.

In order to guard myself from misrepresenting you, I shall quote your own words. The inferences you specify as the result of your deliberations are :—

"1. That in a large proportion of the cases treated by Allopathic physicians, (that is, of the old school,) the disease is cured by nature, and not by them.

"2. That in a lesser, but still not a small proportion, the disease is cured by nature, in spite of them ; in other words, their interference opposing, instead of assisting, the cure.

"3. That, consequently, in a considerable proportion of diseases, it would fare as well, or better, with patients, in the actual condition of the medical art, as more generally practised, if all remedies, at least all active remedies, especially drugs, were abandoned. * * * * *

"Although Homœopathy has brought more signally into the common day-light this lamentable condition of medicine regarded as a practical art, it was one well known before to all philosophical and experienced physicians.

"It is, in truth, a fact of such magnitude,—one so palpably evident, that it was impossible for any careful reader of the history of medicine, or any long observer of the processes of disease, not to be aware of it. What, indeed, is the history of medicine but a history of perpetual changes in the opinions and practice of its professors, respecting the very same subjects—the nature and treatment of diseases ? And, amid all these changes, often extreme and directly opposed to one another, do we not find these very diseases, the subject of them, remaining (with some exceptions) still the same in their progress and general event ? Sometimes, no doubt, we observe changes in the character and event, obviously depending on the change in the treatment,—and, alas, as often for the worse as for the better ; but it holds good as a general rule, that, amid all the changes of the treatment, the proportion of cures and of deaths has remained nearly the same, or, at least, if it has varied, the variation has borne no fixed relation to the difference of treatment." (P. 257–8.)

"The foregoing elucidations, it will not be doubted, disclose a lamentable state of things ; but it is not a state to be despaired of ; much less is it one to be concealed as something disgraceful. It is more our misfortune than our fault

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that it is as it is; but if it were our fault, still it ought to be made known. There, as in morals, the more sensibly we feel our defects, the more openly and heartily we confess them, the more likely are we to get rid of them. As thus reflected in our critical mirror, the features of our ancient mother assuredly look somewhat unattractive. She seems neither happy nor prosperous; yea, she seems sick, very sick; yet not sick unto death. On the contrary, we believe that she is more vivacious and vigorous than at any preceding time; her countenance is merely 'sicklied o'er with the pale cast of thought,' from the strength of her inward throes; 'the genius and the mortal instruments are now in council, and her state, like to a little kingdom, is suffering the nature of an insurrection.' And such, in truth, do we believe to be, literally, the condition of physic at this moment. Things have arrived at such a pitch, that they cannot be worse. They must mend or end. We believe they will mend. The springs of life are yet untouched; the constitution retains its rallying power; the vis medicatrix is in action; and we flatter ourselves that there is yet enough of young blood and energy and wisdom in our ranks to redeem the past, and to achieve that glorious regeneration, which has been long announced by infallible signs and portents in these latter days. Old as we are, we yet hope to see raised the standard of 'Young Physic,' though we cannot expect to see it furled, after the destined victory is won."—P. 261.

So much for your estimate of the old system; one which has long occupied your thoughts, and which, I may be permitted to say, was so entirely mine also, that I thought it worth while to examine the pretensions of a new system.

Then, as to Homœopathy; in commenting on a general comparison of its success, with that of the old system, as shown in the tabulated results of Fleischmann's practice in the Homœopathic hospital of Vienna, and of several French and British hospitals, you say—

"The remarks above made are even of more importance, in relation to the general subject now under consideration, than they may seem to be at first. They not only show that the *kind* of successes and failures experienced by the Homœopathists, is precisely the same as that experienced by the Allopathists; but they also seem to show that the medication of the former can boast of no *peculiar* virtue whereby it can achieve triumphs in fields altogether forbidden to the latter. Under the influence of medicines, all of which must

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be considered *new*—new absolutely, or new in their form, mode of administration, and principle of action—we would have hardly expected the old relations of curability and incurability exactly preserved. Does not this fact, common to both, seem to point to a *community of power, or want of power*, in the two classes of agents, rather than to a speciality of action and potency in one?"—P. 244. And so determined are you to make out your point against the old system, as possessing little, if any, potency as a system of *curing*, that you behave very liberally (as your Allopathic friends will think) to the recorded successes of Homœopathy; but with the purpose of bringing both the old and new systems to the level of your power of nature.

"These tables, (Fleischmann's Homœopathic tables, for instance, substantiate this momentous fact, that all our ordinary curable diseases are cured in a fair proportion, under the Homœopathic method of treatment. Not merely dó we see thus cured all the slighter diseases, whether acute or chronic, which most men of experience know to be readily susceptible of cure under every variety of treatment, and under no treatment at all; but even all the severer and more dangerous diseases, which most physicians, of whatever school, have been accustomed to consider as not only needing the interposition of art to assist nature in bringing them to a favourable and speedy termination, but demanding the employment of prompt and strong measures to prevent a fatal issue in a considerable proportion of cases. And such is the nature of the premises, that there can hardly be any mistake as to the justness of the inference. Dr. Fleischmann 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 honour and respectability, and incapable of attesting a falsehood. We cannot, therefore, refuse to admit the accuracy of his statements as to matters of fact; or, at least, to admit them with that liberal subtraction from the favourable side of the equation, which is required in the case of all statements made by the disciples and advocates of new doctrines. Even after this rectification, we see enough that remains to justify the inference above deduced. No candid physician, looking at the original report, or at the small part of it which we have extracted, 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. The amount of deaths in the fevers and eruptive diseases is certainly below

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the ordinary proportion; but, for reasons already stated, no conclusion favourable to Homœopathy can be thence deduced. It seems, however, reasonable to infer that, even in these cases, the new practice was not less favourable to the cure than the ordinary practice. In all such cases, experienced physicians have been long aware that the results, as to mortality, are nearly the same under all varieties of Allopathic treatment. It would not surprise them, therefore, that a treatment like that of Homœopathy, which they may regard as perfectly negative, should be fully as successful as their own. But the results presented to us in the severer internal inflammations, are certainly not such as most practical physicians would have expected to be obtained under the exclusive administration of a thousandth, a millionth, or a billionth part* of a grain of phosphorus, every two, three, or five hours. It would be very unreasonable to believe that out of three hundred cases of pneumonia, two hundred and twenty-four cases of pleurisy, and one hundred and five cases of peritonitis, (in all six hundred and twenty-nine cases,) spread over a period of eight years, *all* the cases, except the fatal ones, (twenty-seven in number,) were slight, and such as would have seemed to us hardly requiring treatment of any kind. In fact, according to all experience, such could not be the case. But, independently of this *à priori* argument, we have sufficient evidence to prove that many of the cases of pneumonia, at least, were severe cases. A few of these cases are reported in detail by Dr. Fleischmann himself, and we have ourselves had the statement corroborated by the private testimony of a physician, (not a Homœopath,) who attended Dr. Fleischmann's wards for three months. This gentleman watched the course of several cases of pneumonia, and traced their progress by the physical signs, through the different stages of congestion, hepatization, and resolution, up to a perfect cure, within a period of time which would have appeared short under the most energetic treatment of Allopathy."—P. 243.

Again, in reference to the cases published in my treatise, you say of two "well-marked cases of acute rheumatism," and two of "severe neuralgia," that "it would be unfair to deny that the result obtained in these four last cases would have been regarded as very satisfactory under any mode of Allopathic treatment," p. 245; and of the cases generally, "we do not hesitate to declare, that the amount of success

* I shall by-and-by show that this account of the doses is altogether incorrect —W. H.

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obtained by Dr. Henderson in the treatment of his cases, would have been considered by ourselves as very satisfactory, had we been treating the same cases according to the rules of ordinary practice."—P. 250.

Now all these admissions have the appearance of fairness, and considering the manner in which the facts both of Fleischmann and myself have been misrepresented by uncandid reviewers, they will seem startling and extreme to most of your professional readers. Yet they are fair only in a degree,—only to the level of your hypothesis regarding the power of nature, and far short of the truth. A greater amount of success than the old system you will not admit Homœopathy to procure; you allow it to run neck and neck with the former in the treatment of some of the most dangerous inflammations even, but not a hair's breadth more. It must not pass the line of your preconceptions, let the "hard words, and harder figures of statistical tables" say what they may.

But the subject is far too important to be slurred over in a way so summary and inaccurate, and I, therefore, hope you will excuse me if I keep somewhat closer to the facts than your hypothesis finds it convenient to do. To get rid of the overwhelming evidence of the superiority of the Homœopathic practice, as shown on a comparison of Dr. Fleischmann's tables with similar tables of Allopathic physicians, you object to all the statistical tables that profess to exhibit the comparative results of treatment of any kind. The genius of the diseases at different seasons, the influence of the sex, age, and condition of the patients, are so many circumstances that seem to you to deprive the statistics, hitherto published, of value in such a comparison. And you are right to this extent, that we have as yet no statistical details sufficiently minute, or so carefully classified, as to enable us to determine to a fraction what is the amount of superiority which one kind of treatment possesses over another. But a degree of precision such as this is not necessary in the inquiry we have on hand. We want to know, simply, on which side, the Homœopathic or the Allopathic, the advantage lies, and not the exact amount of the advantage. And to settle this point there is an ample accumulation of sufficiently minute information to leave no room for doubt respecting it. In large collections of cases of any disease, the sex, age, and condition of the patient, and the date of the disease when brought under treatment, becomes so much equalized, that there is no danger, in comparing them,

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of drawing an erroneous inference as to the *general fact* of which group has been the most successfully treated—which treatment has been, on the whole, the best, and the most worthy of future confidence.

I conceive that no one will be inclined to dispute that three or four hundred cases of a particular disease, on each side, taken indiscriminately from both sexes, at all ages above infancy, at all periods of the disease according as the persons affected happened to present themselves for treatment, at all seasons, and during a series of years, present very fair grounds for ascertaining the comparative value of two kinds of practice. It is in the very nature of statistics, collected in such circumstances, and embracing so large a number of cases, to do away with accidental sources of error, and to bring out a general fact that might be misrepresented by more limited data. The comparisons I am about to give possess all these safeguards against mistake, and the proofs which they afford are as completely decisive in respect to the general fact of the superiority of the Homœopathic practice as any proofs we have in medicine on any point whatever.

Dr. Fleischmann treated in the Homœopathic Hospital of Vienna during the nine years beginning in 1834, and ending in 1843, two hundred and ninety-nine cases of inflammation of the lungs. Of these, nineteen died, or about one in sixteen.

With these cases I contrast, first, the experience of Chomel in the Hôtel Dieu of Paris. He does not, in the account from which my information is drawn, specify the number of cases that had fallen under his care; but when we consider the frequency of pneumonia in Paris, the size of the hospital in which he practised, and the length of time to which the account refers, we must admit that the number cannot have been less than that treated by Fleischmann. The period of ten years, from 1832 to 1842, during which Chomel's cases occurred, will be allowed to have been sufficiently extensive to have prevented any possibility of error from the variable genius of the disease, coinciding so nearly as it does, too, with the period of Fleischmann's practice. And in order to do away with the possibility of any prejudice to the comparison, in favour of Homœopathy, that might be conjectured to arise from the cases of Fleischmann having been accidentally of an age more favourable to recovery, than those of Chomel, I select from the statements of the latter, what he says of the mortality during that

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period of adult life, when the average success is the greatest, and contrast this with the results of Fleischmann's practice among persons of all ages. It appears, then, that between the ages of twenty and forty, Chomel had a mortality of *one in eight, or double that of Dr. Fleischmann's at all ages.* The Homœopathic physician does not mention the ages of his patients; but the table he has given proves that his hospital is, like other general hospitals, devoted to the reception of all kinds of disease; and judging from the nature of the diseases he specifies in his table, it is clear that the persons admitted were mostly past the period of adolescence. Thus, of the diseases the most common in early youth, scarlet fever, measles, hooping-cough, convulsions, general scrofula, varicella, only one hundred and eighty-seven cases were admitted in the nine years, while the whole number of patients was six thousand three hundred and twenty-two.

Again, if we compare the mortality of pneumonia under the ordinary treatment, within limits of age still more favourable to recovery, (for you know that the mortality is less as the age is earlier—that of early infancy excepted,) we still find it greater than that of Fleischmann at all ages. Barthez and Rilliet give us one hundred and sixteen cases between the ages of sixteen and thirty, and Leroux* one hundred and eighty-two cases between the ages of thirteen and thirty, including a period of seventeen years, in which the mortality is at the lowest, after the age of puberty, in the ordinary practice; and thus we have almost exactly the same number of cases as Fleischmann adduces, and collected, too, in various years and seasons. The mortality was more than one in twelve, or one-fourth greater than that of the Homœopathic practice. All that can be said or imagined, by any experienced and reflecting person, of the sources of fallacy in statistics, cannot subvert the conclusion from these facts,—that the Homœopathic treatment of inflammation of the lungs is vastly superior to the ordinary treatment. The exact amount of its superiority may not be such as these facts represent; it may be greater or it may be less, but that is of no consequence to the present inquiry. What we want to determine, I repeat, is simply which practice is the most successful, and not the precise amount of the difference; and yet it is only to this latter, and, in a practical point of view, altogether secondary and insignificant consideration, that your objections to statistics actually apply.

* Grisolle.

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What makes the comparisons I have made still more important and conclusive is, that the mortality of pneumonia increases after the age of forty, so that from forty to sixty, during Chomel's ten years, it was one in five; and above sixty, one in two. This progressive increase in the mortality with increasing years, appears, according to M. Grisolle, the most learned writer on the subject, "to be the same in all countries, in hospitals as well as in private practice."—P. 520. And yet, with all the disadvantage of a comparison of cases, in which a portion must have belonged to periods of life at which pneumonia increases in danger and severity, with others drawn from a period when it shows much less of a dangerous tendency, the success of Homœopathy is indisputably greater than the ordinary practice.

In order to convey some idea of the superiority of the Homœopathic over the ordinary treatment of pneumonia, at all ages, I adduce the following accounts, of which the Allopathic facts are derived from Grisolle's work, and the Homœopathic from the statistical tables of Fleischmann and Reiss, carried down to 1844. On the Allopathic side we have Louis, Trousseau, Grisolle, Laennec, Bouillaud, all eminent practitioners of a country whose medical practice you hold up to the emulation of British physicians. Together, they furnish five hundred and thirty-one cases of inflamed lungs; of which eighty-one died, *or one in six and two-thirds*. In this number there are included fifty-seven cases by Laennec, of which he says only two died. He is accused of having overstated his success, having confessedly given his account only from memory. And it is affirmed, on the authority of one who attended his wards, that his loss was actually much greater than he has allowed. Still, I have taken his own account of the matter.

On the Homœopathic side we have, in addition to the two hundred and ninety-nine cases of Fleischmann already noticed, forty-four treated by him in 1844; and thirty-four cases of Dr. Reiss, treated in the Homœopathic Hospital of Linz* in 1843 and 1844—the only cases of pneumonia, treated in that hospital, of which an account has been published. In all, then, three hundred and seventy-nine cases, and nineteen deaths, or one in twenty—the last forty-four of Fleischmann, and the thirty-four of Reiss, having all recovered!

It is well known that among females the rate of mortality from inflammation of the lungs is higher than among males,

* Oesterreichische Zeitschrift für Homœopathie.

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though females are much less liable to the disease. The number of the latter that occurred among the cases of Fleischmann and Reiss is not mentioned. But it is certain that the accommodation for females in Fleischmann's hospital is such, that no cases of pneumonia among them were more likely to be excluded than among the males; and there is no reason to suppose that females are less liable to pneumonia in Vienna than in Paris. In the account given of a small number of cases treated by Skoda in an Allopathic hospital in Vienna, the proportion of females is noted—there were nine, to twenty males. Among the five hundred and thirty-one cases that occurred in Paris, the proportion of the sexes is stated by some of the physicians; and we find that of three hundred and fifty-one cases thus classified, only seventy were females; so that no considerable proportion of the immense general mortality could have been due to that source.

It does not appear that any deduction should be made from the success on the Homœopathic side, on the ground that the statements come from the partisans of a particular system, which ought not to be equally made from the alleged success on the other side; for the several physicians, on whose authority the latter is given, were contending, some of them acrimoniously, for the superiority of their respective measures of treatment.

It is curious that while you eschew "any close comparison" of Fleischmann's tables with those of Allopathic physicians, on the ground of an absence of sufficient detail, no statement that can tell in favour of your own views, is too vague and meagre to be admitted in evidence. One might almost suspect, therefore, that, had a close comparison been likely to show Homœopathy to disadvantage, it would not have been so scrupulously avoided. You quote Grisolle's account of the expectant practice, or no practice, of Biett and Magendie, with the purpose of putting that on a footing with Homœopathy in its claims to suffrage. And yet, while the Homœopathic records are distinct as to the number of cases that were treated, and the mortality among them, in hard statistical figures, neither the one nor the other is mentioned of the cases that were left by those physicians to take their own course. Biett's mortality is said, merely, to have been "very inconsiderable" during the year that he treated his patients with only emollient drinks and cataplasms. Magendie's is *not mentioned at all;* the only information we have regarding his treatment of pneumonia being, that "he

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employs *no* other treatment" than the expectant.* "Very inconsiderable," with Grisolle, would doubtless mean one death in every five, or four, or perhaps three, cases, seeing that his own practice, which he stoutly defends against M. Bouillaud, furnished no less than one in six and one-third. The "very inconsiderable" was clearly meant to be coupled with "considering that the cases were left to nature." His own mortality was very considerable beyond all dispute; and if he meant to say that Biett's was, in the plain, absolute, and unqualified sense of the words, very inconsiderable, he was bound to adopt, and to recommend it to others, instead of laying down rules for the use of blood-letting, and tartar emetic.

Equally vague and unsatisfactory is the statement you make on your own authority:—"We may add that, to our knowledge, the same plan has been followed in one, at least, of the large hospitals of Germany, and the result was considered to have been far from unsatisfactory."—P. 246. This abandonment of the old practice, in favour of none at all, is scarcely the most obvious tribute to its efficiency. In this we agree; and as to the German result, to which you refer in such *very precise terms*—so much preferable to hard statistical tables—it amounted, we may suppose, to the loss of only a third, or a fourth, of the cases; a result certainly "far from unsatisfactory," as the consequence of no treatment at all, when even *active* treatment loses between a sixth and a seventh, or, if I may adduce the experience in one at least of the larger hospitals of Britain, *a third!* How would you have dealt with the luckless Homœopath who should attempt to encounter the statistics of Allopathy, with such miserable statements as you oppose to those of Dr. Fleischmann?

Although I have said enough to satisfy any unprejudiced and intelligent person that the Homœopathic practice in pneumonia is very much more successful than the Allopathic, I cannot quit the subject without affirming, that you

* There is a small mistake in your account of this. Grisolle uses the word "*guère*," which is not *no*, but *scarcely*. Even Magendie, notoriously bold and unfeeling as he is, dared not habitually to give up all treatment. The mistake, however, favours your side of the argument. For it would be a presumption, and you employ it as such, in favour of *no treatment*, and in the same measure against the claims of Homœopathy to be *some* treatment, if any physician saw good reason to relinquish the employment of medicine in the treatment of pneumonia. But even Magendie did use some remedies, though apparently neither very active, nor *very successful*; for your Dublin contemporary for February, who appears to speak from personal knowledge, affirms that the mortality was held by lookers-on to be greater under the distinguished physiologist than under his colleagues, *and therefore much greater than under Homœopathy*.

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give the latter less than its due, small as that may be. Grisolle, as you know, left eleven *slight** cases of pneumonia to take their own way undisturbed by treatment, and he gives an account of the time during which the characteristic expectoration, pain, and fever continued, and of the period at which the phenomena of auscultation began to decline, and when they disappeared. He does the same in reference to the cases that were treated by blood-letting, and tartar emetic, and affirms, justly, that the latter were convalescent sooner than the former.

The details are to the following effect: 1. In the eleven left to nature, the pain *did not cease in a single case before the seventh day;* in several it lasted till the 20th, 25th, and 27th days; *the mean was fifteen days.* In four he was forced to have recourse to cupping, owing to the persistence of the pain, and one of them required a blister in addition. [He helped the power of nature a little, after all.]

In the cases that were bled, (two hundred and thirty-two in all,) the *mean duration of the pain was seven days.*

In those that were treated with tartar emetic alone, (forty-four in number,) he does not mention the mean duration of the symptom. But he says, "the first sign of amendment consisted in a diminution, and sometimes a total cessation of the pain, which was often very pungent and acute." In five

* In alluding to them, you say, "Dr. Henderson misjudges these cases in terming them 'slight,' in comparison with the one treated by him. They seem to have been fully as severe." P. 246. I persist, notwithstanding, in calling them slight, unquestionably slight, cases. For Grisolle not only says that the general symptoms were mild enough to satisfy him that he might leave them to themselves without danger, but he says that the inflammation was "*of small extent*" in all of them. Why did you not notice this most essential particular? If you had, you could not have added that they were "fully as severe" as the case of mine to which you allude. That case is stated to have had the lung condensed "as high as the spine of the scapula," and from "the axilla all down the lateral aspect of the side"—about two-thirds, at least, of the whole lung. No small extent truly. In what other respects they were as severe, neither you nor I have any means of knowing. Grisolle himself, the only authority on the subject, says nothing of the frequency of the pulse, or of the respirations, of the state of the mental faculties, or of the state of quiet or restlessness—the very point on which, much more than on any local signs, an opinion of the severity of a case of pneumonia ought to rest. But you take it for granted that his cases had delirium—pulses above 120, respirations 48, and much restlessness night and day! All of these symptoms existed in my case, and must have existed in the eleven if they were as severe. No experienced physician can maintain that the mere fact of the disease having reached "the stage of red hepatization" is a proof that it was severe. A small extent of hepatization, and mild general symptoms, constitute slight cases of pneumonia if the disease can ever be slight. I mention these particulars only to show how strangely you depreciate what is Homœopathic, and magnify beyond all warrantable compass what may seem to bolster up your hypothesis.

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cases of the latter sort, it had *completely ceased after the first day of treatment.*

2. In the eleven, the mean duration of the characteristic expectoration was ten days.

In fifty cases bled in the first stage, the mean was seven days. In one hundred and eighty-two bled in the second stage, it was nine days.

In thirty-five cases treated with tartar emetic, the state of this symptom was noted; and in seventeen it existed in the highest degree; and in them the expectoration was rendered more or less colourless in twenty-four hours after the treatment was commenced.

In the eleven, the phenomena of auscultation did not begin to decrease *till the end of the second week*, and persisted still in various degrees till between the twenty-second and thirtieth days.

In the cases bled within the first four days of the disease, the phenomena began to decrease *between the sixth and seventh days.* In those bled later, the mean of the commencement of the decrease was the tenth day.

In the forty-four cases treated by tartar emetic, the phenomena began to decrease in thirty-six, *between the end of the first and the fourth days of the treatment.*

So much for the course of *slight* cases left almost to nature, compared with that of severe cases treated by the ordinary means. I think that you will feel yourself in a dilemma when I present to you the plain inferences deducible from those facts, and your account of the course and duration of cases treated Homœopathically. You admit, on the competent testimony of a physician, (not a Homœopath,) who attended Dr. Fleischmann's wards for three months, and watchod the progress of several cases of pneumonia, through the different stages of congestion, hepatization, and resolution, up to a perfect cure, that this result occurred "within a period of time which would have appeared short upon the most energetic treatment of Allopathy."—P. 243. Now, the energetic treatment of Allopathy appears, beyond all question, as Grisolle's narrative proves, to cure pneumonia within a period of time much shorter than the power of unassisted nature can do. What is then the demonstration which follows? Let us see if you have not given us a fine specimen of the *reductio ad absurdum.*

Energetic treatment cures pneumonia much sooner than no treatment at all. Homœopathy cures pneumonia in as short a time as energetic treatment; *ergo*, Homœopathy

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cures pneumonia only as soon as no treatment at all! *Quod est absurdum.* I am the more astounded at this result of your argument, because the very facts which I have quoted from Grisolle, in proof of the advantage of the ordinary treatment over *no* treatment, are contained in a previous number of your own review.

You have been unfortunate in not having considered attentively the means which Dr. Fleischmann used in the course of his remarkable success in inflammation of the lungs. Had you done so, you might have avoided the blunder which I have now exposed, for you would have discovered that the solutions of phosphorus, which formed his principal remedy, contained a notable quantity of that very active substance. If you had consulted Fleischmann's notice of his practice contained in *The British Journal of Homœopathy*, No. 5, you would have found that his *first* attenuation of phosphorus contains, in every hundred drops, a grain of the drug; in the *second*, nearly one-tenth of a grain; and in the *third*, about one-hundredth. These are the attenuations used by Homœopathists in severe cases of pneumonia, as you will perceive from the cases detailed by Fleischmann and myself. Now, if you had considered that the dose of this medicine, when given after the rules of your own school, is only about the fiftieth of a grain—or only double the dose of the first Homœopathic attenuation—you could hardly have concluded that the Homœopathic treatment of pneumonia is incapable of producing any positive effect. You would have perceived that it is not by the millionth, or the billionth of a grain of phosphorus that the Homœopathists claim the credit of curing pneumonia so much more successfully than others. And I may here remark, in reference to acute diseases, that the lower or stronger attenuations of the medicines are those almost universally used, a circumstance which you do not seem to have known when you made the general remark at p. 229, that "the primary dilutions, or attenuations, are used comparatively rarely," otherwise you would, doubtless, have made an exception in respect to inflammations, and thus have silenced the clamour of these opponents who labour to frighten the public out of its propriety, by representations founded alike on malice and ignorance. Whatever may be thought of the higher attenuations and their alleged value, in chronic diseases, there can be no doubt, in the mind of any rational man, as to the lower, both actually containing a very sensible quantity of medicine, and being capable of produc-

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ing a sensible effect, even though experience had not abundantly proved that they do.

I feel the more desirous of removing misconception in the minds of professional opponents on this part of the subject, because it is in the treatment of acute inflammations that Homœopathy possesses the most momentous advantage over the ordinary practice. I have already dwelt on this superiority, as shown in the case of inflammation of the lungs. But the tables of Fleischmann and Reiss show an equally remarkable superiority in regard to other inflammatory diseases. From these we learn, that of two hundred and fifty-eight cases of erysipelas, chiefly of the face too, only two died; of one hundred and twenty-six cases of peritonitis, only six; of forty-five cases of inflammation of the membranes of the heart, not one died; of two hundred and forty-eight cases of pleurisy, only three. I need not institute a detailed comparison of such results, with the tables furnished from Allopathic hospitals. You know that this is done in the excellent Introduction to Homœopathy, edited by Drs. Drysdale and Russell; and you know, also, the vast superiority which the comparison exhibits in favour of the Homœopathic practice. I may further observe on this most important point, that it would be strange indeed, contrary to every principle of probability, that the circumstances you mention as liable to vitiate the inferences that may be drawn from a comparison of the kind, should, in so many separate instances, be *accidentally* so entirely in favour of Homœopathy.

Although I have commented with some rigor on a few of the errors of narration, and of inference, which disfigure your allusions to Homœopathy as claiming the credit of a mastery over acute diseases, which no other plan of treatment possesses, I sincerely believe, that if you had entertained but a suspicion of the truth, you would have honestly and manfully avowed it. I am far from supposing that the man who has had the courage and candour to proclaim to the world the unsoundness of the ordinary practice; the necessity of a thorough *regeneration* of practical medicine; that "things (in Allopathic physic) have arrived at such a pitch, that they cannot be worse,"—and that "they must mend or end;"—I am far from thinking that he who could utter such truths as these, so unpalatable to the general taste, so truly *Hahnemannic*, would hesitate to declare that the claims of Homœopathy were just, if he only knew enough of the subject to qualify him to decide. You have borne a testimony

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to the character and genius of Hahnemann, and to the ability and good faith of many of his followers, that must satisfy your readers that you would scorn to rank among those who are filching his discoveries, and arraying themselves in his hard-earned honours. And if, in your tribute to him, you have stopped short of his greatest merits, and have failed to give him credit for having effected any *positive* good in the practice of medicine, the defect must be ascribed to the limited extent of your acquaintance with his labours, rather than to any unkindly feeling towards his memory. The length you have gone, however, is new in this country among the adherents of the ancient school, and gives you a claim to such marked commendations as are usually paid to justice and generosity when they are scarce.

These virtues appear to flourish better among our professional brethren in Germany, so that it seems to confer no particular claim to distinction among them, that a man should give honour to whom honour is due. They are generally much better informed also than our English physicians are, and, therefore, if the propensity to pilfer were exercised by any of their number, he would be sure of being speedily detected and pilloried for his crime. Hence it is, that while in this country one of the most valuable discoveries of Hahnemann's method of ascertaining the curative properties of medicines, has been stolen from Homœopathy, without a word of acknowledgment to indicate the source from which it was originally taken, his more just and candid countrymen, while they take advantage of his labours, award the discoverer the encomiums to which he is entitled. "It was Hahnemann," says Professor Maly, of Gratz, "who first recommended the use of *Aconite* in pure inflammatory fevers, with or without eruption, as well as in inflammatory diseases generally, in obedience to his principle, *similia similibus*, by which the effusion of blood, except in certain exceptional cases, is wholly obviated. *Even were we under no other obligation to Hahnemann, by this simple discovery, he would, like Jenner, deserve to be ranked among the greatest benefactors of suffering humanity.*" Professor Maly teaches Materia Medica in an established university—is no Homœopath, in the technical sense of the term—publishes his series of observations on the subjects of his professorship in an Allopathic periodical, and yet seeks no warrant in these circumstances for plagiarism and injustice. It would be well if others would follow the honourable example in regard to this, and the other discoveries, of the same illustrious man, with

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which they are enriching their monographs and journals without once mentioning his name.

While on this subject it may not be disagreeable to you to be informed of a few other particulars of the homage that is paid on the continent to the value of Hahnemann's contributions to the Materia Medica, and they will, doubtless, receive the more favour with you that they are not furnished by those who enrol themselves under his standard.

The same Professor (Maly) observes, of the Helleborus Niger, after commending its use in dropsies of various kinds, and other diseases, that "Hahnemann's proving of the medicine upon those in health, will be found the best guide" to a knowledge of what it is capable of accomplishing.

Of Pulsatilla he says, "The healing power of this medicine in rheumatic complaints, acute as well as chronic diseases of the eye, and the various affections complicated with derangement of the catamenia, &c., is taught *in the experience collected to so large an amount in the Homœopathic writings.*"

Another writer in an Allopathic journal for 1845, Dr. Popper, of Winterberg, eulogizes the use of Belladonna in inflammation of the throat, and acknowledges that he was indebted for his acquaintance with it to "the numerous indisputable testimonies of many intelligent and experienced Homœopathic physicians," and concludes in the following words:—"A more frequent employment of this medicine, in many diseases, is to be recommended to the use of impartial physicians; and the best source of information upon its virtues is the Materia Medica of Hahnemann, and the writings of liberal Homœopathists."

I give these as samples only of the general estimation in which Hahnemann is held by those who do not rank among his followers, who cannot be suspected of a spirit of partisanship, but possess honesty and information, and are not enslaved by prejudice. Similar testimonies might be easily multiplied, but I leave the consideration of those acknowledgments which have been made of the importance of Hahnemann's contributions to the *details* of the Materia Medica, in order to notice, what is more cheering still, because pregnant with the future recognition of all the valuable parts of his system,—the acknowledgments of the excellence of some of his fundamental principles.

At the Scientific Congress held at Strasburg in 1842, the Medical Section, with Professor Forget at its head, passed the following resolution:—"The Medical Section is unani-

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mously of opinion, that experiments with medicines on healthy individuals are, in the present state of medical science, of urgent necessity for physiology and therapeutics. * * *"†

Dr. Siebert,‡ an Allopathic writer in an Allopathic journal for 1823, observes, "It is not to be doubted that the complaints so loudly made, for some time past, in regard to the want of a foundation for therapeutics, have produced a beneficial effect in two ways: the first is negative, consisting in greater scepticism in the existing Materia Medica; and the other is positive, *being the proving of medicines on persons in health*, and more accurate experiments with them in disease. * * * To outward appearances, Homœopathy stands as much opposed to the old regime as ever; but I do not believe it does so in reality. Under the impulse given by this doctrine, medical science continues to direct more attention to the effects of medicines upon the healthy animal frame; while on the other hand, Homœopathists are every day directing more and more attention to the physiological aspects of diseases which they had before much neglected."

In the *British and Foreign Medical Review* for January, 1846, the learned editor, Dr. Forbes, among the best expedients for bringing his art out of its present deplorable position, recommends the future cultivators of it "to re-consider and study afresh the *physiological* and curative *effects of all our therapeutic* agents, with the view to obtain more positive results than we now possess," and "to endeavour to substitute for the monstrous system of Polypharmacy now universally prevalent, [in the old school, W.H.] one that is, at least, vastly more simple, more intelligible, more agreeable, and, it may be hoped, one more rational, more scientific, more certain, and more beneficial."

Professor Maly, of Gratz, already mentioned, urges the exhibition of medicines one at a time. Dr. Siebert, too, advocates the greatest possible simplification of the number and form of drugs in prescribing.

Now in these, and similar advices from various Allopathic authorities, and which have been partly carried into practical effect, though to a very small extent, by Allopathic physicians, both in America and Europe, a very satisfactory testimony is given to two of Hahnemann's fundamental principles, which he thus expressed, whilst those who now echo his words were enjoying the polypharmics of the nursery :—

† British Journal of Homœopathy. January, 1846. ‡ Ditto.

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"There is no way more certain, or more natural, for finding infallibly the proper effects of medicines on man than to try them separately, and in moderate doses, on healthy persons, and to note the changes which result from them in the physical and moral condition."—P. 194.*

"It will never enter the mind (of the true physician) to give as a remedy more than a single simple medicine at a time."—P. 280.†

I have said that the adoption of these principles of Homœopathy is fraught with the future recognition of the most valuable parts of Hahnemann's system. And, first, for this reason, that the *proving* of medicines on healthy persons will convince medical men of the accuracy of Hahnemann's experiments, and thus effectually silence the objections which have been drawn from the supposed impossibility of such medicinal symptoms as he describes ever having been produced. If the new *provers* of your "Young Physic" proceed courageously and skilfully in their work, this cannot fail to be one result of their labours and sufferings. The transactions of the Homœopathic Society of Vienna abundantly warrant the anticipation. The members of that body have begun to subject the Materia Medica of Hahnemann to a rigid experimental scrutiny, and as their mode of proceeding is worthy of being followed as an example, I transcribe this short account of it, and its bearings on the credibility of Hahnemann.

"The members meet, and to each is given a portion of the medicine to be experimented with, without telling him what that medicine is. At home they take this medicine in various doses, and write down all the effects they have observed; they then meet again, and each reads over the symptoms it has produced on him. Thus, there is obtained a series of testimonies from well-qualified and independent observers. They have found that the general results of Hahnemann's provings are perfectly accurate, and have expressed their admiration of his skill as an experimenter, and faithful describer of his experiments."—*British Journal of Homœopathy*, p. 8, January, 1846.

You who have never *proved* a medicine, I presume, oppose your notions of how medicines ought to behave themselves when taken by a person who does not need them, to

* Exposition de la Doctrine Médicale Homœopathique, 2nd edition. Paris, 1834.

† Ibid.

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the deliberate and oft-repeated experiments of Hahnemann and his friends.

"No unprejudiced person," you affirm, "who examines these records ever so superficially, can for a moment believe that one-half, or one-tenth of the symptoms recorded, were, or could be, produced by the medicaments swallowed."—P. 234. Then Hahnemann and his friends have told falsehoods regarding the more severe symptoms, and recorded many that were trivial and accidental.

I will not accuse you of making the imputation of falsehood, for you have already allowed the integrity of Hahnemann. But that Hahnemann *did* err in recording trivial occurrences among the symptoms that followed the taking of the medicines, no Homœopathist denies; nay, the provers in Vienna, who "have expressed their admiration," &c., proclaim the fact, and reject many of these symptoms. But does his error in the smallest degree effect the practical use of his provings—supposing, for a moment, the Homœopathic principle to be correct, that regulates the selection of a remedy? No, certainly. That principle requires that the symptoms of the disease to be treated, should find in the provings of the remedy, phenomena that correspond with them—with *all* of them if possible, with the chief and most characteristic of them at least. It matters nothing that there should be in the *proving* many more, truly medicinal, phenomena than there are symptoms in the disease; and, of course, it matters as little that there should be as many more trivial jottings, that neither correspond with the disease, nor are due to the medicine. Hahnemann himself anticipated your objection, but he thought it best to err on the safe side,—to note down phenomena that might be accidental and unimportant, rather than run the risk of excluding what might be of consequence. If the line must be drawn nicely between the genuine and the false phenomena, who was to decide on the precise qualifications that entitled a symptom to be retained, or marked it for oblivion? Surely not the first prover,—nor the first few provers. It must be the prerogative of the many, who, having summed up their own experiments and those of their predecessors, thus ascertain what bears the characters of constancy and genuineness, and what seems to be inconstant and accidental. The risk in all provings is rather from genuine symptoms being excluded, than from accidental ones being admitted. At least the Homœopathist feels so, who knows of what importance sometimes are seemingly inconsiderable particulars.

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With all the exuberance of Hahnemann's details, the case is not nearly so incredible as you make it appear. You ridicule the idea of one thousand and ninety symptoms being producible by one medicine. And yet a very little attention to the proving of Calcarea, the medicine you specify, will show you how unfair the inference is that you allow to be drawn by the bare transcription of the numerals which stand at the close of the list. The fact is, that scarcely one-tenth of the number consists of distinct and separate symptoms, (true or false.) For example, the first nine figures, (the seventh excepted) relate strictly to only *one* symptom. And this is multiplied into what appear to be eight to one who does not read the sentences corresponding to the figures, by the degrees of the symptom (vertigo) at different times of the day being separately noted, and by the circumstances of its being present in the open air, on walking and sitting, on moving and lying still, being also noted and numbered separately. In the same way, for the purposes of distinctness. and easy reference, are all the other symptoms split up, as it were, and numbered. This was Hahnemann's method with all his provings, and you perceive how small a degree of explanation deprives your objection of its weight, and how little attention was necessary on this, as on other occasions, to save you the uneasiness of a misrepresentation.

But your objections to the provings of Hahnemann extend to other particulars besides the number of the symptoms. You object to the *nature* of the symptoms also, and lay special emphasis on the absurdity of including *surgical* diseases among them. As I do not know where you fix the disputed boundaries of surgery and medicine, I may not be qualified to feel all the surprise which appears to have pervaded your mind, when somebody gave you to understand that many surgical diseases are recorded among Hahnemann's provings. Possibly visions of compound fractures, concussions of the brain, popliteal aneurisms, and carcinomatous tumours, were called up by the intelligence. If so, I can quiet your concern on the subject, by assuring you, that neither Hahnemann nor any of his assistants went to so great a length in their devotedness to science, as to incur the risk of such serious consequences; and that they do not any where allege that they ever experienced them. There is a little work which you do not seem to have read, called "A Defence of Hahnemann and his Doctrines," &c., that exposes the source of your error. Allow me to refer you to it for information on this point, and on many of the same kind,

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that I may be spared the tedious task of correcting so many errors of detail.

It is barely possible that you may consider every disease that is treated by a *surgeon* as a *surgical* disease. If that be your definition of the term, although it is somewhat of the oddest, we shall, no doubt, agree that many surgical diseases are mentioned among the provings. Surgeons treat erysipelas—belladonna produces it; surgeons treat boils—pulsatilla produces them; surgeons treat ophthalmia—aconite, belladonna, &c., produce it; surgeons treat cystitis—cantharides produce it; surgeons treat caries—mercury produces it; surgeons treat psoriasis—arsenic produces it; and so on with twenty other disorders common to *surgeons* and the *provings*. You deny all this, but Hahnemann, and his company of provers, aver it, and (you will pardon, in so important a discussion, my plain speaking) they were far better entitled to know.

But you deny this also: "Not a shadow of *proof* exists that the symptoms were the consequence or direct effect of the medicine; while a thousand reasons can be adduced for supposing the contrary."—P. 234. What sort of proof would satisfy "philosophers, and hard-headed sceptics like ourselves," it is not for me to say. Philosophers are not always the wisest men in the world. One endeavoured to prove that there was no such thing as motion; another, that there was no difference between right and wrong; a third, the father of a philosophy that finds its disciples in modern times, that we should give no credit to our senses, and so sincerely did he act upon his principles, that "if a cart run against him, or a dog attacked him, or if he came upon a precipice, he would not stir a foot to avoid the danger. He had friends, however, who, happily for him, were not such great sceptics, and took care to keep him out of harm's way; so that he lived till he was ninety years old." Again: "hard-headed," or "unlimited scepticism," as Dugald Stewart has it, "is as great a proof of imbecility as implicit credulity is." Philosophers and sceptics may carry their principles too far, it would seem; and if they wanted more proof of the source of their sufferings (in case they should take to the proving of medicines on their own bodies) than what I am about to specify, would stand very much in need of the "care of their friends."

Suppose some half-dozen men, who had a certain confidence in the evidence of their senses, to set about *proving* the effects of a particular medicine, on their own persons, they

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being at the time in health, and, on the whole, accustomed to enjoy a tolerable share of bodily comfort. And suppose, further, that they took especial care to avoid all irregularities in regimen while their provings were going on. Well, one of them finds that in a quarter of an hour or so after swallowing a dose, say of aconite, "giddiness and headache" come on. Has he reason to conclude that the aconite was the cause? Possibly not. He had no giddiness or headache for many a long day before—but let that pass; they may have been accidental. As soon as he is well again, or some days after, he takes another dose, and in ten minutes he finds his giddiness and headache return. On comparing notes with his colleagues, he finds that the five other have all experienced, at one or more trials, something of the same sort in various degrees and combinations. Is he to believe his own senses, and the concurrent experience of others, or, like Pyrrho the Elean, to discard all such fallacies, and, unless the care of his friends prevent him, swallow the whole bottle of poison to vindicate his principles, and show his contempt for common sense!

Again, though naturally possessing a good digestion, and a peaceable stomach, he discovers that very soon after a dose of this pernicious aconite, he feels a very inconvenient disgust for his victuals, or such qualms as threaten to end in something worse, and sometimes actually do so, or is tormented with pains in his entrails, or his liver. He repeats the experiment again and again; asks his comrades how they felt after their doses, and consults old authors concerning their experience and observation on the subject; and after all his researches and trials, he finds that there is a remarkable concurrence of evidence that those who knew no abdominal ailments for weeks or months before, did, after every dose of that particular potion—some sooner, some later—undergo afflictions of various kinds;—some nausea; some nausea and vomiting; some pain, pressive shooting, or constrictive; some diarrhœa; some vomiting and diarrhœa; some mere regurgitations; some vomiting of blood, some of bile. And he finds, besides, in himself and others, that, after the interval of a day or two from the use of the medicine, he and they eat, and relish, and digest their food as well as ever. He thinks all this affords some ground for believing, on the principles of common sense, that aconite produces certain serious evils in the digestive organs. He notes down his own sensations and doings under its influence, as they occurred at different periods after the several doses he had taken—as they happened to be solitary

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or combined—or as they varied in character and duration ; he notes down all he can learn from his friends of the same kind, or gather from other credible authorities ; and he numbers them separately to keep them distinct, though they are sometimes the same, sometimes but little different from one another, and so the list becomes long. In the same way, tedious and toilsome, he gathers a list of sufferings experienced, if any, in every region of the body, and he is very precise, and very anxious to be correct, all the symptoms—their diversities, and degrees, and shades of difference or of sameness—are classified and numbered : and the last number of the last shade turns up five hundred and forty. He might have omitted some repetitions, and some trifling differences, and some trifling sensations—but he is precise, and he puts them down ; he may have felt certain of them often before independently of medicine ; at all events he feels them now, and their presence can do no harm.

Many years after, a number of men, some twenty or so, anxious to prove this medicine over again, take dose after dose, on numerous separate occasions, and their experiments corroborate all the principal details of the original proving, and add some considerable items to the number. They, too, are healthy men, accustomed to no such aches and pains as they experience while taking the physic ; and they too, on the principles of common sense, refer their sufferings to the same cause, and in their simplicity never consider that "a thousand reasons can be adduced for supposing the contrary to be the fact ! !" Two or three good reasons will satisfy them entirely, I have no doubt, and when they are favoured with these, they will take to aconite afterwards as kindly as goats to milkthistle, or pigs to henbane !

Besides these objections to the provings in general, you single out some substances as peculiarly liable to be considered utterly incompetent to produce any symptoms at all. Thus you say—"When we find the Homœopathist maintaining that substances utterly powerless in a state of sensible bulk, even in the greatest amount, acquire astonishing powers by mere subdivision, without any discoverable change in their physical or chemical properties,—can any proposition be submitted to human apprehension that seems more utterly impossible—more ludicrously absurd ?"—P. 235. And you ridicule the idea that the decillionth of a grain of such substances (charcoal and carbonate of lime,) can produce any symptoms. But neither Hahnemann nor any one else ever affirmed that the decillionth of a grain of charcoal or car-

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bonate of lime is capable of doing any thing of the sort. Hahnemann, in reference to substances commonly esteemed inert, while he maintains that it is only after many triturations that they acquire any power of acting on the system, says, that in experimenting with them on the healthy body, the high trituration selected for the purpose, must be taken dose after dose in increasing quantities, and for many days, until their effects become sensible. That they do produce sensible effects when taken in this manner, is substantiated on the same grounds as those which have, I trust, rendered the provings of aconite abundantly credible to any one but a disciple of Pyrrho. If it be not, either Hahnemann could not "have been sincere in the belief of his doctrines," as you say he was, or he must have belonged to another extreme in philosophy from that maintained by the sceptics, that every real event was imaginary, he must have believed on a large scale in the occurrence of the most painful bodily sufferings, which had no actual existence! He must have imagined several distressing aches in his bowels and his brains, spasms and palpitations that never actually occurred. He and those who experimented with him on such substances, had been well and hearty when they partook of them; yet, again and again, as they returned to them, they became affected with sufferings of no equivocal or contemptible kind. You do not seem to be aware that the potency of minute division, in giving activity to substances innoxious in the gross state, has other advocates besides the Homœopaths. Fluid mercury, you will admit, has been swallowed in ounces and pounds without producing any serious evil. Yet there are undoubted examples of persons inhabiting places in which a quantity of this metal was kept, having become violently affected by the "infinitesimal" dose of it that found its way, at the ordinary temperature, into the air they breathed. Some think that the mercury had become oxidized, and had thereby acquired an activity not possessed by it in its reguline state. But Orfila, the greatest authority I could produce to you on this subject, ascribes the effects simply to the minuteness of the division in which the metallic mercury was afloat in the air. Buchner and Pereira concur with him in the opinion. It seems, then, to be no "gratuitous outrage" to the reason of the most able and best informed men, whatever it may be to that of others, to assert that substances which we can take "into our stomachs with no other inconvenience than their mechanical bulk," in "ounces, nay, pounds," can produce the most formidable symptoms when in a state of very

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minute division. The fact is believed, you perceive, by very high Allopathic authorities. The *principle*, therefore, of your objection is the reverse of an acknowledged one among scientific men—and the only difference between Allopathy and Homœopathy on the subject is, that what Orfila and others assert of mercury, Hahnemann asserts of charcoal, carbonate of lime, and some other substances; and he has this advantage over those who impugn his opinions, that he has experimentally tested their truth, and his opponents have not!

Of the same complexion with your statements on this subject is the following:—"We hold the great alleged fact from which the doctrine took its rise to be no fact at all; or, at least, not to be a fact of that generality of manifestation which a theory said to be of universal applicability ought to rest upon. We deny, on the other hand, that many of the medicines said by Hahnemann to be capable of exciting artificial diseases in the healthy body, are really possessed of such powers. We instance, in proof of our assertion, the very medicine which gave rise to the idea of the doctrine in its author's mind—cinchona. We deny that it will produce ague, or any thing like ague, or any other form of fever, in the majority of human beings; and so of a large proportion of the Homœopathic remedies in common use."—P. 234. This extract is brimful of mistake, gratuitous assumption, and false inference. The "great alleged fact" on which you strangely imply that the doctrine *rests*, is, I may inform your readers, that Hahnemann, when trying on his own person the effects of cinchona, says he became affected with the symptoms of ague, a disease, as is well known, generally treated by that medicine. You might just as well say that the great fact on which the theory of mutual attraction, or gravitation, among the heavenly bodies, *rests*, is Newton's having witnessed an apple fall from a tree! That very small fact "gave rise" to the train of ideas in the philosopher's mind which issued in the discovery of a great law; but I nowhere learn that it is made the *basis* of his doctrine. That basis is found in calculations and facts, which embraced an ample range of observation. The small fact suggested, and found its explanation in the general law, but would have made but a poor basis for so magnificent and comprehensive a theory. Just so with Hahnemann and cinchona. The effects of the drug suggested and found their explanation in the Homœopathic law, but are as innocent in being a *basis* as the fall of the redoubtable apple. The *great fact* on

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which the doctrine rests is, that diseases like those which may be produced by medicinal substances, admit of being cured by such of those substances as, in their effects on the healthy body, resemble those diseases; and that fact, or general law, *is based* on experiments that embraced an ample range of observation too. But, say you, agues, or any other fevers, do by no means so universally follow the taking of bark, as apples fall to the earth when loosed from the tree. Well, be it so; the latter is a great fact, then, because universally true; and the other is not so great a fact, because not universally true. But does it follow that it is no fact at all? That it has so little of fact about it, that it had no business to "give rise to the idea" of the Homœopathic law? If the excitement of febrile symptoms by cinchona were but occasional and accidental, Hahnemann had as good a right to be the subject of them as any one else. He seems to have been so, and has made a better use of the accident than most men would have done.

But is the occurrence of fever from the free use of cinchona so incredible or rare a thing as you affirm? I do not know whether you deny that it ever occurs, or merely that it occurs often. You say, first, that "the great alleged fact from which the doctrine took its rise is no fact at all;" and afterwards only deny "that it (cinchona) will produce ague, or any thing like ague, or any other form of fever, in *the majority* of human beings." As there is some obscurity, or contradiction here, I shall construe the passages in a way the most favourable to you, and presume that, in the first clause, you mean to say that it is no fact at all that cinchona produces the symptoms of *ague*, and in the second, that it will produce "any other form of fever" only in a minority of human beings.

In answer to this latter allegation, I refer you to any authentic work on Materia Medica. Dr. Pereira, describing the effects of bark on healthy persons, says, that by large doses, "a febrile state of the system is set up, (manifested by the excitement of the vascular system and dry tongue,) and the cerebro-spinal system becomes disordered, as is shown by the throbbing head-ache and giddiness."—P. 1404. He does not pretend to determine what proportion of men will be so affected, but seems to think the operation in question rather characteristic, by the use of the indefinite article. "If *a* man in perfect health," &c., take a considerable quantity of cinchona, febrile action is set up. So much for the production of fever, in regard to which property of cinchona,

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you will acknowledge yourself either to be mistaken, or to have no countenance whatever in the authority of those who know most upon the subject.

Then, as to the power of bark to produce *ague*, meaning by the term a fever, consisting of certain stages, completely ceasing for a time, and recurring in paroxysms, I fully agree with you, that we have no evidence that such a power exists. But if you imply, in the passage I have quoted, that Hahnemann alleges that he experienced an *ague*, in this sense, from the use of cinchona, you are very much mistaken. He no where says so. His words are:—"How is it possible, (if not by Homœopathic action,) that the tertian and quotidian fevers which I have radically cured, some weeks past, by a few drops of the tincture of cinchona, should have presented symptoms almost identical with those which yesterday and to-day I have observed in myself, when, by way of experiment, I have taken a little at a time, though in perfect health, four drachms of good cinchona?"—*Lettre à un Médecin.*

Now, I confess, I never could see any reason for supposing that he meant any thing more than this,—that the bark, (taken in doses, frequently repeated, observe,) produced chilly feelings and shiverings, followed by heat of the surface, and perspiration. If he be also said to affirm, that the proper periodicity of ague was produced also by the bark, then he is made to say that he had a quotidian and a tertian at the same time, which is ridiculous. And if you look at his *proving* or the medicine, you will find that he says nothing of a succession of such stages being followed by an interval of cessation, and that again by a new paroxysm. Shiverings, chilliness, flushing, and perspiration, compose the most characteristic symptoms of an *ague* when the fit is present, no symptoms at all characterize it when it is *not*, that is, in the intermission. And when cinchona cures ague, I suppose it does quite enough when it cures the febrile symptoms, in the same sense as other means cure spasmodic asthma, epilepsy, and other paroxysmal diseases; that is, prevents their return. That cinchona does produce the chilliness, shiverings, heat, sweatings, and other febrile conditions that commonly characterize a fit of ague, is attested by twenty other authorities besides Hahnemann. You will find their names appended to the symptoms they had severally witnessed, in this Materia Medica; and you will distinguish among the number some that belonged to the same school as yourself.

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It is possible that you meant, in alleging that the "great fact" is not "a fact of that generality of manifestation which a theory said to be of universal applicability ought to rest upon," to signify that such is the case, because cinchona fails often to cure ague, even in Allopathic doses, and this may be one of the cases in which you say Homœopathy failed where it ought to have succeeded. If such be your meaning, it originates from misapprehension. Cinchona does not produce all the diversities that may occur in the symptoms of ague in all manner of persons; and when one is affected with an ague, the paroxysms of which is distinguished by symptoms which do not closely resemble those producible by cinchona, Homœopathy declares that cinchona will not cure that ague. The simple fact of its being an ague is never alleged by Homœopathists, and was never alleged by Hahnemann, as being all that is necessary to make it curable by cinchona. It must be an ague, with symptoms of a particular kind. This is the doctrine of Homœopathy in respect to cinchona and agues, and in respect to every other medicine in relation to disease, be it true or false. As every diarrhœa is not curable by the same medicine, neither is every ague, nor every stomach complaint. And the peculiar difficulty of the practice lies in selecting that medicine, among several that may appear to suit, more or less, the particular disease, which suits the particular *case* of that disease. Your allegation, therefore, that certain private trials by those who were strangers to the practice, (and it can be to such only that you refer,) were unfavourable to the claims of Homœopathy, is the weakest of all conceivable arguments; and, with a few others of equal calibre that I have yet to notice, shows an eagerness *nugis addere pondus* which proves that you must have been at a sad loss for argument, and can scarcely have left any stone unturned in search of objections.*

Among the *nugæ* more particularly connected with the *provings* of medicines, and their value as guides to practice, I may notice here an objection you make on the ground that some diseases are *latent*, and can, therefore, afford *no* symptoms to guide us in the selection of a remedy. "How many diseases," say you, "have been detected only on dissection after death, and which have escaped the recognition of the most experienced physicians!" How would a Homœopath

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* Andral is the only Allopath who has published trials of the Homœopathic practice, and his are, as is proved by Dr. Irwine, as absurd as can well be imagined.

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treat such cases, is the implied interrogation? How would you? How would "the most experienced physicians?" For my own part, I humbly confess, I should not know how to treat them. Homœopathy makes no claim to the power of resuscitation. But as you allow that the members of your side of the profession "continue to be almost as ignorant of the actual power of remedies in modifying, controlling, or removing diseases," p. 253, as they have been in all times past, and that the changes which follow their treatment are, "alas! as often for the worse as for the better," p. 258, it seems pretty clear that they must sometimes procure, or hasten, the fatal issue of the maladies they undertake to cure, an amount of potency which you do not grant to Homœopathy, and which Homœopathists, to do them justice, are not ambitious of claiming;—as Allopathy, I say, appears thus to possess the power of killing, it is possible that it may aspire to make alive, were it only as a matter of simple compensation. If such be the fact, Homœopathists give way at once, acknowledging the imperfection of their art in this particular, an imperfection which has reduced them to the necessity of consigning their dead to the treatment of the undertaker.

You next observe, "Every physician, for example, has met with cases of chronic pleurisy, with extensive effusion into the chest, which presented *no pectoral* symptoms, and which were only detected by auscultation. How could the fitting remedy for such cases be selected on the principle of *similia similibus?*" This is a fair question, and the cases fair ones for practice, if you mean to bend so far to the imperfection I have acknowledged as to let us try our skill before death and dissection. In the first place, then, a Homœopath, ignorant of auscultation and percussion, could not treat such cases at all, any more than an equally ignorant Allopath could. But Homœopaths study auscultation and percussion quite as much, and know them as well, as your Allopaths, whether of the old, or young, physic school; and as pleurisy is not always latent, but is commonly attended by *pectoral symptoms*, they have been able to determine what remedies are useful in the ordinary cases. When, therefore, extraordinary cases of the kind you mention occur, they still use the same remedies, and on the very rational supposition, that if they cured the pleurisy *with* the pectoral symptoms, they have a fair prospect of curing it *without* them. Analogy, it is true, suggests the means in such latent cases; but the *similia similibus* furnished the initiative. At the

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same time I admit that the *similia similibus* principle doc not apply to the latent disease individually. We must be contented with having got our treatment of it in a round-about way, and with finding that experience justifies its adoption.

But a more important circumstance is involved in this part of the subject than answering the question you have put. It is this, that in every disease, of which the pathology is so far known as to enable the physician to ascertain the nature of the anatomical changes and morbid actions of the part of the body which is diseased, the Homæopathist regards *them* as of *primary* importance in guiding his practice, and the more remote concomitant symptoms of inferior, often of no consequence to that end. It is thus that in pneumonia, pleurisy, and other well-defined diseases, in which the condition of the parts affected are known and can be ascertained during life, the remedies which the Homœopathist uses are few, notwithstanding that the symptoms which may attend such diseases are numerous and variable. He conceives the more constant and characteristic conditions of the disease, when these can be ascertained, to be the surest indications for the treatment,—because denoting with the most certainty the part that is affected, and the distinguishing peculiarity of the affection. To him the anatomy and physiology of disease, when they are not mere conjectures or assumptions, but ascertained truths, are of infinite value, and, therefore, he regards pathology (in this its only scientific sense) as a department of medicine which he is not merely entitled, but, for the progress and perfecting of his art, imperatively required, to study. If he knew as much of the pathology of all diseases as he does of those I have specified, he would in every one of them, I have no doubt, find occasion to make the pathological condition the more immediate object of his concern, and the director of his practice; and would regard such symptoms as were not necessarily connected with, and indicative of it, as claiming little of his consideration. As it is, he regards the most constant and characteristic symptoms as alone of consequence in pointing out the proper remedy in cases where the true pathological condition which causes them is unknown. When he varies his remedy, in diseases commonly considered the same, although their pathology is unknown, or imperfectly known, he does so only when the particular cases of that disease differ in such a way that the symptoms of one resemble the characteristic effects of *one* medicine, and the symptoms of another the characteristic

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symptoms of *another* medicine; and he acts thus in the very reasonable belief that, when the distinguishing symptoms of one case differ from those of another, the difference depends on *some* difference in their pathology, notwithstanding the general similarity of the cases. If he had any direct way of getting at the difference in their pathology, in all diseases that are closely related, as he has, by auscultation and percussion, of ascertaining the different pathological conditions of inflammations within the chest, *that* way would be much preferred by him in practice, to the less certain method of selecting his remedy by external phenomena and sensations. Yet, in this preference, he would not be giving up the law *similia similibus;* for, of course, the medicines have their *pathology* (in order to produce the symptoms of the provings) as well as the diseases; and all that is wanting *to make pathology the basis of Homœopathic practice is, a correct knowledge of the pathology of both the diseases and the medicines.* Where that double knowledge exists, the Homœopathic practice is founded essentially on pathology,—as in bronchitis, pleurisy, pneumonia, peritonitis, nephritis, cystitis, gastritis, dysentery, and many others,—the more variable symptoms of these diseases leading to the use of various remedies, but only of *such* remedies as produce respectively bronchitis, cystisis, &c., of *some* sort, and with varieties in the more important symptoms that correspond to those of the remedies. Where such knowledge does *not* exist, there is no help for it. If we know nothing more of the diseases and the medicines than their symptoms, we must be content to make the similarity of the symptoms of the one to those of the other the rule of practice; and well does it answer; so well, indeed, that in the great majority of those cases, even, whose pathology is known, and known by experience to require particular remedies, the ordinary symptoms serve to indicate these remedies to one who does not know the pathology of the diseases he is treating, as well as to one who does.

In a minority of such cases, however, the pathological practitioner has the advantage; and I may illustrate the statement by the example of pneumonia. When the complement of its symptoms has the usual amount and degree of completeness, he who neglects auscultation can prescribe for the disease as well as he who relies on the assistance of auscultation; but when, as happens in some cases of pneumonia, the symptoms are very few, or of a nature that does not distinguish it from pleurisy or bronchitis, the former may be

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unable to select the most suitable and successful remedy; while the Stethoscopist, by the aid of his additional means for ascertaining the pathology of the case, is able to do so with certainty and ease. Both may succeed eventually in their object, even with this disparity of knowledge, in the majority of instances; but he who has the aid of the more accurate diagnosis will succeed the soonest, and the most frequently.

While the explanation I have given of the manner in which some cases of disease are now treated by Homœopaths, which have had no actual parallel in the effects of the *provings* of medicines on healthy persons, shows how the principle—*similia similibus*—has led to the practice, there are, undoubtedly, not a few instances in which remedies have been introduced among Homœopathists, without having been suggested by that principle. These are termed by Homœopathists—empirical remedies, because they did not spring from the general law, but were discovered by chance, or something akin to it, like the empirical remedies in general. They believe such remedies to cure Homœopathically, because they do so in the same doses as the Homœopathic remedies in general do. This belief may be right or it may be wrong; but the fact explains how diseases may be maintained by Homœopaths to be curable Homœopathically, which it may be difficult to conceive were ever experienced by a *prover*.

As you seem to have read no other work on the doctrines of Homœopathy than those of Hahnemann, and to be unacquainted with the practice as now almost universally pursued by the physicians of his school, it is not surprising that you should have given a very inaccurate account of the actual state of the Homœopathic art and doctrines. It never seems to have struck you that the third of a century might have led to considerable alterations in such parts of the system as admitted of being corrected or modified by experience; or that it was possible that those who embraced the leading precepts of the practice could differ from their author on the soundness of some of his views. To go back to works of Hahnemann, published twenty or thirty years ago, for an account of Homœopathy, to be presented to the public for the present day, as a fair exposition of the system, is about as fair as if one were to produce the views and statements of Laennec as exhibiting, in all respects, the existing principles and practices of auscultation. That distinguished man has had many disciples, (among whom none in this country deserve to rank

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higher than yourself,) who have added much to auscultation that he had overlooked, and corrected many errors into which he had fallen. Yet auscultation, with all the additions it has gathered in the last five-and-twenty years, and with all the refinements which have been introduced into it by the multitude of its acute and zealous students, is, in point of magnitude and difficulty, utterly insignificant compared with Homœopathy. While it, however, has altered and expanded, by the assiduity and acuteness of its cultivators, Homœopathy must not be allowed to move a pinion, or change a feather. Its principles and practice, as they came from their author, must be stereotyped, and go down to posterity with all their imperfections on their head. What work is there in medicine, whose contents twenty, or even ten, years have not rendered more or less antiquated and obsolete? I cannot charge my memory with one; and if such have been the fate of medical dissertations, down even to the smallest on the smallest subjects in the "orthodox" school, how unreasonable and unfair to admit of no modifications and improvements of the original views and precepts of a system which embraces almost the whole field of practical medicine! I need not say more to satisfy a man of your understanding and literary attainments, that you have committed a very palpable violation of justice in the course you have adopted, and that it is incumbent on you to correct the error into which you have inadvertently fallen. Meanwhile, in order to remove the false impression which your review is calculated to make on those whose information does not extend farther than yours, I may inform your readers that there is a very great difference between *Hahnemannism* and modern Homœopathy.

The customs and doctrines of Hahnemann, which are now either abandoned, or regarded as open questions, by Homœopathists, are his *psoric theory* of chronic diseases, or that which refers them generally to the *miasm* of psora, or itch, contaminating the constitution; his *potential* or *dynamical* hypothesis, which maintains that, by triturations and shakings, medicines undergo an increase or development of virtue, in addition to that which proceeds from the finer division of their substance by mere dilution or attenuation; the employment of the higher or weaker attenuations in acute diseases; the necessity of very long intervals between the doses of medicines. And it is but fair to Hahnemann and to Homœopathy to add, that, latterly, he saw it necessary to abandon in his own practice the two last of these.

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To my mind, if I viewed the subject from the same point as you do, it would appear a very suspicious circumstance if the original propounding of a system, so vast in its compass as Homœopathy, had been brought forth in a form so seemingly complete and perfect as to admit of no alteration in its theoretical principles and practical details; if, as the exposition of one man's opinions and precepts, however profound his genius, it had received the unqualified acquiescence of all his disciples; if its hypothesis had not met with opposition among them, and its practical rules had not been modified by their larger experience. The history of every great discovery in art and science, of every new announcement, that proved to be fundamentally true, would mock its pretensions and throw a just suspicion on its adherents, if Homœopathy, after more than forty years' existence before the world, had remained exactly as it came from its author. On the other hand, I affirm that it is no small testimony to its truth, that in no particular of essential consequence to it, as a rule of practice, has the long period of its searching probation found it to be false, (for the whispers to the contrary are too contemptible to be thought of;) and that where it has been modified, it is in those very points where a large and varied experience would have been expected to modify it; and that there should be so general a concurrence, among the hundreds in almost all countries who have made it an experimental study, on the particulars in which it ought to be modified.

As early as 1824, Dr. Rau, of Giessen, published both his high opinion of the Homœopathic treatment and his dissent from the extreme and hypothetical dogmata of Hahnemann, Since then, the moderate Homœopathy, which employs the lower attenuations for the most part—the very lowest, and even the original or "mother" tinctures in some diseases, more especially the acute—which administers them at short intervals, even every hour in severe acute diseases—which discards the psoric theory and the potential hypothesis—which contends for the practical importance of the knowledge afforded by the pathology of internal diseases, and for the value of the most careful diagnosis, has grown up, and is the almost universal Homœopathy of the present day. With all this the *Homœopathic law*, the *similia similibus* principle, the only fundamental principle of Homœopathy, remains the motto and the maxim of this, the true—the only possible "Young Physic."

Now, what is "degrading" in this Homœopathy? You

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make use of the opprobrious epithet on two occasions in your review, and under the avowed conviction that the system is "calculated to destroy all scientific progress in medicine."—P. 251. But the manner in which you work out your conclusion, if it were not palpably the result of ignorance, would call for a very strong term of reprobation to characterize it. You first misrepresent the subject of your criticism in a manner that may be excused in the obscure editors of our monthly and weekly prints, but is altogether unworthy a man of your place and reputation; and then you pelt it with your scientific contempt. You, indeed, qualify the sneer I have quoted by saying, that if, by Homœopathy, "diseases were to be better treated and more speedily and frequently cured, it would be not only absurd, but transcendently wicked to sacrifice the welfare of humanity for the sake of a scientific phantom." What is the scientific phantom that you would thus magnanimously sacrifice for the good of humanity? Phantom! I suspect humanity will think it a strange phantom—somewhat of the gouhl or vampyre genus—that would make the glory of the physician to consist in diagnostic and pathological acuteness, more than in the recovery of his patients. "I am sometimes disposed to doubt," says an eminent disciple of this phantom school, "whether the untoward event of a disease, which his science had enabled him to predict, and which he had assiduously endeavoured to avert with all the resources of his art, is not productive of *more real satisfaction*—as it *certainly is more creditable*—to the philosophic practitioner, than the recovery of a patient of the nature of whose disease he is ignorant." A remarkable sentence,—and all the more so because containing not the opinions of an individual merely, but of an entire school—the modern school of ultra-pathological physic. It is no wonder that those who entertain such opinions should think that even if Homœopathy were partially true, and, therefore, that it might fairly be received as one of the recognised methods of treating diseases, yet owing its success to the guidance of the bare empirical formula *similia similibus*, as that is commonly understood by the ignorant, it would "be very unfortunate for medicine if this were done." Unfortunate for medicine! No matter for humanity. When we consider the vast number of diseases whose nature is unknown, and the paucity and feebleness which you acknowledge in the resources of the ordinary practice, we can suppose that the cup of superior satisfaction and credit habitually overflows.

But wherefore unfortunate for medicine? Homœopathy,

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as is known to all who are familiar with the history of its progressive improvements within the last twenty years, overlooks no pathological knowledge that can be of consequence to a practitioner of whatever school, and if there would be anything unfortunate to medicine in its being received as one of the recognised methods of treating diseases, on the supposition that it is partially true, the misfortune would consist, along with others, in its furnishing the physician with the means of treating diseases whose internal pathology is avowedly unknown, (with a degree of certainty that he cannot derive from any other source,) by a careful study of what *is known* of such diseases, and the application to *that* of the Homœopathic principle of therapeutics. In your work on diseases of the chest you affirm, "that there are many diseases of the pathology of which we are entirely ignorant; and there is every reason for believing that not a few of these, if really consisting in any change of organic structure, are of such a nature as will never be exhibited beneath the knife of the dissector; and though the progress of science since that sentence was written has lessened our ignorance of pathology, you will not deny that the statements it contains are, notwithstanding, still extensively applicable. In regard to those diseases whose pathology is yet unknown, are we to do nothing in the way of improving our treatment, little satisfactory or creditable as it may be even when successful, save by the fluctuating empiricism that tries this drug and the other, without a rational guide or motive? *Est ridiculum*, (says the orator,) *ad ea quæ habemus nihil dicere; quærere, quæ habere non possumus*—at least for the present. When pathology succeeds in doing any thing to remove the darkness that still hangs over so many maladies, Homœopathy will as gladly take advantage of the new disclosures as your Allopathy can do; and if there be any degradation in treating such diseases, till then, without the light of pathology, it seems to me to attach much more to the senseless empiricism of the old school, than to the regulated method (empiricism, if you please,) of the new.

As Homœopathy, then, seeks avowedly for all the assistance that pathology, or an intimate knowledge of all that can be known about diseases, can afford it, what is the branch of medical science which it neglects? Anatomy and physiology are necessary to the pathologist, and, therefore, cannot be discarded by Homœopathy. Materia Medica, including botany and chemistry, are necessary to the distinguishing, identifying, and preparing of drugs, and, therefore,

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cannot be discarded by Homœopathy. A knowledge of the action of medicines on healthy persons is now called for on all hands, and Homœopathy has anticipated the general voice, and added an immense amount of information to that department of science,—nay, has made it a branch of science peculiarly its own; for on your side there is no proper information on the subject, and I shall by and by show that you could not use it if there were. What is there, then, in the science of medicine that Homœopathy has not? Antiquity. Yes, simply antiquity! That is the only particular in which it is wanting. Now, apart altogether from the general opinion (erroneous it would appear) that the science is not the better of being old,—that the science of a century or two ago is scarcely equal to the science of to-day,—pray what is the difference between the antiquity of the present Allopathy and its Homœopathic rival? Homœopathy, we may say, is fifty years old; how much older is the Allopathy you admire? Pathology, physiology, botany, and so forth, are the same in both; it is in therapeutics alone that they differ. And yet while you contend in one page (240) for the weight of the "accumulated materials supplied by millions of observers during an experience of two thousand years" as telling vastly in favour of Allopathic therapeutics, you tell us, very candidly and deliberately, in another, (260,) that "this department of medicine must indeed be regarded as yet in its merest infancy." In the interval between the two quotations you adduce abundant evidence that the latter opinion is correct. For example you say of the ordinary practice,—

"This comparative powerlessness and positive uncertainty of medicine, is also exhibited in a striking light, when we come to trace the history and fortunes of particular remedies and modes of treatment, and observe the notions of practitioners, at different times, respecting their positive or relative value. What difference of opinion; what an array of alleged facts directly at variance with each other; what contradictions; what opposite results of a like experience; what ups and downs; what glorification and degradation of the same remedy; what confidence now—what despair anon in encountering the same disease with the very same weapons; what horror and intolerance at one time of the very opinions and practices which, previously and subsequently, are cherished and admired!

"To be satisfied on this point, we need only refer to the history of any one or two of our principal diseases or principal remedies, as, for instance, fever, pneumonia, syphilis; antimony, blood-letting, mercury. Each of these remedies has been, at different times, regarded as almost specific in the cure of the first two diseases; while, at other times, they had rejected as useless or injurious. What seemed once so unquestionably, so demonstrably true, as that venesection was indispensable for the cure of pneumonia? and what is the conclusion now deducible from the facts already noticed in the present article, (p. 246,) and from the clinical re-

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searches of Louis and others? Is it not that patients recover as well, or nearly as well, without it? Could it have been believed possible by the practitioners of a century since, that syphilis could be safely treated, and successfully cured, without mercury? Or that it could even be questioned that mercury was not specific in the cure of this disease? And yet what are the opinions and the practices of the surgeons of the present day, and the indubitable facts brought to light during the last thirty years? Are they not, that mercury is not necessary (speaking generally) to the cure of any case, and that it is often most injurious, in place of being beneficial? The medical god, Mercury, however, seems as unwilling to be baulked of his dues as the mythological. If he has lost the domain of syphilis, he has gained that of inflammation; and many of our best practitioners might possibly be startled and shocked at the supposition that their successors should renounce allegiance to him in the latter domain, as they themselves had done in the former. And yet such a result is more than probable, seeing that there exists not a shadow of more positive proof (if so much) of the efficacy of the medicine in the latter than in the former case.

"The same truth, as to the uncertainty of practical medicine generally, and the utter insufficiency of the ordinary evidence to establish the efficacy of many of our remedies, as was stated above, has been almost always attained to by philosophical physicians of experience in the course of long practice, and has resulted, in general, in a mild, tentative, or expectant mode of practice in their old age, whatever may have been the vigorous or heroic doings of their youth."—P. 258–9.

The general testimony of millions of physicians for two thousand years amounts, then, but to a very small matter; and if you prefer Allopathy on evidence of that kind, you may, on as good, prefer believing in ghosts too. It was precisely on such testimony that Johnson did so. "This opinion," says the sage, "which perhaps prevails as far as human nature is diffused, could become universal only by its truth."

One word more and I have done with this question of antiquity. You never once advert to the notorious and admitted fact, that some of the medicines long used in the old school act *Homœopathically*. Nor do you hint that, so far back as the time of Stahl, at least, that is, about 150 years ago, the Homœopathic principles, *similia similibus*, was expressly enforced as the proper rule for the prescription of remedies !*

* In the following terms, "The rule admitted in medicine, to treat diseases by remedies contrary or opposite to the effects which they produce, is completely false and absurd. I am persuaded, on the contrary, that diseases yield to agents which produce a similar disorder (*similia similibus.*)" This passage is quoted in Hahnemann's Organon; for Hahnemann does not lay claim to the discovery of the Homœopathic law by which remedies act, but to the merit of having determined the methods by which it may be carried out into full practical effect; as the proving of medicines on the healthy body more carefully, and the diminution of the doses—two *desiderata* which had rendered the law, before his time, of very limited use in practice.

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If what I have said of the existing state of Homœopathy shall convince you or your readers that, in giving Hahnemannism—that is, the theories, hypotheses, and practices, without exception or modification, of the founder of the system—as the Homœopathy of the present day, you have made a great mistake; it may be replied that you have, at least, beat Hahnemann out of the field, and, to that extent, have accomplished the purpose of your review. This would be a mistake, however, quite as glaring as the other. For anything that you have said to controvert his principles and practices, they stand just where they were before you engaged in the conflict. Not an argument, or the shadow of an argument, appears in your paper that touches a single position of Hahnemann. Hahnemannism might, in every particular, be received as truth itself, if no more could be adduced against it than is contained in your attack upon it. In proof of which assertion, I shall briefly notice the manner in which you think you dispose of one or two of its precepts and doctrines, in addition to those I have already considered.

After detailing the manner in which Hahnemann recommended the medicines to be prepared for use, the triturations and scrapings of the dry preparations, and the shakings of the liquid attenuations, you say, "altogether, it must be admitted, that the whole complexion of the thing bears a much closer resemblance to what we have heard or seen of magical ceremonies and the tricks of conjurors, demonstrations for effect, and to produce an impression, than to any operation of a scientific or *bonâ fide* character,"—a sentence which you justify, because, "in the first place, it is manifestly *impossible* for any human being, during the course of a long life, much less in the course of a few years, to have performed a sufficient number of experiments, or made a sufficient number of comparative trials, to enable him to state, with any degree of certainty, that these particular manipulations, and none others, were the exact and exclusive means to produce the desired effect. * * * In the second place, it certainly has a very suspicious look of a foregone conclusion, rather than of a legitimate deduction from facts, that all the scrapings and rubbings to which each remedy is subjected, in each single state of its transmigration, should occupy exactly *one hour*, and not one minute more or less." P. 238.

In reply to all this I remark, first, that Hahnemann nowhere alleges that he had been led to adopt the six minutes' friction

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in the mortar, and the four minutes' scraping to detach the powder from the instruments, preparatory to renewed friction, by having found these preferable to any other number of minutes. The precise directions given by him for the preparation of the medicines, is universally known by Homœopaths, and might have been easily ascertained by you, to have for their object a *uniformity* of medicinal power in the several attenuations, by whomsoever manufactured. Indeed, within fifteen lines of the place where your translation of his directions stops, he says as much. He there lays down additional rules for carrying on the attenuations to the higher degrees, and he does so expressly, "in order to institute some uniformity in the preparation," &c. Quite enough to have made it evident to any unprejudiced person, not eager to put a disparaging construction on his proceedings, that all his directions were for the same purpose, and yet you actually say, "we cannot find in Hahnemann's writings any explanation of or reason for the *precise* and *peculiar* mode and amount of the manipulations prescribed."—P. 237. The explanation and the reason, notwithstanding, lie in the very pages you must have read; and of all the misstatements into which you have fallen in the course of your article, none surprises me more than this. Hahnemann states in the plainest language, that he believes the powers of all remedies to be exalted by *trituration*, (distinct from mere subdivision;) and though experience has proved that such is not the fact, that belief of his, and the desire that the preparations everywhere should be made of a uniform potency, are the very obvious explanation and reason for the precise and peculiar mode and amount of the manipulations, and ought to have protected him from the sneer about juggling tricks, and magical ceremonies.

Homœopathists, with few exceptions, do not concur with him in thinking that medicines acquire any such increase of power by trituration besides what is due to mere minuteness of division. They admit, however, that in chronic diseases the higher attenuations sometimes act better than the lower *as remedies*, either because the fineness of the division of the particles, or the smallness of the dose, is more suitable to the exigencies of particular cases.

Then as to the "exactly one hour, and not one minute more or less," to which you remark that the frictions and scrapings are precisely limited, I confess that I felt with you on the point,—that it was very ridiculous; and, besides, that Homœopathy must have an intense vitality indeed, nothing

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short of that which invaluable truth alone can give, to have survived the unfortunate notions which Hahnemann has tacked to it. Still, as I knew he was no pretender to magic, no conjuror, and as I had had occasion to observe that you were not an accurate historian of his proceedings, it occurred to me that I might as well consult himself in regard to this awkward particular. I did so, and, while my mind was immediately relieved regarding Hahnemann and his directions, I acknowledge that I did feel a momentary uneasiness about Dr. Forbes. Magic there may have been, nay, there must, in the conversion of six sixes and five fours, (minutes,) into "exactly one hour, and not one minute more or less." But who is the conjuror,—Hahnemann or you ? I fear you must plead guilty, as Hahnemann says nothing on the point, but innocently leaves his arithmetical readers to find out that the sum of the minutes he specifies for rubbing and scraping is just fifty-six ! which, it may be necessary to add, is four minutes less than an hour,—that magical division of time which you have selected for your commentary.

Hahnemann, it seems, is not very intelligible to you when he speaks of as great an *amount*, but lower *degree* of medicinal power, being developed by some differences in the preparation. A knowledge of his doctrines would have prevented your difficulty. He thought friction and agitation developed, or brought out, the *latent* virtues of medicine, so that the same quantity of medicine might, according as it was triturated or not, have its powers either partly latent or fully developed. Here, also, he may be wrong, but his meaning is intelligible enough. And in one sense, he cannot be held to be very palpably wrong, when supported by the authority of Orfila, and Buchner, and Pereira. According to their views of mercury in fine division, as compared with mercury in a crude undivided state, a grain of the latter, though it contained the same actual amount of power or capacity of action, would exert far less medicinal energy than after having been finely divided by trituration with some inert powder.

In all that you have said, then, on the manner in which Hahnemann directs the medicines to be prepared, and of the effects of that preparation, you have not succeeded in proving him to be absurd ; and if any of your readers thought that you did, their opinion must have rested on your inadvertent misstatement of the facts.

Lest it should be thought that the differences between Hahnemann and his followers on the points just adverted to,

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are of a nature that necessarily vitiates and invalidates his claims to success as a practitioner, I may remark that the one difference relates only to a hypothetical explanation of the reasons why the high attenuations are capable of acting as remedies, namely, whether it is merely that in a state of extremely minute division they are still capable of acting, or that their activity depends on their virtues being augmented and developed by friction and agitation; and that the other is a question which relates solely to the energy of their action. Modern Homœopathists do not deny that the high attenuations exert a remedial action in many chronic diseases; but they consider that the lower are preferable in general, because they act more speedily and energetically. Similar differences of opinion exist every where among other physicians, while they profess equally to be guided by experience, and neither party is entitled to assume that the mode adopted by the other is without its measure of success.

That Hahnemann erred on this subject, simply practical as it is, was, doubtless, owing to his attachment to his potential hypothesis—an hypothesis which necessarily enforced the employment of the higher attenuations, as signifying the greatest degree of energy. Why he was so misled by an hypothesis may be a proper subject for the derisive inquiries of those, if such there be, who have never been misled by hypothesis themselves, but have always walked by the unerring rules of demonstration.

Men, who like Hahnemann, have discovered important truths, and are endowed with an ardent genius, learn, it may be too easily, to place implicit confidence in the suggestions of faculties which had already penetrated far into new and unexplored regions of science. They do not always wait for the tardy steps of induction; but, as the history of almost all the great discoveries, as well as of the great errors of genius, declares, grasp by anticipation at conclusions which future experience is left to confirm or annul. The latter is much the more frequent result; and hence, if genius be the benefactor of philosophy, "it is genius also, and not the want of it, that adulterates philosophy, and fills it with error and false theory." Such being the frailty common to minds of that class to which he belonged, it can be only ignorance and injustice that would found on the acknowledged errors of Hahnemann an argument or a sneer against the whole of his system. The more especially that, from the very nature of the subjects to which his hypothesis referred, many diffi-

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culties arose to prevent a speedy and conclusive proof of its fallacy ; and that the particular view which he took of new and extraordinary phenomena was countenanced, and, it may have been, suggested, by certain facts, which seemed to admit of no other interpretation, namely, the actual acquisition of medicinal energy, by means of trituration, by substances otherwise inert. Minute division, and the solubility which it bestows, appear to be the true explanations of these facts—explanations, however, which could have been afforded with certainty only by long and unbiassed observation.

I must, however, do you the justice to acknowledge, that you do not argue in the way I have condemned ; you admit that though the theory, doctrines, or principles of Homœopathy were proved to be false, "we have no demonstrative evidence that it is false in its practical bearings—false, that is, powerless, as a means of cure." If this be true of the doctrines in general, it is very plainly so in reference to the potential hypothesis, which has nothing to do with the main law of Homœopathy—the *similia similibus* principle. The former is disproved by Hahnemann's own followers, and by them only ; the latter they hold to be demonstrably true.

The psoric theory, or rather hypothesis of Hahnemann, is, perhaps, the most unfortunate of his speculations. Not, indeed, on account of anything essentially unphilosophical in either its pathological or practical bearings—but because of the peculiar light in which the disease from which it takes its name is regarded—at least in this country. And as I, like yourself, am an undeniable Caledonian, I am not less sensible than you are, that there is something of the ludicrous about it. But if we lay aside our national feelings on the subject, and look at it in sober seriousness, we must admit, I think, that it may bear a construction discreditable neither to the pathological acuteness of its author, nor to his practical sagacity. It amounts essentially to this, that the majority of chronic ailments are due to a constitutional taint, which betrays itself by a variety of symptoms and sensible effects in different persons, or in the same person at different times ; and that, in order radically and effectually to cure those chronic disorders, it is not enough that the physician should direct his treatment against them individually or collectively, but that he should also have regard to the state of the constitution from which they spring. There is nothing new in all this. Every one knows that in one form or another the doctrine is applicable to a multitude of troublesome and dangerous disorders. Scrofula, gout, syphilis, rheumatism, are

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each held to be constitutional affections, and any one of them may persist for years, or for a life time, sometimes latent, or lulled into inaction, sometimes betraying itself by more or less considerable disorders of one kind or another. In the treatment of these occasional outbreaks of disease, the prudent physician does not always content himself with seeing *them* disappear, but follows up his treatment of them by means that are supposed capable of improving the condition of the system, of modifying or subduing the constitutional evil.

Had Hahnemann admitted psora to rank but as one among many constitutional taints that might from time to time discover itself by various local symptoms, I do not know that any one would be prepared to convict him of error. Nay, it is certain, that his opinion would be strengthened by the concurrence of more than one respectable authority. For it is not a doctrine peculiar to Hahnemann, that the disappearance of the psoric eruption from the skin gives occasion to other evils of a more serious kind. One of his opinions is, that the mischief to the constitution is less when the eruption is abundant on the surface; and it is at least some excuse for his notions on the subject, that when the persons affected with the disease are enfeebled by chronic ailments, of one kind or another, the eruption is much less considerable than in the vigorous and robust, as Biett justly observes. He may be wrong in having supposed that the chronic disorders of such persons are due to the "miasm" of psora being thrown in upon the system; but the two facts, first, that the eruption is abundant when it affects the robust; and, 2nd, that it is scanty in the feeble and otherwise unhealthy, form as good grounds for his particular view of the matter as many of our common pathological opinions regarding cause and effect can boast of. And when it is further considered that such a man as Pringle, not to mention earlier writers, avers that the psoric eruption is sometimes critical, or appears on the surface just when some serious internal maladies have ceased, and apparently in a pathological connexion with their cessation, we see some additional reason for regarding the doctrine of Hahnemann on this subject with leniency.

I confess I have not given the subject so much consideration as to justify me in giving an opinion on the question,—Whether psora is ever the cause of a constitutional taint which may appear in the form of chronic maladies of various characters? And I hesitate all the more to give an opinion regarding it, that the question is answered in the affirmative

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by men who are held, even in our day, as no contemptible authorities in medicine. For example, Autenrieth advocates the doctrine in the following remarkable terms, and at great length in the same strain :—

"The most formidable, and, in our country, the most frequent source of the chronic diseases of the adult, are the psoric eruptions badly treated by sulphur ointment, or generally by other active greasy applications. I have so often seen here the misery which by psora occurs to the lower classes, and to those who have a sedentary occupation, and I see it daily in such a manifold, melancholy aspect, that I do not hesitate a moment to declare it loudly as a subject worthy of the observation of every physician and even of every magistrate, who lays to heart the health of those committed to their care."*

I may notice that pulmonary consumption is one of the diseases he traces to this cause.

Again, Schönlein, the present professor of pathology and therapeutics in the University of Berlin, in his Clinical Lectures for the year 1840, is reported to have expressed himself to the following effect :† (The case under observation was one of organic disease of the heart, with dropsy.)

"What is the cause of this affection? On looking backwards we find no other complaint than the itch. Latterly, the admission of sequelæ of the itch, that old medical dogma, is not only become dubious, but has been abandoned and turned into ridicule. Among the older physicians, we particularly notice Autenrieth, who wrote a masterly treatise on this subject, so that it was remarkably impudent in Hahnemann to pretend that he was the first to point out the sequelæ of the itch.‡ * * * I must confess that, according to my own observations, and to those of many other physicians who deserve the fullest confidence, I have no doubt whatever about the existence of sequelæ of the itch."

And then he goes on to show reasons for his opinion, and the grounds on which he presumes that the chronic disease under consideration took its rise from the itch, which had existed nine years before.

If the errors of one set of reputable physicians can be admitted as some extenuation of the errors of another, supposing them to be in error on this point—and they do so in the way of dividing the unenviable distinction of being wrong—we can adduce some nearly parallel examples of an unwarrantable pathology. Stahl, you know, restricted all chronic diseases to affections of the vena portæ (porta malorum.)

* Versuche für die praktische Heilkunde, p. 229. Tübingen, 1807.

† Lancet, 1844, p. 211, &c.

‡ Hahnemann did not do so. He claims only the credit of having traced *almost all* chronic diseases to the itch, which is more than others had done.

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Portal ascribed all hereditary diseases (and they include a pretty long catalogue of chronic ailments of all kinds) to scrofula; which, again, in all its multitudinous forms, Astruc, Lalouette, and others, (Portal himself among them,) conceived to be degenerated syphilis.

Once more, and I have done with my apology for the psoric hypothesis. Psora is the most common of diseases, in all parts of the habitable globe. No age, sex, or condition can resist its pestilent infection; and back to a remote antiquity its attachment to the family of man is recorded with humiliating certainty. The poor it attends everywhere with the fidelity of a shadow, intrudes wherever men gather in numbers, from the workshop of the tailor to the tent of the soldier. Wherever a chronic disease can creep in, psora can lead or follow. And if it be argued that chronic diseases often afflict persons who never had the eruption of psora, it may be replied that no one can tell with certainty how long the infection, which is commonly betrayed by the eruption, may remain latent in the system. Biett admits that it may for months; Hahnemann thought that it might much longer, and even never cause an eruption at all.

But all this is no proof of Hahnemann's hypothesis. It is not intended to be so;—if it be received as some extenuation of his error, my object is gained. He, in common with Autenrieth and Schönlein, has failed to prove it, or even to make it very probable;—yet it is not utterly and absolutely absurd, whatever "the half-educated multitude" might think of it.

The really important inquiry in reference to this hypothesis is, whether it affects the practice of Homœopathy, so as to involve in its overthrow the pretensions of the latter to success in the permanent cure of chronic diseases. That it does, is the drift of your jocose observations on the subject,—that it does not, is the unquestionable inference, from a candid consideration of the "anti-psoric" treatment. All that is really of consequence in Hahnemann's instructions respecting that treatment is, that chronic diseases in general can be radically cured with certainty only when the remedies which are used for the purpose are selected from among those which cure psora. I have no doubt that he regarded this circumstance as an additional proof of the accuracy of his psoric hypothesis, and if the circumstance be as he says it is, I should consider his inference from it by no means contemptible. That it is true, I do not believe, any further than this, that Hahnemann had some reason to conclude, from experi-

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ence, that the so-called anti-psoric medicines produced a more lasting benefit to the constitution than many other medicines. A much greater range of observation than one man can overtake in a lifetime would have been necessary to have warranted him in saying more. His psoric hypothesis probably appeared to him sound enough to supply the deficiency of actual observation.

It is of some consequence to notice one peculiarity of Hahnemann's psoric hypothesis which you seem to have misapprehended. You make it appear as if he affirmed that when *a chronic disease* is not treated anti-psorically and Homœopathically, *it* must infallibly relapse, and get worse, until *it* ends in death. He says nothing of the kind. So comprehensive is his psoric hypothesis that it makes chronic diseases, with few exceptions, to be of one family,—the offspring of the same blood. Hence, though *one chronic disease*, in the common acceptation of the term, may be perfectly and permanently removed, yet if *another*, though totally different in its symptoms should at any time subsequently appear, Hahnemann would have called it merely a different *form* of the same radical distemper, of the same chronic disease. So that if a man who once had some chronic disorder of his bowels, should, twenty years after it was removed, become affected with palsy, in Hahnemann's opinion it would have been the old disease recurring in a new form, either because the constitutional psora had not been cured along with the former illness, or because the taint had been contracted anew. This affords an explanation of what he means by chronic diseases occurring in a worse and worse form as age advances; and the fact that they very often do so, you will hardly deny, although you may reasonably demur to the doctrine that would make them all essentially the same, however dissimilar in their symptoms; and their occurrence to depend on the one constitutional taint having been uncured. You are, doubtless, sufficiently aware that it is too commonly the characteristic of even the same chronic disease to go on from year to year gradually gaining strength, and becoming less and less amenable to treatment, until it eventually ends in death. This unfortunate course is not witnessed only under what Hahnemann would have termed improper Homœopathic treatment. It is common enough under Allopathy, and every other "pathy," despite of anti-psorics. At the same time, my conviction is, that Homœopathy can do more for many such chronic complaints than any other treatment can,—and it may be, that the "anti-psorics are the most useful of the Homœopathic means.

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When you speak of the "anti-psoric" treatment being as chronic as the diseases, in referring to the "two years" that it requires in order to eradicate them, you mistake the meaning of Hahnemann. He refers not to the cure of what *you* would call the chronic disease, but to the removal of the psoric taint in the system, *his* chronic disease. And I do think that the time he demands is not unreasonable in that view. Most men would be very thankful if they could get scrofula, or gout, eradicated in two years, or ten; though they might think either period rather long for the cure of a single fit of the latter, or of the sore eyes, or glandular swellings of the former.

Once more on this subject. When Hahnemann says that your power of nature cannot cure chronic diseases, he still plainly refers to the "psoric miasm," the constitutional distemper. If his psoric doctrine be regarded as true, the affirmation in question is also true; for psora is well known never to disappear spontaneously. But, waiving the psoric doctrine, it is pretty certain that the power of nature does not, in the sense of Hahnemann, *cure* the liability of human beings to become affected with chronic ailments, from time to time, throughout their lives; and *that* is what Hahnemann considered a proof that nature does not cure chronic diseases, or psora with its many heads. There is no small difference between this, the true view of the doctrine, and your version of it. The latter you give in italics, as if to appeal to every man's experience to testify that Hahnemann was grossly in the wrong; whereas he made no such allegation as your words imply.

But I must shortly notice my second reason for believing that the course of *provings* which you and so many others recommend to "Young Physic," must pave the way for a universal adoption of Homœopathy. Suppose the task executed, and executed well, what can you gain by it, as Allopaths, but some additional purgatives, emetics, narcotics, antispasmodics, diuretics, diaphoretics, and such like, of which you have a store already ample enough to melt the mammiferous creation from off the face of the earth, or to lull it into an endless sleep? I can understand how you may stumble on remedies for particular diseases by trying drug after drug, as each comes to hand, on persons that are ill. This is the method that has been pursued for two thousand years, or thereby, and it has brought some useful remedies to light, of which some, probably the most, act Homœopathically where they act with advantage. But what you can learn of the

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virtues which a medicine, tried on the healthy body, shall exert on the diseased, beyond its probable evacuating and nauseating, and narcotising, and one or two other energetic influences long since abundantly supplied, I am at a loss to conjecture. Will "Young Physic." then, allow all his pangs to go for nothing? Was it only for this that he has panted, and groaned, and writhed, and coughed, and spit, and sneezed, and bled? That he has endured headaches and colics, stitches and twitches in every section of his frame, and so many a fac-simile more of the ills "that flesh is heir to?" Can he make no use of them Allopathically, or Antipathically; or must he be contented to let them stand as penances?

Supposing he should try to turn them to some remedial account, what can he make, Antipathically* or Allopathically,† of such an effect of a medicine as a racking pain in his stomach, for example, or a fiery redness of his nose? Why, *Allo*pathically, he can get up an artificial pain in his stomach, to remove a natural pain from his head, or his feet; or he can set his nose in a blaze, to cure an erysipelas of his legs, on the principle that one fire puts out another. But will the cure not be as bad as the disease? Then, *Anti*pathically, how will he manage to make a practical use of his voluntary afflictions? I can understand how he may succeed, when his nose is disagreeably white, to strike the more becoming hue by a skilful administration of the reddening remedy,—but I am at a loss for the useful employment of the pain in his stomach. The *opposite* of a painful is an agreeable sensation, and I know not an instance of a pleasurable feeling in the stomach playing an important part in pathology. Yes, there is one such. You will find it in the treatise of worthy Dr. Underwood, on the diseases of children. The "inward fits," quoth he, are betrayed by a frequent and sweet smiling during sleep; the which is provoked by wind pleasantly tickling the stomach. Now, for just such a dose of the ache-causing remedy as shall nicely strike the balance between a pleasure and a pain. What an opportunity for our infant Hercules, our young Antipath! to still the apprehensions of a fond mother, and disappoint the forebodings of the lugubrious nurse.

Seriously, what can be made of nine-tenths of the know-

* *Antipathy*, I may remind the reader, means the treatment which aims at producing a state *opposite* to the disease.

† Allopathy,—the treatment which aims at producing a state merely *different from* the disease, or in a different part of the body.

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ledge of the effects of medicines taken in health by the Allopathic or Antipathic methods? The Homœopathic turns them all to account, and no *prover* suffers in vain. Because, for every morbid symptom or effect it seeks a corresponding medicinal one. Let your new provers but bring their experience of medicinal diseases to corroborate that of the Homœopaths, and the universal adoption of Homœopathy is at hand. They cannot leave their knowledge of the provings of medicines to lie useless while others turn theirs to the advantage of mankind. They will try if they cannot do the same, and for a rational man to try Homœopathy is tantamount to his conversion.

But when they do try to employ the medicines they have proved on the healthy body, as remedies for disease on the Homœopathic principle, does it follow that they must adopt the Homœopathic doses? May they not continue to use them in the larger quantities of the old practice? These are points which they must determine by experience for themselves, if they will not extend their confidence to those who have practised Homœopathically before them. I may, however, in the meantime observe that, even in the old system, nothing is less determined than the proper doses of medicines. A sagacious and experienced Allopathic physician, not very long ago remarked to me, in reference to this subject, "What do we know of the proper doses of medicines?" Almost every thing has yet to be determined among you on the subject, for it does by no means follow that the utmost quantity of a drug which a patient can swallow without speedy and obvious detriment, is the right quantity for curing his disease, although this is unquestionably the principle which guides the common practitioner in his prescriptions. In regard to the doses of medicines, and the frequency of their repetition, Professor Jörg, of Leipsic, the very opposite of a Homœopathic practitioner, made the following suggestions twenty years ago, which his brethren have been slow in adopting "the smallest doses of medicines that are yet effective, exhibit their essentially curative powers with most purity and most certainty, and secure us best against any secondary or concomitant medicinal effects. * * * Most of the powerful medicines are at present taken at far too short intervals, and the recovery of the patient thereby greatly retarded, if not altogether prevented, by his becoming affected with medicinal disease in too great an extent."* Jörg is esteemed a great

* Materialien zu einer künftigen Heilmittellehre, p. 9. 1825.

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authority among your best writers on Materia Medica ; and he is almost the only physician, as he certainly was the first of the old school, who followed the example of Hahnemann in proving medicines on the healthy body. You will not, therefore, despise his opinion on the doses that ought to be given, when medicines are employed Homœopathically.

" Medicines operate most powerfully upon the sick when the symptoms correspond with those of the disease. A very small quantity of medicinal arnica will produce a violent effect upon persons who have an irritable state of œsophagus and stomach. Mercurial preparations have, in very small doses, given rise to pains and loose stools when administered in an inflammatory state of the intestines. * * * Yet why should I occupy time in adducing more examples of a similar operation of medicines, since it is in the very nature of the thing that a medicine must produce a greater effect when it is applied to a body already suffering under an affection similar to that which the medicine itself is capable of producing." (P. 16.)

In the last number but one of your review you had occasion to lament the loss of a physician who took a Homœopathic remedy in Allopathic doses. " The case," you say, " may be a most useful warning, and speaks more powerfully than any reasoning as to the absolute necessity of caution in the use of aconite." Let us hope it will be so. Had the unfortunate gentleman taken the medicine in the Homœopathic doses, he would have experienced all the good effects it was capable of affording, and he might yet have been alive. In the work which is reviewed in that article, there are several cases mentioned in which patients narrowly escaped destruction from the same medicine, by the instructions of the physician having been misunderstood. And thus it is that the discovery of a medicine which justly entitles Hahnemann to a rank among the greatest benefactors of mankind, is made to peril or destroy human life—to leave it at the discretion of careless or stupid attendants, by the doggedness of practitioners who sneer at his advice, for its safe and efficacious employment.

On your criticism of the cases which I have published I have little to say. You affirm that the recoveries are all due either to nature or imagination, while you admit " that the amount of success obtained by Dr. Henderson in the treatment of his cases, would have been considered by ourselves as very satisfactory, had we been treating the same cases according to the rules of ordinary medicine." No doubt they would, but why not consider them a great deal more satisfactory than those rules can enable you to effect, seeing that

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the acute cases were cured without the effusion of blood, the pains of purgation, or the miseries of nausea and blistering, and that not a few of the chronic cases had resisted the rules of ordinary medicine, though applied, and in the most serious instances too, by some of the wisest practitioners of your art. I know no reason for presuming that the rules of ordinary medicine in other hands could have effected what it could not in theirs; though I feel very certain, that if any ordinary practitioner had had the opportunity of trying, and had succeeded, he would have regarded the cases as both *very* satisfactory, and his treatment very superior to that which had failed. Since you think that the medicines employed in the cases I have published, deserve no credit for the success, a way is open for you to place the pretensions of Homœopathy on their proper footing. Produce a hundred and twenty-two cases of the same kind, treated by your bread pills, (farina 30,) and the experiment will be complete. You have already endeavoured to prove that that favourite remedy of yours was as useful in an epidemic diarrhœa of considerable violence, as "a course of orthodox physic." It was unnecessary to make any argumentative exertion to prove that it was so, for all Homœopaths, (and your argument is specially addressed to them,) will heartily concur in your conclusion, and believe, moreover, with you, that it would be far better for mankind if the farina practice were more generally adopted in preference to orthodox physic.

I have given one half of an experiment, give you the other. It can cost you no other difficulty than keeping notes of your cases; you can have no scruples, founded on the advantages of the rules of ordinary physic, to overcome, considering that the amount of success in my cases was, in your estimation, *very* satisfactory, though by means which you deem no better than doses of flour. Such is your assertion in favour of your crumbs, and the *onus probandi* on that point lies with you. Bread pills seem to be one of your *recognised* methods of treatment; you have shown them to be preferable to orthodox physic, show them next to be preferable, or equal to Homœopathy, in the same kind and number of cases as I have published; and not only will your professional sagacity be magnified, but you may aspire to the thanks of the agricultural interest, at present so much in need of consolation.

Hard-headed scepticism and credulity go hand in hand. Those who are sceptical on one subject, are very easily satisfied on another; and their unbelief arises quite as much,

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or more, from a blind attachment to the notions they cherish, as from a deficiency of probability or proof in favour of the doctrines they reject. Hard-headed scepticism of this, the ordinary quality, utterly unfits men for philosophical and scientific investigations on a subject to which they are opposed. If it be beset with sources of mistake, the biassed mind of the sceptic can see nothing but these; lays hold of them with avidity, and delights itself in the sapient conviction that, because there are some things fallacious in the subject of its hasty and partial study, there can be nothing that is true. If Jenner had started on his researches regarding vaccination with the antipathies of a hard-headed sceptic, wedded to a foregone conclusion, as all hard-headed sceptics are, his studies might easily have issued in a deliberate refutation of the popular supposition, in his neighbourhood, that cow-pox was a protection against small-pox; and the world might yet have wanted the blessing of his discovery. As it was, with all his determination to know the truth, he almost yielded before the sources of fallacy he had to encounter. How speedily would a hard-headed sceptic,—whether an Ingenhouz, or a Rowley,—have closed his inquiries on the subject, when he had ascertained that the cow milkers often contracted sores on their hands in the course of their occupation, and were not, therefore, exempt from small-pox. What a clear proof that all the whispers to the contrary were old women's fables! What truly sceptical spirit could want more satisfactory evidence? But Jenner's head having been made of penetrable stuff,—not yet become indurated and sapless by the seasoning processes of scepticism,—admitted the idea that, though the circumstances in question were undeniably true, they might not constitute the whole truth. He persevered in his researches, and obtained a glorious reward of his labour.

Scepticism is much more a matter of feeling than of judgment; and there is ample reason for believing that the general scepticism of the profession regarding Homœopathy is owing far more to a *dislike* of it than to any *convictions* of the understanding at variance with its pretensions. In almost utter ignorance of its principles and practice, many, no doubt, like yourself, think the general adoption of it would be "very unfortunate for medicine," and therefore, *hate* it with all the sincerity of hard-headed scepticism, as the supposed enemy of their favourite "phantom." And yet it is this temper which men ridiculously mistake for the philosophic—for that which preserves the mind neutral in the investigation of con-

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tending claims,—which shuts the door against no evidence, but impartially weighs and listens to the arguments on both sides. With this spirit they strangely confound the one-sided scepticism which locks the door against all new comers, or says, with the man in the play, "I'm fixed, determined; so now produce your reasons. When I'm determined, I always listen to reason, because it can then do no harm." And this scepticism, too, which was once held to be a very fine thing, the property of these quite superior minds, which ought not to believe with the vulgar, appears to have overflowed its receptacles among the lustrous population of the higher regions of mind, and to have gravitated to those low-lying valleys of intellect, where there can be but little reflection, because there is little light, and where scepticism is easily accommodated, because there is little to dispute with it the virgin soil. In reference to Homœopathy, at least, it can be said truly, that scepticism is no indication of superior wisdom, for if there be men of talent and learning (on other subjects) opposed to it, it is undeniable that, among the bitterest unbelievers are to be found, both in and out of the profession, a host of persons distinguished alike by their ignorance and their incapacity.

In the course of your strictures on my work, you extract three cases apparently as samples of the whole. If this was the intention with which they are given, I can only say, and say with justice, that you could hardly have acted more unfairly. Even on the supposition that all the recoveries were due to your power of nature, the proceeding is unfair. There are cases, and not a few among those I have published, the recovery of which, within the period specified of each, and more especially considering the time during which the diseases had lasted, and the nature of the sufferings, was sufficiently remarkable to have entitled them to notice, were it only to show how much better *no treatment at all* was than the ordinary treatment. There are cases, also, which had been under no treatment for a long time before the Homœopathic was employed—and some of these might have been noticed as striking examples of what your power of imagination can do, or of the remarkable coincidences that sometimes happen between the commencement of a particular treatment, and the spontaneous termination of a disease. Your readers might then have formed some conception of the reasonableness of the *shifts* by which you endeavour to explain away the apparent efficacy of the practice. They

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would have been able to discover the rules by which one shift or another was selected, as thus :—

First,—That when cases recovered, promptly, from chronic diseases that had resisted the rules of orthodox treatment, continued down to the time when the Homœopathic was adopted, the results must be ascribed to the lucky cessation from orthodoxy.

Second,—That when cases recovered, promptly, from chronic diseases that had *not* been under orthodox treatment for a long time before the commencement of the Homœopathic, the results must be ascribed to the power of imagination, or the accidental and spontaneous cessation of the diseases.

Third,—That when the persons affected were too young to be the likely subjects of this power, the result must be ascribed *only* to the spontaneous cessation of their diseases.

Referring your readers to the work itself for the particulars which you have withheld, I have no hesitation in affirming that no candid and experienced man can peruse the cases *attentively* and say, with sincerity, that he has no doubt that the results are adequately accounted for under one or other of these three heads. This is the utmost that I expected the narration of the cases to accomplish ; and this, I am satisfied, it is fitted to accomplish. I did not dare to hope that it would overcome the strong prejudices of the hard-headed, or silence the opposition of the feeble-minded and malignant. These are conquests which no record of cases can ever achieve.

To those who do not belong to this corps of invincibles I would suggest the propriety of calculating the probability of the causes you assign for the recoveries under Homœopathy. In regard to one of these causes, the *coincidence* of recovery and the use of the Homœopathic remedies, some approach to a mathematical estimate of probability may be obtained ; as, for example, a disease having lasted, without improvement, for six, eight, twelve, twenty-four, or two hundred months, and having no ascertained natural limits, what are the chances of its ceasing of itself in one, two, four, or six weeks, after a certain day ? With every instance, in a given number of unselected cases, in which the amendment commences shortly after that day, (on which a particular practice has been commenced,) the probability lessens of its being due to chance ; until, if nine-tenths of the cases do so amend and recover, no probability is left that chance can account for the results. As to the influence of imagination in produc-

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ing the benefits in the cases to which I advert, I think that reasons satisfactory to all but the invincibles can be shown for its absence in the majority of them, while it remains only as a presumption or possibility in the others. Thus, in some cases the *coincidences* occur in persons who are too young for the work of fancy; in some, the persons affected have no notion of the marvellous nature of their physic, and are very plainly incapable of being moved by the knowledge if it were imparted to them; in some there is a total want of expectation of any result whatever; and in some the remedies given are not at first correctly chosen, according to the rules of the practice, and produce no effect; but when afterwards they are better selected the good effects follow. These, however, are particulars which can be properly estimated only by the man who practically examines, and experiments for himself. No printed records and statements can impress them on the reader as they impress themselves on the practitioner, and therefore it is that documentary evidence can never settle the question in all its divisions.

But why is the subject left to be settled in any measure by documentary evidence? If the practice of Homœopathy have grown to the vast extent which you allow, all over Europe and America—has learned, experienced, and honest men among its practitioners—is so successful as you admit in the treatment of acute, as well as chronic, diseases, and so forth, that you "can refrain no longer" from noticing it,—if "as an established form of practical medicine, and as a great fact in the history of our art," (p. 239,) you are obliged *nolentes volentes* to consider Homœopathy,—why should you restrict your consideration of it to *documents*, which cannot, in regard to every particular, furnish conclusive evidence, and omit to examine the practice in person, or to recommend it to others? This is the only way of considering it that can lead to a definite result on the general question. No man will believe in Homœopathy, in all its extent, on the testimony of those who have practised it, because testimony in practical medicine is so easily evaded by the doubter; and no man ought to disbelieve on the authority of those who *have not.*

Some of my cases you object to as trivial. Now, apart from the fact that a disease does not need to be deadly, or even severe, in order to test the action of a remedy, the objection has probably been founded on the very success of the practice. Take, for instance, the cases of dysentery, and others among the acute cases;—*after the practice was begun*

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their course was mild enough certainly, and their recovery was for the most part very speedy. Does it, therefore, follow that the cases were slight? Would any man be entitled to say from the first report of them, before the treatment, that they were slight of their kind? I say no. And it *is* rather too much to urge the *very success* of the practice, as lessening the evidence in its favour! If the cases had continued as at first, or had increased in intensity, for a number of days, you would call them severe, no doubt; and you would at the same time have evidence, which no Homœopath could gainsay, that the practice was useless. We are entitled to the converse of this, however,—the cases decreased rapidly in severity after the treatment was begun; affording some evidence that the practice was not useless.

Of three cases you quote, there are two concerning which a few remarks are called for. The one is that of a gentleman who had become, from necessity, dependent on aperient medicine for above two years. He took some Homœopathic medicine, and soon became restored to perfect health. The result you consider to have been due to the pill system having been discontinued. Possibly you may be right, and possibly you may not. But you act unfairly in conveying the impression to your readers that I adduced the case as a proof of the marvellous effects produced by the millionth part of a grain of nux vomica. You profess to have read the introductory part of my work, and quote from it a passage to the effect that I published every case of which an account had been taken down at the commencement of the treatment. You seem also to have read the summary at the end of the work, in which it is stated that I do not mean to assert that all the recoveries were due to the Homœopathic remedies. These statements might have suggested to you that, in publishing the cases, I committed myself to no opinion of the cause of recovery in any individual case, (one of the cases of pneumonia excepted,) but acted the part merely of a faithful transcriber of the details of a series of experiments—contenting myself with the remark, that I could not believe the very favourable course and issue of so large a proportion of the cases to be due to accident, or imagination.

You may say, indeed, that if a case were of such a nature that its recovery could not help us to form an opinion of the value to be attached to the treatment, it was useless to detail it at all. But then you forget that, though the recovery of a case may not prove any thing in favour of the treatment,

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its *not* recovering may help to prove something against the treatment. Had the case in question, and others of the same kind, undergone no improvement under the treatment, would you have sneered at them as contemptible? I suspect not,—and you would have been right.

The other case to which I refer I transcribe, with your comments upon it.

"A young lady, aged 19. Aug. 3.—For between two and three years has been subject to diarrhœa, with pain in the bowels, after intervals rarely exceeding a week. The attacks last for several days, and the bowels are moved from six to ten times a day. She is ill at present with one of them. Pulsatilla, 6 twice a day. 29th.—A day or two after last report, the diarrhœa ceased, and has not recurred. 10th September.—Continues without having had a return of diarrhœa; a length of interval which she does not remember to have occurred since the complaint began.

"When the intervals *did* exceed a week, how much did they exceed it? Did they ever reach four weeks? If the young lady could not *remember* this, Dr. Henderson should have inquired of those who could, before he adduced this flimsy case as evidence of the potency of his billionth of a grain of Pulsatilla. Does Dr. Henderson think it a strange thing in the economy of nature, and only to be explained by the *Deus ex machina* of Homœopathy, that a case of diarrhœa, *characterized* by intervals of health, should stop *as usual*, although an incomprehensible something was given, and that it should not return for a few days longer on one particular occasion? These may seem little things to comment on, but surely little things will not be despised by Homœopathists of all men; and here they very significantly show the sort of philosophy we have to deal with. Men capable of admitting cases of this kind as evidence—and we could extract fifty from Dr. Henderson's book much feebler than this—are demonstrably disqualified to treat of things which demand for their handling the stern logic of a masculine mind."—P. 249.

The severe observation which the last sentence contains on myself I let pass without remark, as I have reason to believe that you regret it. I may say, however, that it gave me no uneasiness, because I felt it to be undeserved. As to your inquiries about the case, I confess I am puzzled to know to whom I should have applied for the particulars you desiderate so very much. Who ought to know more of such matters than the person chiefly concerned, when arrived at years of discretion? I know no one who took so lively an interest in the transaction as she, or who had a better right to do so; and if *she* could not remember, who knew all the outs and ins of it, whose memory could have been trusted? It is certain, however, that she could have remembered whether, for two or three months before, she had had an interval of four weeks' freedom from her complaint. That, I think, will be allowed. Then, she may be allowed to have

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had no such interval for several months, at least, before the commencement of the practice as she had immediately after. Still the case does not prove that Homœopathy was the source of her improvement. Granted: but had the complaint continued to recur with intervals "rarely exceeding a week" after that treatment was begun, as it had done for some two or three months before, (her memory may be trusted so far, surely,) the case would have proved that Homœopathy had *failed*. In a series of experiments regarding the truth of an allegation, the failures are of no less importance than the successes, nay, in physic they are of far more value as evidence, for successes may be only apparent, may be fallacious, whereas about failures there can be no mistake.

You say that you can extract fifty much feebler cases than this from the book. You cannot extract one that does not bear upon the investigation in the same way, and with even more significance than this; and though the view of these experiments which I have now given appears never to have occurred to you, it is not the less an important one, or one which you ought to have seen without my help.

The cases in general were of that kind which composes the great majority of the ailments which are treated by the rules of orthodox medicine, by purgatives, anti-spasmodics, emmenagogues, leeches, blisters, anodynes, tonics, antacids, mercurials, &c., and yet without any of these they recovered, as you admit, very satisfactorily. Some of them were of a more serious description, and had resisted the orthodox rules, though applied in a few of them by some of the best practitioners in this city; and yet of these the majority recovered, or were greatly benefited also; and in a very short time. Those that did not, were mostly of a kind, or in a stage of disease that defies all medical treatment, with the exception, possibly, of the Irish. For, though your new contemporary of Dublin,* with a racy Irish *équivoque*, proclaims that results which you term very satisfactory, would, in the hands of Dublin Allopathy, "have been widely different"—I will suppose the writer to mean that the Dublin practice would have proved more successful; that the Allopathy of the favoured Isle, where the "vulgar regard the physician as scarcely second to the priest," (p. 179,) and where the polite, we presume, regard him with much less reverence,—would have been more fortunate than the Allopathy of Edinburgh, or of London—for modesty does not flourish every

* The Dublin Quarterly Journal of Medical Science, No. 1, February, 1846.

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where. I leave you and your contemporary to settle the point between you; while I content myself with the fact that the results of my cases must have appeared to the said writer too satisfactory to be published in his review, seeing that in his report of them he has taken such liberties with the text as, I trust are not to be regarded as specimens of Irish honesty, among priests or physicians, the vulgar or the polite. It promises little for the character and prosperity of a new periodical that it should come into the world with disingenuousness stamped on its forehead. For, unwilling as I am to make a grave charge against an opponent as long as charity can suggest an excuse for him, in the present instance no choice is left me;—and I accuse the writer of that review of having studiously misrepresented the cases he has quoted. How sad it is that an uncandid spirit should befoul the current of criticism in questions of science and humanity. In the words of Hazlitt, "a writer who assumes the garb of candour, and an inflexible love of truth, to garble and pervert it, to crouch to power, and pander to prejudice, deserves a worse title than that of a sophist."

If the literal truth were known, I suspect it would appear that the cases are something of a puzzle to you Allopaths.—One will have it, that the recoveries are so satisfactory that they must be due to the cunning hand of nature,—whose works so far excel the doings of man; another, that they are so incredible that the cases must have been too highly coloured,—(The *Lancet*, 1845;) a third (the *Dublin Journal*,) that Allopathy could have done better. On the whole, then, the cases and recoveries may be regarded as tolerably good.

That portion of your article which is specially addressed to the practitioners of orthodox medicine, and lays down rules for the future guidance of "Young Physic," does not lie within the scope of what I proposed to myself in this letter, and I shall say little about it. Almost the only thing that strikes me as worthy of remark, in connexion with it, is, that your Dublin contemporary is somewhat unkind in receiving so coldly your scheme in reference to a "Young Physic," considering that you propose that a part of his nursing should be according to a genuine Irish receipt. Your ninth rule runs thus:—

> "To discountenance all active and powerful medication in the acute exanthemata, and fevers of specific type, as small-pox, measles, scarlatina, typhus, &c., *until we obtain some evidence* that the course of these diseases can be beneficially modified by remedies."

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"I'll never go into the water again till I learn to swim"—was the wise resolution of the Irishman, as the story has it, who narrowly escaped being drowned. "Use no physic in the acute exanthemata till we learn that it is of use," is the new practical rule of a system strongly suspected of having drowned not a few in its day.

Even the young Dublin Quarterly has some doubts, although not very definite, of the great advantages to science of such a contemplative method as this; and ventures, very innocently, to surmise that "Young Physic, if it ever germinate at all, cannot possibly be expected to bear any fruit till our children, and our children's children, have been gathered to their fathers." If I mistake not, the treatment of the acute exanthemata will not have commenced even then, unless Young Physic apply to mesmerism for a revelation on the point.

On looking over the extracts I have made, in the foregoing pages, from your review, I observe that they do not include your reservations in favour of a mild and judicious Allopathy. As it would be unjust to allow the readers of this letter to carry away with them the impression that you condemn the ordinary practice altogether, I am bound to inform them that this is not the case. While you boldly arraign the medical art, as generally practised,—denounce the too indiscriminate and profuse administration of drugs,—and lament the existing ignorance respecting their remedial powers, you distinctly affirm that Allopathy is a system "which, with all its faults, contains a considerable amount of truth, and a yet greater amount of good."

This statement, indeed, does not refer especially to the Allopathic *art*, but appears to include its pathology, and other branches of medical science. These, as I have already said, are equally the property of Homœopathy, and therefore no Homœopath will desire to controvert your opinion. Yet, supposing it to include a little of Allopathic *practice* also, I can offer no objection to its justice. For while embracing Homœopathy, in the sense in which I have explained it in this letter, I do not think that it contains the whole truth of therapeutics, though I believe it to contain much more than any other system. I am aware that in making this avowal I shall not please the bigots among the disciples of Hahnemann, and may incur the sneer of the suspicious and sordid (the sordid are always suspicious) among their opponents of the old school. I count either event a very small matter, persuaded that when the candid and intelligent on both sides

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come to know one another, and understand one another's views and methods better than they do at present—when the dusts of controversy have had time to settle and the atmosphere is clearer, they will find that they are not so very far asunder as they at present suppose. Yet we may have many a tough encounter before we "sheath our swords for lack of argument,"—a prospect which we Homœopaths rather rejoice at. We claim nothing but a fair field and no favour; and are ready to fight it out, without a shadow of doubt as to the issue.

The contest may be conducted as it becometh gentlemen to contend, without the rash imputation of unworthy motives —without appealing to the prejudices and passions of the ignorant—without wilful unfairness, and without discourtesy. You have set the example of an onset free from those degrading vices of controversy, and I trust that I have in this defence been also successful in my endeavour to avoid them. If not, I shall be heartily sorry for my failure.

With every sentiment of esteem, I am,

Your obedient servant,

WILLIAM HENDERSON.

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JAMES R. MANLEY, M. D.

New-York, September 7, 1842.

Certificate of JOHN F. GRAY, M. D., formerly resident physician to the New-York Hospital, Lecturer on the Theory and Practice of Physic, Censor of the State and New-York Medical Societies, etc., etc.

I am very glad to find the press engaged in diffusing a knowledge of the German medical literature in this country. At the head of the German books of practice stands this book of the good Hufeland. Mr. Bruchhausen and Dr. Nelson have labored with diligence and good faith in rendering the *Enchiridion*; and so far as I have had leisure to compare their work with the original, I find no error of magnitude. Another edition will, no doubt, be called for soon, and then the worthy American curators can dispense with the somewhat meagre characteristic given to it by their too close adherence to the letter of the author. I heartily wish success to the good enterprise.

JOHN F. GRAY, M. D.

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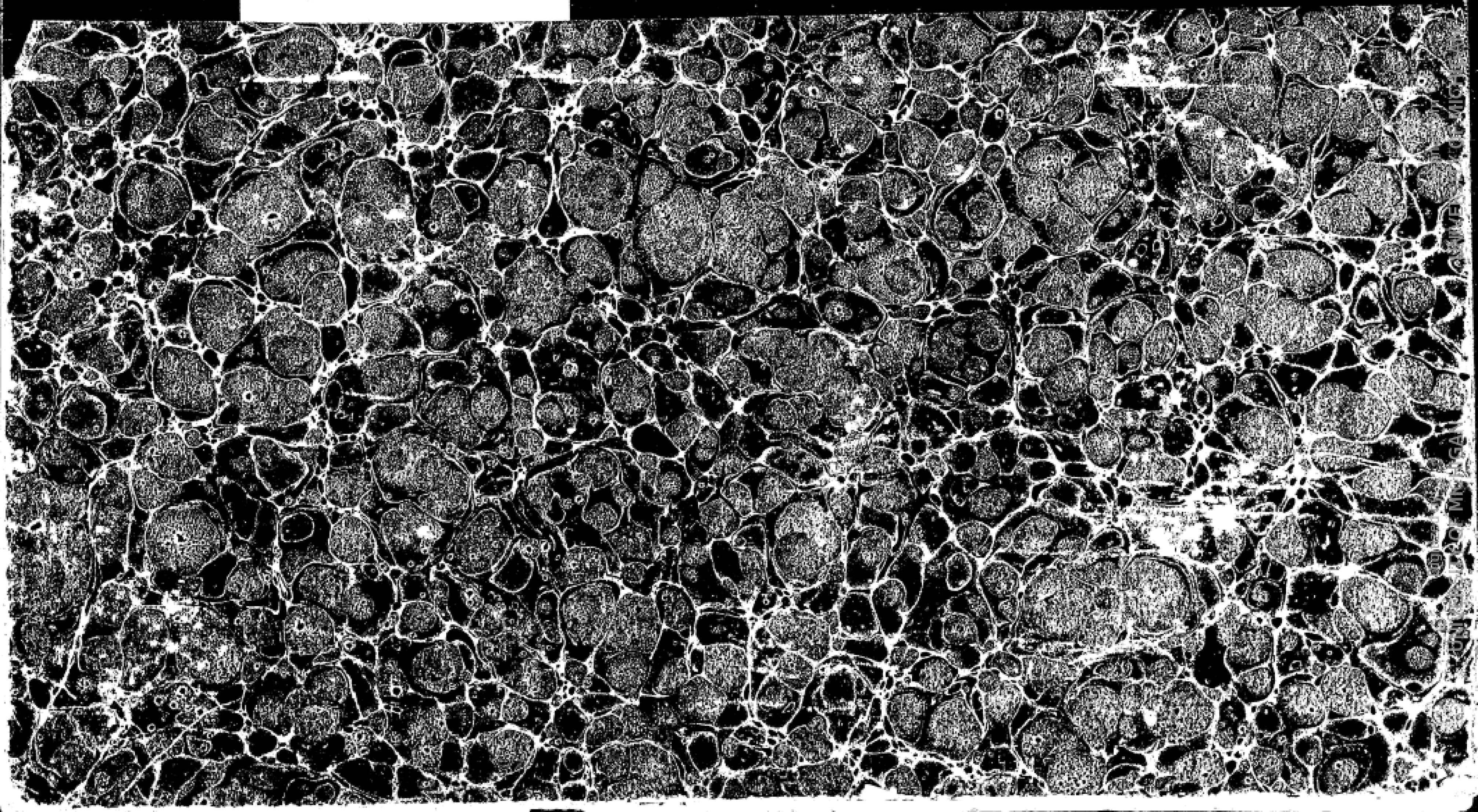

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