

## **Correspondence between Professor Christison and Dr. George E. Stewart, on homoeopathy.**

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Christison, Robert, Sir, 1797-1882.  
University of Glasgow. Library

### **Publication/Creation**

Edinburgh : James Hogg, [1851]

### **Persistent URL**

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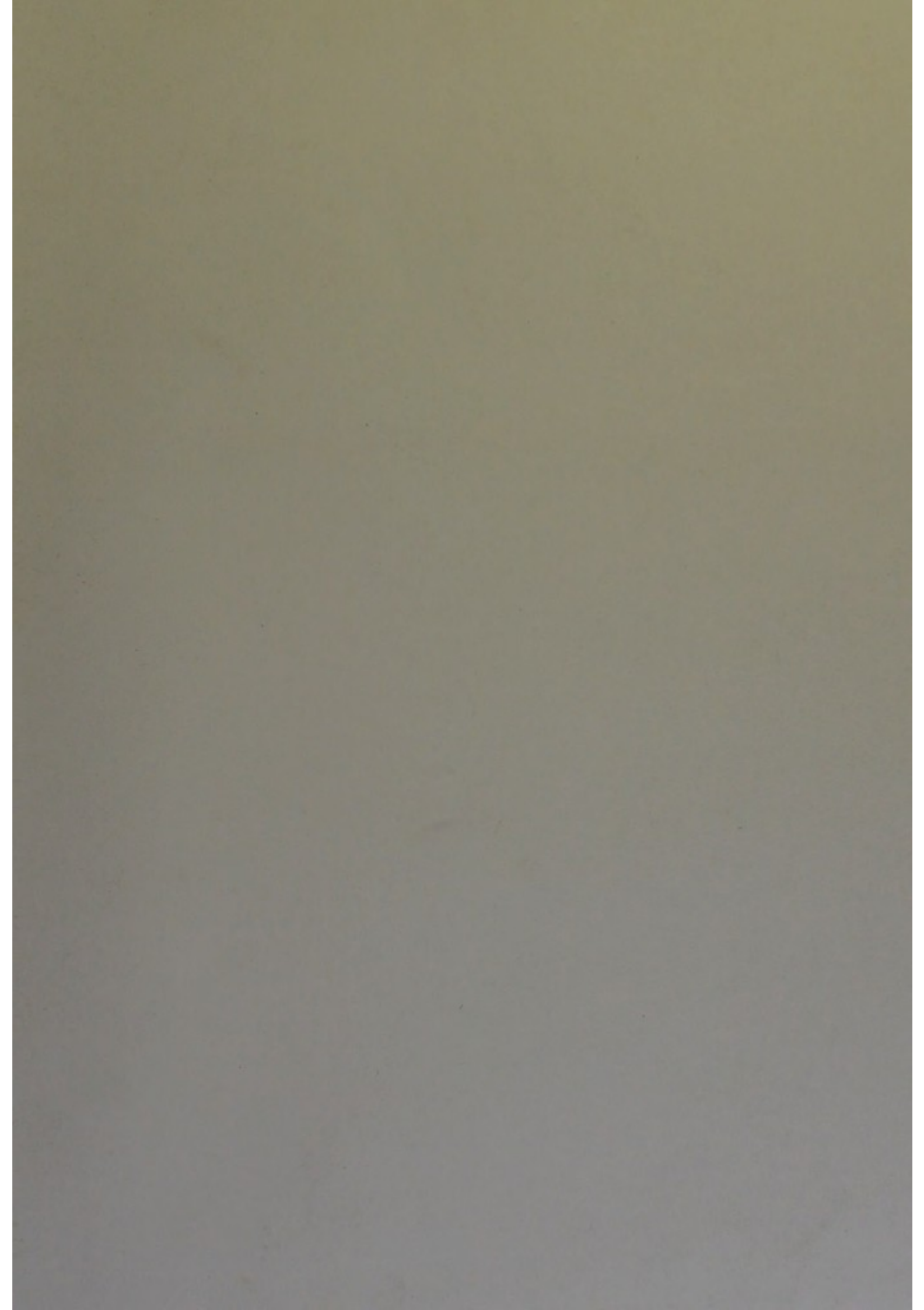
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CORRESPONDENCE

BETWEEN

PROFESSOR CHRISTISON

AND

DR GEORGE E. STEWART,

ON

HOMŒOPATHY.

EDINBURGH:

JAMES HOGG, 4 NICOLSON STREET;  
JAMES BROWN, 34 SOUTH CASTLE STREET.

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*Price One Shilling.*

C.

CORRESPONDENCE

CORRESPONDENCE, &c.

PROFESSOR CHRISTISON

5 ST JAMES' SQUARE, 21st Nov, 1851

Sir,

I have been favoured with notes of a recent lecture by you (taken during its delivery) on the subject of Homoeopathy, in which I find the following passage:—"Edinburgh has been charged with sending forth eight Homoeopathic Graduates out of the forty-eight who graduated here last year; but I hardly believe this. If it is the case, they must have concealed from their examiners their true sentiments, and, for base ends, have declared themselves adherents of a system they believed to be false."

As I am one of those who believed in the superiority of the Homoeopathic system, at the time of graduation, I deem myself entitled to inquire whether the passage referred to contains a substantially correct report of what you said.

I am, Sir,

Your obedient servant,  
GEORGE F. STEWART

To Dr Christian.

MORAY PLACE, 22d Nov, 1851

Sir,

I am sorry indeed that any Graduate of the University of Edinburgh should have felt the necessity of making such a concession as that which I have received from you this morn-

ing

## LETTER, &c.

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MY LORD AND GENTLEMEN,

IN addressing the public through the medium of a letter to you, chiefly to vindicate myself from certain misrepresentations of the Medical Faculty of the University, I owe you a few words of apology and explanation. You are not only the Patrons of the University, which has, by a recent decision of its Faculty of Medicine, become in some measure a party in the controversy which at present agitates to an unusual degree the stormy atmosphere of medicine, but, as the representatives of a large and intelligent community, are looked up to as the patrons of reasonable freedom and even-handed justice among those over whom you preside ; so that I have a double claim on you for regarding with indulgence the form in which I publish this defence. I have no other favour to solicit at your hands ; I am no petitioner for the exercise of your authority in any way ; for in such disputations as I may have in the least degree connected with scientific matters, there is nothing I should deprecate so much, even were it possible, as the interposition of magisterial power to defend my opinions, or to oppose an obstacle to the fullest exercise by others of their undoubted right to

liberty of thought and speech. In this way of thinking, indeed, I stand, with one honourable exception, alone in the Medical Faculty ; yet I am not the less satisfied on that account that my conclusion on the subject is as thoroughly right, as it is on another great point, of which I have the undivided honour to be the only advocate in that body.

That I do the Faculty no injustice by the statement I have made, will appear evident from the Resolutions I have to lay before you, which shew plainly their disposition to thwart the progress of principles which are held by a large and respectable class of medical practitioners, and by many intelligent persons of all ranks among the public, to be important scientific truths, by inviting the exercise of your powers, in some way or other, to injure, degrade, or silence a Professor in your University, because he avows his belief that those principles are true. I need scarcely say that it is no justification of the Faculty, in having contemplated such a measure, that they honestly regard those principles as false, for a similar plea might be advanced on behalf of all the intolerance and persecution that disgrace the history of the world.

To accomplish their object, the promoters of this scheme have chosen a course that takes the business of my vindication quite out of the field of scientific controversy, and engages me in a merely personal conflict. Having long anticipated such a result, it has been my endeavour to avert it by such means as I could employ without discredit—by the peaceful discharge of my professorial duties, by replying to no attack made on my opinions within the walls of the University, by heeding no gossip, and retorting no sneer, with which the taste of some of my colleagues has occasionally seasoned their discourses. I have not even offended the Faculty with my presence but on the most

necessary occasions ; for I concur entirely in the sentiments of a much more famous man than any of us, when he says, “ were I even wrongfully suspected, and thereby made offensive to my fellow-citizens, I would rather shun their company than be looked upon with hostile eyes.” All, however, has been of no avail, and since the Faculty have preferred the personal assault to the more academic course of rational discussion, it remains for me but to accept their challenge. It may be true that their recent experience of public opinion has led them latterly to shrink from carrying their purpose into full effect ; but the Resolutions which embody that purpose still remain unrecanted and without apology, and I am not tamely to submit to their injustice, to be badgered and bated as I have been for daring to think for myself, and yet allow those who intended to do me the deepest wrong to escape scathless from the false position they had taken, perhaps to seek their object in another way. At the same time, I neither feel so keenly nor regard as of so much moment the conduct of the Faculty in joining theirs to the many voices that are in full cry against me, as to have any apprehension that I shall be betrayed into expressions that shall injure the great and good cause which is aimed at in my person, or that shall offend the excellent and cultivated of all shades of opinion on other subjects, who look with interest on our medical controversy, and whose sympathy, as lovers of fair play, would nerve me for a much more unequal contest than I can conceive this to be.

On the 24th of June last, the Faculty, in a meeting at which ten out of thirteen members were present, passed the following Resolutions, unanimously :—

“ 1. That the public profession of Homœopathy, by the Professor of General Pathology, is inconsistent with the efficient



discharge of the various duties which belong to that Chair, and is calculated to injure the University as a Medical School.

“2. That the *Senatus Academicus* be requested to transmit a copy of this Resolution to the patrons of the University, together with the expression of a hope, on the part of the Medical Faculty, that some step may be taken to avert the danger thus threatened to the University.”

Like everything else, either great or little, these Resolutions have a history, and to that I shall briefly advert; not with the purpose of making the Faculty appear ridiculous, but to shew that the University of Edinburgh, in spite of these Resolutions, is still entitled to be regarded as the seat of philosophy and the liberal arts. To me, individually, it could have been no disadvantage though the *Senatus* had complied with the request of the Faculty of Medicine, for my vindication would still rest on common justice and common sense, which even the greatest names in literature and science can, happily, in these times never outweigh. I do not rejoice the less, however, that the proposal of the Faculty was regarded by the other professors with the most decided disapproval, and that too, not simply as impolitic or inconvenient, but as in the highest degree illiberal and unfair, and as not the manner in which differences of opinion on scientific questions should be settled. Had the result been different, could the *Senatus Academicus* of the University of Edinburgh have been capable of any other decision, the remark of one illustrious countryman of ours, that universities were like rocks in a stream, that served but to mark the onward motion of the current, and of another still more famous in the peerage of genius, that they were the “dormitories of science,” would have received an illustration both apt and conclusive.

Why the Faculty should have determined on the extraordinary course which has met with so severe a check from their colleagues, I may hazard a conjecture in the sequel. That it has been somewhat suddenly conceived, admits of indisputable proof. In the June Number of the Journal conducted by the very members of the Faculty with whom this persecution originated, they exonerate themselves from the charges of being lukewarm on the subject of Homœopathy, and of tolerating a colleague who professes a belief in that system, in the following words,—“as to the Professorship of General Pathology, the Medical Faculty have no power in the way of appointment or dismissal; but we have no doubt that if it ever came to their knowledge that Dr. Henderson was making his lectures from this Chair the vehicle of Homœopathic doctrines, they would petition the patrons for his removal. The testimony of students who have attended this course assures us there has hitherto been no ground for any such complaint; *and so long as this is the case, we do not see how the Medical Faculty can take cognizance of the principles upon which their colleague chooses to conduct his private practice.*” A month did not elapse, however, before they altered their opinion, and fancied they could make out some pretext for molesting their colleague, on account of those very principles “upon which he chooses to conduct his private practice.” And what makes this rapid change all the more remarkable is, that it is singularly inconsistent with their conduct during the previous seven years that their colleague has been known over the three kingdoms as a believer in Homœopathy. Thus, the Professor of Clinical Surgery, who took the lead in this proceeding of the Faculty, was the mover, in the College of Surgeons a short time previously, of a Resolution which bore, that meeting in consultation with those who practise

Homœopathy, is to countenance the system, yet he himself has repeatedly given such countenance to it in this city. Lest he may have forgotten the occasions, I may remind him of two that fell within my own knowledge. The one occurred a few days before he set out to take possession of his London appointment, the other a few days after his return from that enterprise. The Professor of Midwifery has still more frequently countenanced it in the same way: and several others are in the like predicament; while one professor, who was present at the meeting and concurred in the Resolutions of the Faculty, requested me by a letter under his own hand to attend his only child during an attack of scarlet fever, a little service which was as freely rendered as it was frankly asked, and has now (shall I add?) been handsomely requited!

It is scarcely possible to peruse the Resolutions in which the Faculty profess to unburden themselves of a serious duty, without being reminded of the celebrated paradox, that language was given to man in order to conceal his thoughts. Not, indeed, that the Resolutions are a happy illustration of the doctrine;—quite the reverse, for it can escape no reader of common acuteness, that there is an incongruity between a certain cause they specify, and an effect ascribed to it, which at once raises a suspicion, that what is alleged is not a fair representation of what was actually in the mind of its authors. Language may, indeed, be employed to conceal the thoughts, but that its natural purposes are those of frank and manly honesty, is plainly evinced by the difficulty men generally experience in giving the air of consistency to statements that are not thoroughly candid. It is, therefore, in a certain sense, creditable to the framers of the Resolutions that they are not such great adepts in the style of composition they have chosen as to have made their contrivance

perfectly answer the purpose. It will be observed, that the first part of Resolution No. I. professes a belief that certain University duties are insufficiently executed by the professor of pathology. So far the conception is unmistakably expressed, and no one can justly quarrel with the concern it shews for the proper performance of professorial functions. I, at least, however I might console myself with the reflection that I honestly attempted what I had been appointed to do, must have silently bowed to the censure. But there is another limb to the invention, and it matches so ill with its fellow, as to defy the best surgery to remedy the lameness of their gait when they would march in company. It is the "*public* profession" of Homœopathy that is blamed for the deficiencies which are alleged! There might be no very manifest absurdity in saying that a *belief* in the Homœopathic doctrine was inconsistent with the duties of a professorship of Pathology;—it is conceivable that under the cloak of Homœopathy there might lurk some jealous and unsocial demon that would flatly deny and "repudiate" every other principle in medicine, and in that case, doubtless, a *belief* in Homœopathy would disqualify the present Professor of Pathology for discharging one or two of the lesser duties he is now, as University affairs are managed, called upon to perform. This supposititious case I shall consider in its proper order. It is not the belief of anything, but, as we have seen, the *public* profession of something that is said to be "inconsistent," &c. Now, I retort the charge of inconsistency on the propounders of this doctrine. There can be no possible relation between any amount of publicity on the one hand, that is, any amount of knowledge on the part of the public, and open avowal on mine, of the opinions I hold regarding Homœopathy, and, on the other, the manner in which I criticise a student's prescription or a thesis.

They are totally distinct, incommensurable, and unconnected, and no clear or candid reasoner could have brought them within sight of each other. The authors of the Resolutions would have us believe that they are actuated by anxiety for the right discharge of professorial duties; but the garb in which they dress their argument is so badly contrived that the naked truth protrudes before the eyes of a "discerning public," and vindicates the gift of language from the stigma of being designed only to mislead. It is obvious that the public profession of Homœopathy is the offence, and that the alleged imperfection in "the discharge of the various duties" of the Chair, as in any degree deducible from that, can be nothing but the merest pretence.

By founding their charge on the public profession of Homœopathy, the projectors of it tacitly admit that the belief of Homœopathy, if kept private, or confided to their ear alone, would not be inconsistent with the discharge of the duties of the Chair of Pathology, so that the publicity constitutes the essence of their grievance, and, thanks to the fidelity of speech, they have plainly, however unwittingly, expressed themselves to that effect. Many of their brethren may affirm that that publicity is an evil sufficient to justify the Faculty in taking measures to injure their offending colleague, but no amount of concurrence on that point can help the Faculty, or those members of it, at least, who have given any measure of attention to the Resolutions they have allowed to appear in their names, out of the serious dilemma of having attempted to avenge that offence at the expense of a charge at once uncandid and ridiculous.

That in these strictures on the Resolutions, I am not taking advantage of what ought to be regarded as a merely inadvertent awkwardness of expression in order to expose the Faculty to contempt, will be sufficiently evi-

dent when I state that those who were the chief promoters of these proceedings do not scruple to affirm that Homœopathy is nothing but profession,—that those who profess it do not believe in the principles they avow! They are of course at liberty to form on that point such charitable conclusions as their particular dispositions may suggest, but when they pretend to infer from such an accusation a consequence which is an outrage to the simplest logic and common candour, they have only themselves to blame for the result.

There are other members of the Faculty of more fairness and greater courtesy, and they may have intended their indictment to imply that a *belief* in Homœopathy disqualifies me for the discharge of my duties in the University, I therefore proceed next to the consideration of that supposition.

That they do not charge me with teaching Homœopathy from my Chair appears from the passage I have quoted from their Journal for June, and is yet more recently attested by the following quotation from the same periodical for the present month:—“The principles and practice of Homœopathy have never been taught there (*viz.*, within the walls of the University) by any Professor.” While this is true, in so far as I am concerned, it must not be presumed that I have been deterred from such teaching by the fear of the Faculty, or of the profession. My commission from the patrons does not require me to lecture on the principles of Therapeutics, otherwise no consideration should have hindered me from expressing my deliberate convictions on the comparative uncertainty and inefficiency of most of the Allopathic and Antipathic principles, or from explaining and advocating others much more worthy of being universally adopted. The students, whom some of the Faculty have had the delicacy to interrogate on this subject, might

have gone further in their evidence, and have deponed that when I have adverted to Therapeutics at all, it has been to illustrate Pathological principles by the effects of drugs administered according to the ordinary practice. In their imperfect conception of the liberty which he feels who is untrammelled by prejudice, and indifferent to unmerited rebuke, to avail himself of all varieties of truth, by whatsoever artificial distinctions men choose to divide them, this, too, may be regarded by the Faculty as an inconsistency; and their limited knowledge of Homœopathy may lead them to conjecture that it is excluded from my lectures out of deference to those who are opposed to it. The latter supposition is worthy of a single observation, and it is this,—that the Homœopathic action of remedies, being a *specific* action, admits as yet of no rational explanation, can throw no light on Pathological principles, and can therefore have no place in General Pathology. When the two first of these statements shall be no longer true, when any member of the Faculty or of the profession at large shall reveal the principles on which specific remedies, mercury, arsenic, iodine, &c., act in producing their results, then, though the discoverer should be the bitterest of the present adversaries of Homœopathy, I shall not hesitate to avail myself of the light his discovery may throw on the principles of Pathology. In doing so I shall probably lecture on Homœopathy, for I believe that the specific action of remedies is no other than Homœopathic action. When that time comes, the Faculty may follow the example of their brethren in the University of Montpellier, and apply to the proper authorities to deprive me of my class if not of my Chair. The professor of Pathology in that university was forbidden by the Minister of Public Instruction to mention Hahnemann or Homœopathy in his lectures; but I cannot believe that in similar circumstances

the more liberal rulers of a British Institution would care to imitate, even in that comparatively lenient measure, an example presented to them in the unscrupulous days of Louis Philippe. The Faculty of Montpellier did not attempt to injure their colleague for his "public profession" of Homœopathy; they took exception only to his lectures. It has been reserved for a Medical Faculty in this land of liberty to signalize themselves by a higher flight of intolerance.

There is but one other professorial duty which I have to perform—the examination of candidates for graduation; and I have been told by one of the members of the Faculty who concurred in the Resolutions, that there are two parts of that examination with the conducting of which a *belief* in Homœopathy is supposed to be inconsistent. One of these is the criticism of the two or three prescriptions which each candidate is required to write. Now I have not only never denied the existence of Allopathic and Antipathic modes of administering medicines, but six years ago publicly and plainly, in a treatise on Homœopathy, expressed my conviction that such methods of treatment have their measure of advantage, and that in judicious hands many of them are of no inconsiderable service. It matters not that I believe the Homœopathic method to be of greatly superior efficacy; Homœopathy is not taught as yet by any professor in the university, and I am not entitled to demand from students a knowledge of what they never were required to learn. In admitting that there are other modes of treatment besides the Homœopathic, which may be of service to the sick, I am no more to be charged with inconsistency, than I should be in maintaining that there are slow coaches on the old roads, as well as railway trains on the new. And where only slow coaches are permitted to be employed, I



can see no reason why I should be disqualified for passing my opinion on them, merely because I deliberately think that railway carriages are greatly to be preferred. If a very moderate estimate of the common remedies be inconsistent with a candid judgment of ordinary prescriptions, will the Faculty hold that one of the most eminent of their own school, Dr. Forbes of London, could not discharge that very simple duty, and would be unfit for a medical professorship, because he has declared that things in the old system have come to such a pass "that they must mend or end?" I apprehend they would not; and yet I do not suppose that I think so meanly of the old system as that gentleman does.

Precisely the same remarks apply to the other part of these examinations—the criticism of such theses as are submitted to my judgment. The exacting of such essays at all from candidates is an idle custom, and, as in the examination for admission into the Faculty of Advocates, little if anything more than a form. The advocates, I understand, invariably buy theirs; the medical graduates *may* do the same; there is no check to prevent them, and they doubtless often take advantage of the laxity. That I am not singular in thinking lightly of this great duty, will appear from the following anecdote of the late Dr. Gregory, which I have heard from the benignant lips of the present occupier of his Chair, the almost fabulous strength of whose conservative principles is attested by the circumstance that they could bend his noble nature to a seeming acquiescence in these proceedings of the Faculty. That celebrated professor on being condoled with by a friend on the labour of perusing so many long dissertations as were piled on his table, replied to the following effect:—  
 "You don't suppose I read them through! Do you eat

up a whole gigot in order to know the quality of your mutton? A slice is enough." As for me, I may think our inaugural mutton not always of the best quality, but if it be as good as our pastures at present afford, I am bound to be satisfied, trusting that the progress of scientific culture will provide us by and by with a superior description of both.

These remarks should be enough to satisfy every temperate and reflecting man, whatever his medical creed may be, that there is no inconsistency between my opinions and my University duties, and that they furnish no grounds either for a legal deposition by the Patrons, or for my voluntary departure from the Chair which I occupy. I care as little for the emoluments and accessories of a Chair as any man in the University,—but to quit spontaneously that which I possess, would be a confession that one entertaining my opinions was disqualified for holding it or any other medical professorship. Anything so false and absurd I shall never admit. There is no confession of faith in medicine to which a man is bound to adhere, and by which he can be tried. Like all other natural sciences, it is progressive, and therefore subject to continual change. "Theories," says Liebig, "are true but for the time;" the subjects of knowledge increase in number and extent, and the principles which spring from them alter and multiply in a proportionate degree; so that no one stereotypes his knowledge, or ought not, on being made a professor. "The education of a man of open mind is never ended;" and in medicine, of all sciences, it never ought to end; nor is there a restriction on its progress in the public any more than in the private ranks of his profession, save what the physician himself chooses to impose.

So much for the first charge which the Faculty attach

to the public profession of Homœopathy. The second, which affirms that it "is calculated to injure the University as a Medical School," may, I think, be disposed of with equal ease. We have had now an experience of seven years by which to test the influence of the public avowal of my opinions on the prosperity of the University; for ever since the summer of 1844, when I resigned the Professorship of Clinical Medicine, in consequence of my adoption of Homœopathy, my views have been sufficiently known to have attracted the frequent notice of Allopathic journalists and physicians. On comparing the number of medical students registered at the University annually during the seven years which ended in 1844, with the annual registrations in the seven years which have since elapsed, we obtain data for a fair estimate of the amount of injury likely to accrue from the cause which the Faculty profess to dread.

The number of medical students was as follows:—

1838 ... 575 students.	1842 ... 451 students.
1839 ... 556    ,,	1843 ... 351    ,,
1840 ... 467    ,,	1844 ... 331    ,,
1841 ... 433    ,,	

Thus the decrease on the seven years was 244. Contrasted with these figures we have the following:—

1845 ... 358 students.	1849 ... 368 students.
1846 ... 316    ,,	1850 ... 412    ,,
1847 ... 346    ,,	1851 ... 413    ,,
1848 ... 361    ,,	

There has therefore been an increase of 82 students at the last annual registration as compared with that of 1844. Had the numbers continued to decrease since the latter date at the same rate as they had previously done for seven years, we should have had to rejoice last session over 87 students instead of 413, or somewhere about  $6\frac{3}{4}$  a-piece.

Again, on comparing the number of graduates annually

made during the period of six years ending in 1844, with that of the seven years since, we find that there were—

1839 ... 119 graduates.	1842 ... 86 graduates.
1840 ... 111 „	1843 ... 91 „
1841 ... 103 „	1844 ... 66 „

being a decrease of 53.

1845 ... 79 graduates.	1849 ... 50 graduates.
1846 ... 64 „	1850 ... 67 „
1847 ... 58 „	1851 ... 45 „
1848 ... 63 „	

In the last period, therefore, there has been a considerable fluctuation, and an average decrease between its lowest number and the number in 1844 of three annually; while in the former period the annual decrease was, with one exception, a constant occurrence, and the average nine. Had the like annual decrease happened since 1844, we should have had at last graduation three graduates instead of forty-five. It may not be without interest, in connexion with the point now under consideration, to advert to the statistics of graduation at Glasgow during corresponding periods. In the six years which ended in 1844 the number of graduates at that university had decreased from 117 to 45, and in 1850 it had further decreased to 29; what the number is for the present year I have not learned.

It is not my business to detail the causes which have been in operation during the last fourteen years to alter the condition of the medical schools in these two universities,—to enumerate the new schools which have sprung up in England and in Ireland to give instruction in their native countries to those who were formerly obliged to seek it at a distance,—to tell of the new colleges where graduates are made, and the easier terms on which university honours may be procured. It is enough for me to say that

such causes have all come into operation within the period to which I have adverted, in order to make it a matter of satisfaction that in Edinburgh we are still so prosperous, whilst other schools have been nearly or altogether ruined by the competition. That I am not alone in supposing that such causes have proved injurious to our medical school, by lessening the numbers of our graduates at least, appears from the following expressive passage by the Professor of Clinical Surgery, who in 1840 discovered among "the many adverse influences at present co-operating against the prosperity of our University," (Homœopathy was not then in the way, otherwise it would have come in for its share of the blame,) these several moral and medical evils "that the University of St. Andrews, *which possesses no Medical School*, by a shameful prostitution of her privileges, confers the degree of M.D. on candidates who have never studied a day within her walls; that students are drafted over from Edinburgh several times a year for the purpose of receiving this spurious honour; that there are agents having the name of Examiners, resident in Edinburgh, who pander to the authors, and accompany the victims, of a sordid policy," &c.\*

Should the number of graduates, or of registered medical students, henceforth decrease at a rate greater than was observed prior to 1844, (which was the first session of my connexion with the University,) it will become reasonable to suspect that some new cause shall have come into injurious operation, and it will be a fair subject for speculation to determine what that cause may be. That it cannot be my "public profession of Homœopathy," the facts I have adduced are sufficient to prove. That has stood the test of seven long years, and has not prevented an increase

\* Letter to the Lord Provost, &c., p. 8.

in the number of students. If we are to begin a course of rapid decline, the history of the last session must furnish the cause; and those who are curious in such inquiries will have to settle the contending claims of the well-known mesmeric soirées of the Professor of Midwifery, of the "Letters to a Candid Inquirer" of the Professor of Chemistry, with all their spiritual wonders of animal magnetism and second-sight, and though last, certainly not least, of the late conduct of the Medical Faculty, which cannot but abate that *prestige* of dignity and scientific wisdom which had descended to them from their venerated predecessors. I refer here not merely to the Resolutions, but to the treatment of the candidate who was lately rejected, for the time, partly on the ground that he admitted his intention to study Homœopathy. When it is considered that the number of adherents which Homœopathy possesses among the public is now very large, and that among its supporters are not merely men of rank, but many others favourably known by their attainments and good sense, and some highly distinguished by their literary or philosophical talents, it will be admitted that our medical teachers cannot be expected to vilify Homœopathy, or to close their doors against those who are inclined to practise it, without exciting either hostility or contempt among classes whose estimation cannot be safely disregarded. If the Medical Faculty would act wisely and fairly in reference to Homœopathy, and to candidates for their degree, they should test experimentally the claims of the system they repudiate before they venture to condemn it. It will not do to sneer at it as a mere system of infinitesimal doses, for should any one affirm it to be so, I am entitled to tell him that, if he do so without having studied the subject, he speaks in ignorance, let him pronounce his opinion with what pretentious pomp he may; and that if

he do so after having studied the subject, he says what he must know to be untrue. While we maintain that no College or Faculty in existence is in a condition to decide what may be the smallest quantity of a medicine that is capable of acting on the sick, and that doses very much smaller than our Colleges and Faculties have any conception of, actually do operate as remedies, the whole question of doses is left open to the discretion and experience of the physician, who may be as strictly a Homœopathist as Hahnemann himself, though he should give his medicines in the ordinary form, simply if he select them according to their Homœopathic fitness. This alone is Homœopathy, and it has been studied, or the study of it recommended, by men quite as considerable in social and scientific rank as any member of the Faculty. Our late townsman Dr. Andrew Combe was a good specimen of the more philosophic physicians, and he wrote of Homœopathy thus:—"I am not, and for a long time have not been, *hostile* to Homœopathy. I have long thought that the Homœopathists have made out a case for *serious inquiry*, and on that ground urged our medical nephews to avail themselves of the opportunities presented to them to investigate its claims, and verify them in practical observation. If I were to continue in my profession, I should consider it a duty to *test* these claims."\*

Brera, the Professor of Practice of Physic and Clinical Medicine in the University of Padua, one of the most eminent examples of the practical physician, wrote of Homœopathy in these terms:—"Homœopathy, though it may appear vain to some, singular to others, and extravagant to a great number, does actually reign in the scientific world just like any other school; for it has its Chairs, its Books, its Journals, its Hospitals, its Cliniques, its Professors

\* Combe's Life, p. 456.

who teach, and its Public who listen. . . . Such being the case, it can no longer be treated with contempt; it merits that calm examination, that severity of judgment which have been applied to all systems of medicine successively,"\* &c. &c.

The late Mr. Liston, Professor of Clinical Surgery in University College, London, was the finest example of the practical surgeon this country ever produced, and his opinion of Homœopathy appears in the following extract from his Lectures. After detailing the particulars of the cure of cases of erysipelas, which he had treated with Homœopathic remedies, that great surgeon thus addressed his pupils:—  
 “Of course we cannot pretend to say positively in what way this effect is produced, but it seems almost to act by magic; however, so long as we benefit our patients by the treatment we pursue, we have no right to condemn the principles upon which this treatment is recommended and pursued. You know that this medicine (Belladonna) is recommended by Homœopathists in this affection, because it produces on the skin a fiery eruption or efflorescence, accompanied by inflammatory fever. *Similia similibus curantur*, say they. . . . I believe in the Homœopathic doctrines to a certain extent, but I cannot as yet, from inexperience on the subject, go the length its advocates would wish, in so far as regards the very minute doses of some of their medicines. The medicines in the above cases were certainly given in much smaller doses than have hitherto ever been prescribed. The beneficial effects, as you witnessed, are unquestionable. I have, however, seen similar good effects of the Belladonna, prepared according to the Homœopathic Pharmacopœia, in a case of very severe erysipelas of the head and face, under the care of my

\* British Journal of Homœopathy, April 1848.



friend Dr. Quin. The inflammatory symptoms and local signs disappeared with very great rapidity. Without adopting the theory of this medical sect, you ought not to reject its doctrines without due examination and inquiry."\*

The Faculty may decline to follow the advice or example of these distinguished men, but it is far from being likely that the students in our schools will read with indifference those deliberate statements from men who stood among the foremost in their several departments, or that they shall be insensible to the suggestions of curiosity when aroused by such weighty testimony. To oppose penalties and regulations to the beliefs or purposes of their pupils, will soon be found a vain expedient for cutting off the supply of qualified and licensed practitioners to the ranks of the rising system, and we hope to be spared the annual recurrence of scenes which place this city, that was wont to take a first place in great intellectual and scientific movements, not only behind other seats of learning in free countries, but actually behind such dens of despotism as Vienna and St. Petersburg. In both these cities the young science which struggles in Britain for bare toleration, under the ban of Colleges and threatenings of pains and penalties, is sanctioned by the authority and protected by the strong arm of the State. The *Odiū Medicū* can be more tyrannical than the employers of Haynau, and more intolerant than the imperial jailor of Siberia.†

It may not be easily believed that the prompter of the notable Resolutions of the Faculty is the conductor of a Medical print published weekly in London, and

\* Lancet, 1836.

† That very Dr. Arneth who, last winter, was so much thought of by some members of the Edinburgh Faculty, and so complimented by an Edinburgh Medico-Chirurgical Society, is the person alluded to in the following

## CORRESPONDENCE, &C.

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5 ST JAMES' SQUARE, 21st Nov., 1851.

SIR,

I have been favoured with notes of a recent lecture by you (taken during its delivery), on the subject of Homœopathy, in which I find the following passage:—"Edinburgh has been charged with sending forth eight Homœopathic Graduates out of the forty-eight who graduated here last year; but I hardly believe this. If it is the case, they must have concealed from their examiners their true sentiments, and, for base ends, have declared themselves adherents of a system they believed to be false."

As I am one of those who believed in the superiority of the Homœopathic system, at the time of graduation, I deem myself entitled to inquire whether the passage referred to contains a substantially correct report of what you said.

I am, Sir,

Your obedient servant,

GEORGE E. STEWART.

To Dr Christison.

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MORAY PLACE, 22d Nov., 1851.

SIR,

I am sorry indeed that any Graduate of the University of Edinburgh should have felt the necessity of making such a confession as that which I have received from you this morning.

The cautions which I considered it my duty to give my students on the subject of Homœopathy were only in part written, and that portion in many places not delivered as it was written: consequently, I cannot be certain of the terms actually used by me. I am quite certain, however, that the passage adverted to in your letter was not delivered in the terms in which you say it has been reported to you. As to the purport of the passage, my object was to express disapprobation of the conduct of any candidate for the medical degree of this university—if, indeed, there was any such—who in his examinations should give his examiners to understand that he believed in rational medicine and would practise it, while in his secret heart he believed in Homœopathy, or, at least, intended to practise according to its principles.

I shall be sorry if, in expressing this disapprobation, I used language which could be justly interpreted as imputing base motives to one so circumstanced, because, when reluctantly compelled by my position to discuss this miserable subject, my wish has always been to state facts, but to leave it to my hearers to judge of motives for themselves; and if your informant was as anxious to report what might soothe, as to convey what would irritate, those whom I could not but censure, he must have told you that, at the close of my remarks, I begged to recall any statement I might have made which might seem to impute base motives to any individual professing Homœopathic principles, and to avow my willingness to believe that many Homœopaths are honest men, afflicted with lamentable delusions.

I beg to add, that our correspondence on this subject must here terminate.

I am,

Your obedient servant,

R. CHRISTISON.

Dr Stewart.

5 ST JAMES' SQUARE, 29th Nov., 1851.

SIR,

Your letter of the 22d, of which I have to acknowledge the receipt, is so far satisfactory, that you disclaim any intention, in your recent lectures on the subject of Homœopathy, of imputing base motives to any individual professing Homœopathic principles. At the same time, I cannot but regret that, your lecture having been unwritten, and your recollection of it, on your own statement, so imperfect, you are not in a position to give a more explicit contradiction to the false and calumnious statement attributed to you in the extract from the notes of the lecture which I quoted. You rather tell me what, according to your present belief, it was your intention to say, than what you did say on the occasion, so that I must regard the report in the notes as having every chance of being nearer the truth, especially as they do not fail to contain the soothing remarks at the close of your lecture alluded to in your letter. Under the circumstances, finding the impression conveyed by the notes to have been also left on the minds of not a few of the students, I think it due to myself and the other Graduates referred to, to make our communications public. This will explain to you why I do not comply with your desire that the correspondence should terminate with your note.

In order to correct any false impressions which may have been left either on your own mind or on the minds of your students, I have now to state emphatically, that on no occasion during my examinations with a view to graduation did I ever make the slightest profession of any belief which I did not entertain. I had previously become fully satisfied of the truth and importance of the Homœopathic law, and of the efficacy of infinitesimal doses, *given in accordance with it*; but on neither of these subjects was I, directly or indirectly, asked any questions; and it was certainly not my part to propose subjects for examination.

Indeed, so entirely satisfied am I that, in such examinations, any questions tending to elicit a confession of belief, or of in-

tentions in reference to practice, are entirely out of place and irregular, that I was quite prepared, had any such been put to me, to decline answering them upon that ground, and to abide the consequences, taking my appeal, in case of necessity, to the Senatus and Patrons of the University.

A candidate for a Degree is not, in my estimation, to be regarded as pledging himself to practise at all, much less to follow any particular method. He rests his claim for a diploma on the fact of his having, in the acquisition of medical knowledge, spent both time and money, for which the diploma is the receipt. If the examiners find, by his answers to their questions, that he has prosecuted his studies successfully, they recommend him for graduation, and here their responsibility ends. For his future conduct as a practitioner, he is answerable only to his own conscience and the laws of his country; and to neither of these tribunals need he fear to appeal, when the charge brought against him is that he has investigated a plan of treatment which he had heard stated to be superior to that he had been taught at college, and that, finding this declaration true, he dares to act upon it.

In regard to the number of graduates, I am quite *certain* that five were believers in Homœopathy at the time of graduation, and are now prosecuting its study, with a view to practice. I also know of as many more who had *then* a leaning to it, and who, I have no doubt, are now daily becoming more convinced of its superior efficacy.

I cannot but remark, also, that any such observations upon the conduct of those who are now Graduates of the University and your professional brethren, as were made by you, on your own confession, in the absence of those to whom they applied, are, in my opinion, highly undignified and indiscreet, as well as un-academical.

Having made this statement, I should have here ended, had you confined yourself to the single point adverted to in my first note, and to which the preceding remarks apply; but, as you have taken the opportunity to express your regret that any

Graduate of the University of Edinburgh should have made the confession of his belief in the superiority of the Homœopathic mode of treatment, I also shall take occasion to make a few remarks on that subject.

It appears that in your view Homœopathy and Rational Medicine are opposed; in mine, they are identified. Having pursued, with a fair measure of assiduity and success, my studies at the University—having had the advantage of the lessons of its teachers both as to the theory and the practice of therapeutics, I was fortunate enough to become acquainted with a mode of treatment not taught in the University, and, as it appears, unknown or misunderstood by most of the Professors there; among whom, from the terms of your inaugural address, and from the notes I have seen of your lectures, I must believe you to be *as yet* included. I have thus had an opportunity of comparing for myself the Homœopathic with the more generally received modes of treatment—which you appear not to have enjoyed—and the result has been a firm conviction of the vastly superior efficacy of Homœopathy. But, in thus learning Homœopathy, I have unlearned nothing valuable; and I am anxious to express my sense of obligation to those teachers from whom I have derived so much useful knowledge, without which I could not have occupied my present position. Surely a rational medicine, or, as I understand the term, one derived from reasoning on *facts*, is more likely to be found when the individual in search of it tries the merits of all the systems of medicine brought under his notice before he adopts any, than when, without such inquiry, he clings blindly to that which his fathers believed, and which he happens to have been first taught.

I do not see, however, why, because I believe the Homœopathic method of treatment so superior to all others as to consider it my privilege and my duty to practise it, I am at all bound to maintain that every other must of necessity be useless. Because I hold that with small doses, administered on the principle "*Similia similibus curantur*," cures are effected most speedily, and with greatest certainty and safety, I am by no

means to be considered as denying that large doses, administered on some other principle or variety of principles, are capable of, to any extent, promoting recovery, far less as refusing to acknowledge that an amount of benefit may flow in some instances from the use of blisters, &c. ; in fact, I am rather inclined to look upon blistering, and other means of counter-irritation, as a coarse kind of Homœopathy. There is as much ground for the conclusion that, because I believe the affluent now cross the Atlantic in steamers, I must deny that sailing vessels also carry their human freight.

It might, moreover, occur to a Homœopathist to be called on to treat a patient when he had no Homœopathic medicines at hand, and no means of obtaining them. In that case, if he has seen a patient, similarly affected, derive benefit from Allopathic, or any other treatment, he is bound, if he can, to avail himself of the plan he had known to do some good, while still he deeply regrets that what he has discovered to be the more effectual method (the Homœopathic) is impracticable.

Again, Homœopathy is still imperfect—is, indeed, only in its infancy, as admitted by all its advocates, in spite of your statement, as got from the notes, that “it came forth full-fledged, and has grown none since” If this imperfection should be found, in any case, to be so great as to render the practitioner unable to engage in combat with the disease homœopathically, I have no doubt he would resort to the more blunt and unwieldy weapons of Allopathy ; and, in so far as he is a Homœopathist, there is no inconsistency. But, with all the imperfections of their system, I do not think Homœopathists need fear that they will often require to have recourse to such a shift.

In your letter, you speak of Homœopathy as a “*miserable* subject.” If you mean that it has been, and threatens to be to a still greater extent, a source of misery to you and your “legitimate” professional brethren, I cannot say that I see any grounds for taking objection to your statement. The “Lancet” appears to me particularly to be writhing under its inflictions, and I am warranted in announcing a still greater host of tor-

ments for him and "his" at no distant date. But if, by the expression, you intend to imply that Homœopathy contains in itself only misery, I at once beg to dissent from you. To Homœopathists it is a fertile source of gratification, as it at present stands, and it points their hopes to the still higher pleasure they may gain, if they will only prosecute the study of it with diligence and sagacity in spite of persecution from those who, in ignorance, oppose it.

Throughout the whole course of your remarks on Homœopathy, you appear, from the notes of them with which I have been furnished, to have fallen into your old error—and the error of almost all Allopathists who have attacked Homœopathy—of regarding it as merely a system of infinitesimal doses. I had thought that Professor Henderson's pithy hint to you on that mode of treating the subject, in his letter to the Patrons of the University, would have led you to take up a better stand when you next entered the field against Homœopathy. But perhaps you thought your remarks, in this instance, were not likely to acquire the same publicity as your former ones. At all events, "the burden of your song" in your own class-room, as in that of Professor Gregory, has evidently been small doses. You do, it is true, say (I copy from the notes), that "many unprofessional individuals have been convinced of its [Homœopathy's] falsity and delusiveness by observation and experiment, but I have never heard of one being convinced of its truth by argument." Now, why did you go to *unprofessional* persons for your disproof of Homœopathy by observation and experiment—the tests which Homœopathy desires to be tried by—although, in my opinion, she may safely flatter herself as having "the best of it" on the arena of argument also? Why not have undertaken the task of observing and experimenting yourself, or have got some Allopathist, half as famous as yourself in such a work, to do it for you, and thereby have at once brought an end to the throes of the *miserable* subject?

You once had a good chance. When Dr Henderson was one of the professors of clinical medicine—before *he resigned* that office, would it not have been a good, an honest plan, to



have attempted to get *facts* either in support or disproof of Homœopathy? You might have got a ward set apart for cases to be treated by him, in accordance with the law "*similia similibus*," under your own inspection, and that of as many Allopathists as chose to come. The cases at first given to him might have been selected from among those patients whose diseases were of a kind to admit of being left to nature for a time without danger, or from among the not usually inconsiderable number, in regard to the treatment of whom you and your other colleagues were quite at sea. If the results had been favourable, which I have no doubt would have happened, you could then, perhaps, have mustered courage to go a step farther, by allowing the next allotment to be cases of a different kind—those over whose complaints you were conscious of having some control.

In your "Dispensatory," in speaking of belladonna as a prophylactic against scarlatina, you say—"The alleged property [of preventing the invasion of scarlatina] has, however, been generally disbelieved in this country, mainly, perhaps, in consideration of the individual who first announced it, and who made use of the facts in his own way to support his wild theories respecting Homœopathy. But, as the treatment is simple and safe, it seems irrational not to give it a fair trial, so long as there are hundreds of public institutions for children in Britain that every now and then are overrun and decimated by scarlatina."

In regard to your statement in the above extract, it is gratifying to observe that you award to Hahnemann his due; for we have found several journalists denying his claim to the discovery, and thus, by the by, fulfilling one of the conditions of a true discovery which you gave us in your inaugural address, that it must be denied before being generally accepted, and also fulfilling the following prediction given in the "Introduction to the Study of Homœopathy," edited by J. J. Drysdale, M.D., Ed., and J. Rutherford Russell, M.D., Ed. 1845. P. 14:—

"This much, however, in connection with the point, may be pre-

dicted, without any arrogant assumption of the prophetic mantle, that the times are not, perhaps, very distant when it shall fare with Hahnemann's discovery much as it did with Columbus' egg. All the Salamanca Bachelors and Theologians were able to make the egg stand after Columbus had knocked the big end of it into a provisional and very primitive sort of a pedestal. Let only Homœopathy force its way into Academies and Universities, and Hahnemann's discovery will turn out no discovery at all. The present hue and cry will lull itself into the professorial drawl of—“We knew as much.”

To rectify a misapprehension which may, however, be conveyed to your readers by the expression you employ in reference to the discovery of belladonna as a prophylactic, I may quote Hahnemann's own words to show that the discovery was *really* made by the light of the *previously discovered* Homœopathic law:—

“I shall now relate the mode in which I made the discovery of this specific preservative remedy. The mother of a large family, at the commencement of July, 1799, when the scarlet fever was most prevalent and fatal, had got a new counterpane made up by a sempstress, who, without the knowledge of the former, had in her small chamber a boy just recovering from scarlet fever. The first-mentioned woman, on receiving it, examined it, and smelt it, in order to ascertain whether it might not have a bad smell, that would render it necessary to hang it in the open air; but, as she could detect nothing of the sort, she laid it beside her on the pillow of the sofa, on which, some hours later, she lay down for her afternoon's nap. She had unconsciously, in this way only (for the family had no other near or remote connection with scarlatina patients), imbibed this miasm. A week subsequently, she suddenly fell ill of a bad quinsy, with the characteristic shooting pains in the throat, which could only be subdued after four days of threatening symptoms.

Several days thereafter, her daughter, ten years of age, infected most probably by the morbid exhalations of the mother, or by the emanations from the counterpane, was attacked in the evening by severe pressive pain in the abdomen, with biting itching on the body and head, and rigor over the head and arms, and with paralytic stiffness of the joints. She slept very restlessly during the night, with frightful dreams, and perspiration all over the body, excepting the head. I found her in the morning with pressive headache, dimness of vision, slimy tongue, some ptyalism, the submaxillary glands hard, swollen, painful to the touch, shooting pains in the throat on swallowing, and at other times. She had not the slightest thirst; her pulse was quick and small; breathing hurried and anxious; though she was very pale, she felt hot to the touch, yet complained of horripilation over the face and hairy scalp; she sat leaning somewhat forwards in

order to avoid the shooting in the abdomen, which she felt most acutely when stretching or bending back the body; she complained of a paralytic stiffness of the limbs, with an air of the most dejected pusillanimity, and shunned all conversation: "She felt," she said, "as if she could only speak in a whisper." Her look was dull, and yet staring; the eyelids inordinately wide open; the face pale; features sunk.

Now I knew only too well that the ordinary favourite remedies, as in many other cases, so also in scarlatina, in the most favourable cases, leave everything unchanged, and, therefore, I resolved, in this case of scarlet fever, just in the act of breaking out, not to act as usual in reference to individual symptoms, but if possible (in accordance with my new synthetical principle) to obtain a remedy whose peculiar mode of action was calculated to produce in the healthy body most of the morbid symptoms which I observed combined in this disease. My memory and my written collection of the peculiar effects of some medicines furnished me with no remedy so capable of producing a counterpart of the symptoms here present as belladonna. It alone could fulfil most of the indications of this disease, being that, in its primary action it has, according to my observations, a tendency to excite in healthy persons great dejected pusillanimity, dull, staring (stupid) look, with inordinately opened eyelids; obscuration of vision, coldness, and paleness of the face, want of thirst, excessively small, rapid pulse, paralytic immobility of the limbs, obstructed swallowing, with shooting pains in the parotid glands, pressive headache, constrictive pains in the abdomen, which became intolerable in any other posture of the body, besides bending forwards, rigor and heat of certain parts to the exclusion of others, *e. g.*, of the head alone, of the arms alone, &c. If, thought I, this was a case of approaching scarlet fever, as I considered was most probable, the subsequent effects peculiar to this plant, its power to produce synochus, with erysipelatous spots on the skin, sopor swollen, hot face, &c., could not fail to be extremely appropriate to the symptoms of fully developed scarlatina.

I therefore gave this girl of ten years of age, who was already affected by the first symptoms of scarlet fever, a dose of this medicine ( $\frac{1}{32,000}$ th part of a grain of the extract, which, according to my subsequent experience, is rather too large a dose.\*) She remained quietly seated all day without lying down; the heat of her body became but little observable. She drank but little. None of the other symptoms increased that day, and no new ones occurred. She

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\* "At least if given for a preventive object, too large a dose for a child of this age, but probably exactly appropriate for the so far advanced symptoms of scarlet fever; but this I do not know for certain. I cannot therefore advise an exact imitation of this case; but yet neither can I advise that it should not be copied; for the scarlet fever is a much more serious evil than a few troublesome symptoms produced by a somewhat too large dose of belladonna."

slept pretty quietly during the night ; and the following morning, twenty hours after taking the medicine, most of the symptoms had disappeared without any crisis; the sore throat alone persisted, but with diminished severity, until the evening, when it too went off. The following day she was lively; ate and played again, and complained of nothing. I now gave her another dose, and she remained well, perfectly well, whilst two other children of the family fell ill of bad scarlet fever without my knowledge, whom I could only treat according to my general plan detailed above. I gave my convalescent a smaller dose of belladonna every three or four days, and she remained in perfect health.

I now earnestly desired to be able, if possible, to preserve the other five children of the family perfectly free from infection. Their removal was impossible, and would have been too late. I reasoned thus: A remedy that is capable of quickly checking a disease in its onset, must be its best preventive; and the following occurrence strengthened me in the correctness of this conclusion. Some weeks previously, three children of another family lay ill of a very bad scarlet fever; the eldest daughter alone, who up to that period had been taking belladonna internally for an external affection on the joints of her fingers, to my great astonishment did not catch the fever, although, during the prevalence of other epidemics, she had always been the first to take them. This circumstance completely confirmed my idea. I now hesitated not to administer to the other five children of this numerous family this divine remedy as a preservative, in very small doses; and, as the peculiar action of this plant does not last above three days, I repeated the dose every seventy-two hours; and they all remained perfectly well, without the slightest symptoms throughout the whole course of the epidemic, and amid the most virulent scarlatina emanations from their sisters who lay ill with the disease.

In the meantime, I was called on to attend another family, where the eldest son was ill of scarlet fever. I found him in the height of the fever, and with the eruption on the chest and arms. He was seriously ill, and the time was consequently past to give him the specific prophylactic remedy. But I wished to keep the other three children free from this malignant disease. One of them was nine months, another two years, and a third four years of age. The parents did what I ordered; gave each of the children the requisite quantity of belladonna every three days, and had the happiness to preserve these three children free from the pestilential disease, free from all its symptoms, although they had unrestricted intercourse with their sick brother.

And a number of other opportunities presented themselves to me, where this specific preventive remedy never failed.\*

\* The Lesser Writings of Samuel Hahnemann. Collected and Translated by R. E. Dudgeon, M.D. London: William Headland, 15 Princes Street, Hanover Square. 1851. Pp. 353-357.

Why not carry the same spirit, which is indicated in the above extract, with you in all your dealings with Homœopathy? On your own statement, all Homœopathic treatment is simple and safe; and I have no doubt you will confess, that cases almost daily present themselves to you to which you are at a loss what Allopathic, and therefore comparatively heroic, treatment you can apply. If such be the case, is it not irrational to refuse Homœopathic treatment a fair trial?

Had you acted in the matter of Dr Henderson's clinical professorship with the same liberality, as is indicated by the passage above quoted from your own work, I feel assured that you would now have occupied a much more favourable and dignified position, when entering upon a consideration of the merits of Homœopathy.

As I have already said, the thorn which galls you in connection with the subject of Homœopathy, is the fact that it is so intimately related to small doses. You are reported in the notes as having said—"Homœopathists reduce the dose infinitesimally. Some will tell you that the question of doses is left open. This is not true. It is all very well for some to wish that certain parts of the system had been modified, but they must take Homœopathy as it was made for them." Indeed! Then Hahnemann's writings must be taken by all Homœopathists as the canon of plenary inspiration on the subject of Homœopathy. Why, then, have the wilful Homœopathists made a distinction between Hahnemannism and Homœopathy, and not stuck to the former only, or, what would have been more rational, discarded all the truth they find in Homœopathy, because there is also found interspersed a considerable amount of error? They should have made no allowance for the false notions which Hahnemann necessarily imbibed from the age in which he lived, and the extravagances into which, like all who have been benefactors of the species by the discovery of great and important general laws, he may have been led in his enthusiasm for making his discovery still more perfect! Because we accept Newton's law of gravitation, we do not think it neces-

sary to make a little hole for the kitten, as well as a large one for the cat (which is told of him), or even to consent to his interpretations of the Prophecies of Daniel.

Have you taken the system of therapeutics given you by your teachers, and kept it sacred in the state in which it was when committed to you? Does any Allopathist profess to do so? Have no alterations, no improvements, been made on it? Why, then, all your investigations, all your writings in favour of changes in formerly received notions? If all who were at one time your fellow-students, and who received the same instructions from the same teachers, had acted on this plan, how could there have arisen among them so many opposite views in regard to the treatment of the same cases? It will not do for you merely to assert, "It is not true that the question of doses is left open." I can affirm the contrary, and go a step farther on my side of the question than you thought fit, or were able to do on yours—namely, by bringing forward a few facts in support of my declaration.

All Homœopathsists who have expressed an opinion on the subject, so far as I know, contradict your assertion. Perhaps you can point me to one who does not. I am not ignorant of the fact, that some Homœopathsists are great advocates of high dilutions (infinitesimal doses) in the majority of cases; but I also know that there are many, especially among Continental physicians, who prefer in most cases the mother tinctures, or large doses. If you would read even the periodical literature of Homœopathy, you would see this to be true. Even those who have in their experience found that these doses are generally adequate to effect a cure, do not bind themselves invariably to adhere to them. In fact, a Homœopathsist, in so far as he is merely entitled to that designation, is quite at liberty to give a puncheon of physic if he sees it necessary. But why do Homœopathsists, in the majority of cases, only give infinitesimal doses, perhaps you may ask. It is simply because experience has proved them to be quite sufficient.

If you had studied Hahnemann's writings sufficiently, you

would have found that he himself gave large doses at first. For your benefit, I will here insert a case in which he did so. I have extracted it from "The Lesser Writings of Samuel Hahnemann," formerly alluded to:—

#### CASE OF RAPIDLY CURED COLICODYNIA.

"L——ie, a compositor, 24 years of age, lean, of a pale, earthy complexion, had worked at the printing-press a year and a half before he came to me, and then for the first time suddenly felt great pain in the left side, which obliged him to keep his bed, and which, after several days, went away under the use of ordinary medicines. Ever since that, however, he had experienced a dull, disagreeable sensation in the left hypochondrium. Some months afterwards, when he had overloaded his stomach with sweet beer soup flavoured with caraway, he was attacked with a severe colic, the violence of which he could not express, but at the same time could not say whether it corresponded with the colicodynia which succeeded it. The attack passed off this time, I don't know how, but he observed that after it he could not bear certain kinds of food. The mischief increased unobserved, and the colicodynia, with its distinctive symptoms, took firm root. The worst kinds of food for him were carrots, all sorts of cabbage, especially white cabbage, and sour crout, and every species of fruit, but pears in particular. If he were so incautious as to eat any of these things within eight days after an attack which had been brought on by them, the liability was so increased, that he could not eat even a morsel of a pear, for example, one or two weeks after, without bringing on another severe attack.

The course of a severe attack was as follows:—Four hours or four hours and a half after eating of such food, having previously felt quite well, a certain movement was felt about the umbilical region; then there took place suddenly, always at the same place, a pinching, as if by pincers, but attended with the most intolerable pain, which lasted a half or a whole minute, and each time suddenly went away with borborygmus extending to the right groin about the region of the cœcum. When the attack was very bad, the pinching came back, and the subsequent borborygmus more and more frequently, until in the worst attacks they were almost constant. There occurred also the sensation of a constriction above and below, so that flatus could pass neither upwards nor downwards. The uneasiness and pains increased from hour to hour; the abdomen swelled and became painful to the touch. Along with all this suffering, which resembled a fever, there came an inclination to vomit, with sense of constriction of the chest; the breathing was shorter, and attended with more and more difficulty; cold sweat broke out, and there came on a sort of stupefaction, with total exhaustion. At this period, it was impossible for him to swallow a drop of liquid,

much less any solid food. Thus he lay, stupified and unconscious, with swollen face and protruded eyes, and without sleep, for many hours. The attack of spasmodic colic gradually subsided by diminution of the pain; then followed some escape of flatus, either upwards or downwards, and so the attack went off (sometimes only after sixteen or twenty-four hours from its commencement). The strength only returned after three or four days, and thus he was again like a person in health, without any other uneasiness except the dull, fixed pain before described, and general weakness and sickly appearance. He could not positively say whether this dull pain went off during the severe attacks or not, but he thought it did.

In these circumstances, he could not retain his situation at the printing-press; he became a compositor. The attacks always recurred under the condition described, and had continued to do so for more than a year, when he put himself under my care. It might easily be supposed that the attacks arose from flatulence; this, however, was not the case. He could take, without the least inconvenience, a good meal of dry peas, lentils, beans, or potatoes; and he was obliged to do so, moreover, as his position did not allow him the opportunity of getting much else. Or it might be supposed to arise from some kind of fermentation in the primæ viæ, or from some idiosyncrasy in respect to sweet things. But nothing was further from the case. He could take cakes baked with yeast and sugar and milk, as much as he pleased, even to satiety, without the slightest threatening of colic; although the first attack *seemed*, as I have said, to be occasioned by the beer soup.

Or could an injurious acidity have occurred within the four hours (for the attack *never* occurred sooner after partaking of the above things)? This was not the cause. Lemon-juice and vinegar were both innocuous. Neither did he ever vomit sour matter, either during the retching that occurred with the attack, or when ordered an emetic. None of the absorbent earths or alkalis were of any use to him, whether taken during or before the attack.

A physician had suspected tape-worm, and subjected him to Herrnschwand's treatment, without any result. Neither before nor after had he passed anything which had the smallest resemblance to a tape-worm, or indeed to any kind of worm at all. When he came to me, the idea of tape-worm had taken so firm a hold of his mind, that I was obliged to order him all that was peculiar in the methods of Nuffer, and of Clossius. He used all the medicines with patience, and pressed me to try every means with this view:—tartrate of antimony, gamboge, scammony, male fern (four ounces daily for four days together), charcoal, artemesia in large quantities, colocynth with oils, castor-oil, tin, iron, sabadilla, sulphur, petroleum, camphor, assafœtida, and laxative salts—nothing was left untried; but they were given, as I have said, rather on account of his urgent request than to satisfy my own conviction, for, besides the fact that no



worms were ever seen, the two symptoms which I have so often observed to attend worms were absent, viz., the deeply wrinkled countenance, and the sensation of a cold stream winding itself towards the back, immediately after a meal.

Immediately after the *sabadilla*, which produced a creeping sensation, like ants upon the skin (formication), and a heat in the stomach and over the whole body, I let him try the test of eating a piece of pear. It appeared, indeed, as if the attack had returned quite mildly, but after I had left him without medicine for eight days, and again tried him with a small piece of pear, the colic came on just as bad as ever.

I have forgotten to mention that I had already previously tried all sorts of powerful so-called anti-spasmodic remedies at the commencement of the paroxysm, small doses of *ipecacuanha* taken dry, lukewarm foot-baths, and larger baths, opium, and *cajuput* oil, without any result, even without any palliative effect. I only sought to palliate the symptoms at that time, in order that he might continue without molestation to use *cinchona* bark, and to wash with cold water, to get the better of his weakness. As his condition required immediate help, inasmuch as the *colicodynia* began to appear even upon the use of the smallest quantity of vegetable food, and as all I had done at his entreaty had been of no service whatever, I determined to give him a medicine, which produced very similar morbid symptoms. The similarity of the griping pain, anxiety, constriction of the chest, fever, loss of strength, &c., produced by *veratrum album*, appeared to me calculated to give permanent relief. I gave him four powders, each containing four grains, and told him to take one powder daily, but to let me know at once if any violent symptoms appeared. This he did not do. He did not return until five days thereafter. His unlimited confidence in my aid had nearly played him an awkward trick. The benefit I had promised from the powders had induced him to take two instead of one daily. After the second powder, without his having eaten anything injurious, there began an attack which he could not otherwise describe than as his spasmodic colic, or something very like it. This did not prevent him, however, from taking the third and fourth powder the following day (taking thus sixteen grains in rather less than two days), upon which this artificial colic, if I may so speak, increased to such a dreadful extent, that, to use his own expression, he wrestled with death, covered with cold sweat, and almost suffocated. He had required the remaining three days to recruit, and had returned for further directions. I reprimanded him for his imprudence, but could not avoid, notwithstanding, comforting him with the prospect of a good issue. The result confirmed it. Under the use of tolerably good diet, he regained his strength, and he has not had, for half a year, even a threatening of an attack, although from time to time he has eaten of the food which before was so injurious

to him, but in moderation, as I impressed upon him he should. Since this event, he has taken no more medicine, and no tape-worm was passed after the use of the veratrum. The dull pain in the left hypochondrium likewise went at the same time."

I think the above is calculated to show you more than that Hahnemann used large doses at first, and with success—namely, that he was possessed of sufficient observational powers and reflection to justify any one in giving heed to his statement, that he had discovered small doses to be generally all that was necessary. Want of time prevents me from giving you more cases of the same kind; but you will find others recorded in the Introduction to his "Materia Medica."

In regard to some medicines, and in some cases, the whole body of Homœopathic practitioners agree to give large doses. For instance, camphor is often so administered in cholera. The solution of this substance in almost universal use, in these cases, among Homœopaths, is much stronger than even the tincture of the Edinburgh Pharmacopœia. The former contains one part of camphor in every seven of the solution, while the latter has only one in every seventeen. The dose generally administered to cholera patients is two or three drops every five, ten, or fifteen minutes, according to circumstances.

I may also mention to you, that even your colleague, Dr Simpson, has publicly in his class-room confessed to the efficacy of a medicine administered by him in accordance with the Homœopathic law, at the suggestion of a Homœopathist, and in not *very* small doses either.

Towards the close of last winter session, when Dr Arneth, of Vienna, was in Edinburgh, he was in the habit of attending Dr Simpson's lectures. On one occasion, in Dr Arneth's presence, I recollect that Dr Simpson told his class of a case of vomiting during pregnancy then under his care. He had launched forth in succession all his Allopathic appliances—opium, naphtha, prussic acid, &c. (I forget if chloroform was also used)—but without relieving the patient. He had either taken Dr Arneth to see it, or had told him about it, and the

latter recommended him to try either a quarter or half a grain of Ipecacuanha ; I do not recollect which. He acted upon this suggestion, and with decided good results. Dr Simpson made a remark to the effect, that this, no doubt, looked very like Homœopathy, but yet that he was bound to acknowledge the fact.

You are also represented to me as having said, that one "peculiar branch" of the Homœopathic system "is that medicines destroy one another's effects." Is this peculiar to Homœopathy? Do you not hold that acids and alkalis neutralise each other's effects if they are mixed? But I suppose your object, in mentioning this *peculiarity*, had been to give a telling effect against Homœopathy to the following syllogism, which is noted as having been advanced:—"It follows from the known constitution of the atmosphere, that, by the prolonged and repeated trituration to which it is necessary to submit all Homœopathic medicines [not the tinctures, of course], in order to their infinitesimal division, ammonia (a very important Homœopathic remedy), and other matters, must be mixed—in very small quantity, no doubt, but of great consequence when dealing with infinitesimal doses; but medicines are rendered inert by mixture: therefore, all Homœopathic remedies are inert—a very possible conclusion."

Now, sir, it is not asserted by Homœopaths, nor is it a fact, that two medicines given at the same time are inert. What they say, and say truly, is, that, when you administer two medicines mixed together, you cannot assert that the effect of the mixture shall be the sum of the effects of each given singly; and that, unless you have proved the mixture on the healthy body, there is no room for the proper application of it in strict accordance with the Homœopathic law. They admit that, if the two remedies are antidotes, and the power of the one equal to that of the other, they will neutralise each other; but if two medicines are given which do not antidote each other, and one of them be Homœopathic, it will act in promoting recovery; while the other, not being Homœopathic, will not, from the smallness of the dose, produce any appreciable effect.

At the same time, the two *may* so interfere with each other's action, as not to lead to the curative result. In other cases this may not happen; but what these are, Homœopathists as yet do not know, and hence they prefer giving their medicines singly, or at most in alternation.

I grant it is not impossible, that, by trituration, the medicines may become mixed with ammonia; but, without descending to the pestle and mortar, you might have gained the same end, by informing your students that Homœopathists did not restrict their patients from breathing the air containing the ammonia while they were taking the medicines; so that ammonia and the medicine must be acting on the patient at the same time. What conclusion can you draw? Ammonia is unquestionably a valuable Homœopathic remedy, but *only* when it is administered in a case of disease to which it is Homœopathic. Were a person (to suppose a case) breathing atmospheric air containing no ammonia so affected by disease, that ammonia was indicated as the remedy according to the law *similia similibus*, I believe that even the amount of it he can inspire with the ordinary air, would effect his recovery; if it did not, I would exercise my right to increase the dose.

The same explanation—the fact of the Homœopathicity of the medicine to the disease—accounts for small doses taking effect, even when they are administered in common spring water, which generally contains much larger quantities of several other medicinal substances. These other substances have no appreciable effect, unless they are Homœopathic to the disease, because it requires larger doses to elicit phenomena from the healthy system, or from a condition of the system bearing other than a Homœopathic relation to the medicine—namely, not affected in a similar way to that in which, if healthy, it would have been by a sufficient dose of the medicine; in other words, not having its sensibility to the particular influence of the remedy exalted. No doubt the small doses will produce *some* effect even on a healthy organism, but it is so slight as to be imperceptible.

You say (in the notes):—"Reason and common sense must

reject the theory of infinitesimal doses. Homœopaths use the millionth, billionth, or even the trillionth part of a grain! Now, have you ever endeavoured to form an idea of such a dose? The question is, whether the human mind is capable of conceiving a clear idea of a trillionth; I confess that it conveys no very clear idea to my mind."

I must say, it is not obvious to me how reason either must, or can, reject the theory of infinitesimal doses. Reason has nothing to do with it; it is to be tested by observation and experiment. If you witness undoubted curative effects produced by these quantities, you are forced to believe that they are not powerless. There is no room for, no need of, reasoning; it essentially belongs to the category of axiomatic truths. No doubt, if you appeal to common sense—by which, in this case, I mean the sense common to Allopathists, that large doses of medicines are necessary for the cure of disease—the response must be, that infinitesimal doses are no doses at all, in so far as the capability of making any impression is concerned; but still in this there is no reasoning. Here there is a mere *petitio principii*—a begging of the question. If large doses are *alone* capable of influencing a disease, small doses *must* be ineffectual. No doubt of it; but first show that the notions of Allopathists on this point are right, then the whole matter will be set at rest—the dispute settled.

The question is *not* whether the human mind is capable of conceiving a clear idea, or *any* idea, of a trillionth; but is it capable of *seeing the effects* of a trillionth of a grain of medicine administered Homœopathically? To establish that lightning is a powerful agent, and capable of producing great destruction, it is not necessary to conceive an idea of the weight of a flash, or of the ingredient in it (if it be compound) which has the strength. It is enough to see the effects, or even to hear them related by credible witnesses. If you differ from me on this point, pray favour me with a statement of the exact weight, so that I may have a more perfect realisation of the devastating power so often displayed during a thunderstorm.

Is it necessary to conceive an idea of the weight or bulk of the emanations from a small-pox or typhus-fever patient, before I can believe in their power to produce such loathsome and deadly results? On this subject I may quote from a paper by Dr Samuel Brown, in "The Introduction to the Study of Homœopathy," already referred to.\*

"The very direction in which a power is applied, or in which a weight is allowed to operate, is so immensely more significant than the weight itself, that Archimedes, after having showered imponderable arrows of sunfire on the enemies of Syracuse, and burned up their vessels of war, wanted but a point to plant his lever, in order to move the world with his puny arm! What is the weight of water with which Watt clips thick iron, like paper, into shreds, and sends his huge leviathans, throbbing in their irresistible struggle, across the Atlantic, with all but the regularity of the freighted planets themselves? Are not a few pounds of weight transformed into tons, by the mere disposition of them by Bramah, on the principle of the old hydrostatic paradox? Paradox! One had thought the day of paradoxes was over for ever now. Everything great is a paradox at first; because our own ignorance makes it strange. To the last of the Ptolemaics it was paradoxical to think that the sky is not a hyaline vault, studded with heavenly lights. It was paradoxical to the scholastic contemporaries of Torricelli that nature can endure a vacuum. It was paradoxical to the Stahlian chemists that phlogiston is a nonentity. It was paradoxical to the Royal College of Physicians, that Harvey's circulation of the blood should not be persecuted, by exclusion from their precious fellowship as an irreligious heresy! It is our own limitation that is the originator of such paradoxes as these."

And again (pp. 169, 170) the same author writes:—

"Even unorganised nature, then, is an admirable commentary on the narrowness of such partial interpreters as insist upon reducing every manifestation of force to the standard of weight and measure. This, however, is not a physical, but a physiological inquiry. There is that which is a thousandfold more delicate and more susceptible of every influence, not substituted for, but super-added to and incorporated with, mere physical sensibility to reaction of every kind. Everything that has been said about material forms, into which the breath of life has not been inspired, must be affirmed, and more urgently affirmed of the living frame, with

\* The same articles contain many other beautiful illustrations of this subject, and in the "Introduction" there are many other interesting articles on Homœopathy.

its fearful though harmonious complication. The physician and his forces have to deal with a quivering epitome of all the species of susceptibility in creation, one kind reacting on another, so as to produce a combination of harmony so highly strung, that the prick of a pin shall grate upon every fibre, and a cooling odour, in a hot atmosphere, impart refreshment and delight to every nerve. According to the experiments of Leuchs, if the 10,240th part of a grain of tartrate of mercury be diffused through the substance of a sweet pea, the beautiful germ of a graceful flowering herb, which lies folded up within its horny pericarp, shall never come out and be expanded, though you imbed it in the softest mould, and solicit it by every art. Before Androclus will a lion, with a paltry thorn in his royal palm, crouch in his rock-built palace, and humbly crave deliverance from the insignificant prickle that has unstrung his fibrous frame. But man is a creature of such exquisite and manifold sensibility to the agency of even physical re-agents, that, when the compacted balance of all the parts is disturbed in any one way, and idiosyncrasy is produced, the feel of velvet produces nausea in some; a 'professor of natural philosophy faints under a sprig of lavender;' an Erasmus cannot as much as taste fish without a fever; a Cardinal Haüy de Cardonne swoons at the smell of a rose; a Scaliger falls into convulsions at the sight of cresses; and a Tycho Brahé trembles in the awful presence of a hare."

The testimony of Professor Jörg, of Leipsic—an *Allopathist*—may also be adduced in favour of *small doses* administered *Homœopathically*:—

"On the other hand, 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 the 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," he exclaims, "should I occupy time by 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 much 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." \*

From a still more recent work, I extract the following remarks, which appear to me to be particularly appropriate:—

"For, on the whole, the idea of medicine itself is homœopathic;

\* British Journal of Homœopathy. 1845. P. 48.

it does not give health-producing agents to engender health, but poisons which would issue in disease: it is, therefore, the general application of the law, by which like is to be cured by like. It is in the particulars that medicine does not recognise the application of the Hahnemannian formula; and thence, whenever it comes into details, it is in contradiction with its own idea. It is homœopathic in theory, and allopathic in application—a house divided against itself. And in the matter of doses it is subject to the like remarks; for no one gives physic in the same quantities as food, but a few grains of calomel, or a few fractions of a grain of arsenic, are considered sufficient even by ‘heroic practitioners’ of the old school. Why is this, but that there is a working in these poisons which takes them out of the category of the ordinary materials which we put into our mouths? And if a grain will produce results upon a man of fourteen stone weight, where is the absurdity to end, without experiment, which may choose to show that the millionth or decillionth of a grain will have even better results? I marvel how men who lift fourteen stones by the equipoise of a skilful grain, can sneer at other men who do the same nice balance by incalculably lesser weights. For it is evident that all medicine is on this railway of smallness, and is more perfect and harmless for every fresh terminus that it reaches. If the Allopathists were accustomed to give calomel porridges, their wrath against small doses would be consistent; but, when they are themselves reduced to grains, why should they cavil at other healers who, by experiment, have found out the value of grains of grains?”\*

Have you ever tried whether the millionth of a grain of a medicine administered in accordance with the Homœopathic law can produce any effect? If not, where is your right to assert the impossibility of its doing so?

If you, after having made up your mind in regard to the probable termination of a case under your care, Allopathically, were afterwards to consult a Homœopathist as to the remedy,—to obtain it from him,—and to administer it in infinitesimal doses; and if you were, after a succession of such experiments, to find uniformly no positive result, then you would have a strong case made out against Homœopathy; but you cannot get this in any other way. So many cases have been reported by

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\* The Human Body and its Connection with Man, Illustrated by the Principal Organs. By JAMES JOHN GARTH WILKINSON, Member of the Royal College of Surgeons of England. London: Chapman & Hall, 193 Piccadilly. 1851.



conscientious and able men, in which even decillionth grain doses administered Homœopathically, with the object of effecting a cure, have been so speedily followed by the desired result, that it would be considered rash, by all sound and independent minds, in any one to deny that they were the cause.

One strong argument against Homœopathy, in your opinion, appears to be that "it takes a long time to establish one remedy in regular practice." "Iodine," you say, "has been known for thirty-one years, and yet its action is still obscure. But in Homœopathy we are called upon to believe that one sagacious man did at once what many have been unable to accomplish in many years." To my mind, there is in all this no argument against Homœopathy at all; although, I must confess, there is in it great cause for admiration of Hahnemann. Do you really think this an infallible stumbling-block in the way of any one entering the list of Homœopathic practitioners? I can give you a clue to guide you through the whole difficulty. It is this: Hahnemann hit upon the right plan; the others to whom you refer did not. This plan was the proving of medicines on the healthy body in such a way as to ascertain, to use his own words, not only "the general characters of a substance, whether it has a power to produce purging, vomiting, convulsions, &c.; but all the finer shades of its operation, and its complete sphere of action." To this he was necessarily led, in consequence of the law *similia similibus*; for by no other means could it be rendered of practical value. His experiments for this purpose on himself and many of his friends were not made "at once," but extended over a period of more than thirty years. That he investigated patiently and recorded faithfully, is abundantly shown by the coincidence of his findings with those of the society instituted at Vienna, for the purpose of proving and adding to his results. Similar instances abound of one gifted individual having, in a short period of his lifetime, found out what many able men previously to him had in vain tried to discover. Such is Sir Isaac Newton's finding the course of each planet by the law of gravitation; and the names of Copernicus, Galileo,

and Torricelli, need only to be mentioned to call up other examples.

In conclusion, I have a few remarks to make on your statement, in the notes, that "there are no existing facts which go to support the truth of Homœopathy." Have you ever tried to obtain any? From what I have already said, you will now allow, perhaps, that there are *at least* a few; but I can even give you some from your own "Dispensatory."

To begin with tartar emetic, of which you say, in the notes, that "it has been adduced as an example of the Homœopathic method of cure; but it is very difficult to prove that it acts by producing inflammation." In your "Dispensatory" (p. 149), I find the following: "Internally, tartaric emetic is, in large doses, an irritant poison, which produces *inflammation* of the alimentary mucous membrane, and also, it appears, *of the texture of the lungs,*" &c. Farther on, in the same page, you write—"The same doses which, in the healthy state produce in general symptoms of irritant poisoning, unless speedily discharged by vomiting, appear frequently to exert a sedative or *controstimulant* [!] action during the presence of general reaction or active inflammation." This singular action, and the cure of *pneumonia*, pleurisy, and other acute internal inflammations, through its means, are facts in therapeutics, to the truth of which, however much they were at one time doubted in this country, it is now almost unnecessary for any author to bear personal testimony. The diseases in which the *controstimulant* plan has seemed most beneficial are *pneumonia*, pleurisy, and erysipelas." Instead of adopting the large dose system, you consider that small, nauseating, and diaphoretic doses are equally effectual. But although nausea and diaphoresis precede the cure, I think it would be "very difficult to prove" that they cause it. Indeed, if you would select a case of pneumonia, the symptoms of which are completely covered by those of tartar emetic, and give this substance in still smaller doses, even infinitesimal ones—you would find that recovery may take place without either nausea or increased perspiration. Every

case of pneumonia is not, however, suitable for tartar emetic. Homœopathists have several other remedies, which are much more frequently found applicable.

Again, in speaking of the actions and uses of arsenic (p. 184), you write—"A great multiplicity of secondary affections has farther been observed, among which the most frequent and most remarkable are partial palsy, *epileptic convulsions*, and *dyspepsia with emaciation*." In page 136, however, you admit that "it is one of the *standard remedies* in *epilepsy*, and is perhaps fully as often successful as any other method of cure;" and (in p. 134), that it "is in *small doses a tonic*."

In speaking of the "troublesome effects apt to accrue from its physiological operation," you say—"Among other phenomena in such circumstances, *desquamation of the cuticle*, dropping of the nails, and falling out of the hair, have been observed," and yet "it is now a *staple remedy* in some of the more obstinate species of *scaly cutaneous disorders*, such as lepra and psoriasis.

Camphor.—(p. 257.)—"When taken largely in coarse powder, it commonly occasions *pain in the stomach, sickness, and vomiting*; and animals die chiefly with symptoms of irritant poisoning. But when in a state of fine division, or when dissolved, large doses excite little else than narcotic symptoms, such as giddiness, staggering, obscurity of vision, confusion of ideas, and *delirium*—a state, in short, which considerably resembles intoxication, is attended with *increased frequency of the pulse*, and ends in *stupor, interrupted by occasional convulsions*." Again, (in p. 258)—"While these phenomena would appear to indicate a *stimulant action on the circulation, and still more on the nervous system*, with a secondary depressing effect, the current doctrine refers the *therapeutic influence of small doses* to a *primary sedative operation upon the nervous system*. It is held to *allay nervous excitement, subdue pain, arrest spasm*, and sometimes to induce sleep. At the same time it acts as a gentle diaphoretic, and induces a sense of coolness and refreshment; so that *where the pulse is excited, especially in connection with an*

*excited state of the nervous system, depression of the circulation, and even also of the animal temperature, may ensue. On account of these virtues, camphor is used in continued fevers of the typhoid type, especially when nervous symptoms, such as delirium, watchfulness, tremors, and starting of the tendons, prevail in the advanced stage; and by some authors, such as Dr Cullen, its utility in such circumstances has been considered undeniable. It does not accelerate, but, on the contrary, rather reduces the frequency of the circulation in fever," &c. [that is, when it is already increased].*

Chlorine.—(p. 310.)—“If chlorine gas be somewhat diluted, it may be inspired, and then causes a painful sense of rawness in the throat and down the windpipe into the lungs, with coughing and expectoration, which may be followed by *bronchial inflammation.*” Again, speaking of its therapeutic applications (p. 311), you observe—“It has been held to be an astringent, in consequence chiefly of the property ascribed to it of diminishing chronic *bronchitis* when inhaled;” and in the same page, “Chlorine inhalations have been found serviceable in chronic *catarrh.*”

Creasotum.—(p. 376.)—“In the human subject, its poisonous action has been sometimes manifested by *nausea, vomiting, headache, &c.*” And again, on next page—“Its most striking effects, however, as an internal sedative, are shown in the arrestment of *vomiting.* \* \* \* It allays *vomiting*, from functional disorder of the stomach, or nervous irritability of that organ; and even in that which depends on organic disease in the stomach itself, it is not unfrequently of *great temporary benefit.* It has been found by accoucheurs a valuable remedy for the *chronic vomiting* of pregnancy. Others have observed it to be useful in *the most obstinate of all kinds of vomiting—sea sickness.*”

I had intended to extract several other passages, of a nature similar to the above, from your own work—for it is full of them; but, as this communication has already extended over a greater space than I expected, I shall rest satisfied with the

specimens I have already adduced of your unintentional advocacy of Homœopathy. I do not mean to affirm that Homœopaths would give the above medicines in the diseases you specify. I could not do so; for a Homœopathist, in order to treat any patient successfully, must not content himself with the bare fact that he has this or that particular symptom, however important it may be, but must take *all* the symptoms into account, and consider the pathological state of which they are the indices, before he can select the appropriate remedy.

What I have said may not be sufficient to dissuade you from the belief you at present entertain; but if I have succeeded in my endeavour to impress you, or any Allopathist who may chance to peruse this letter, with the conviction that the subject of Homœopathy is one deserving of a careful and impartial consideration, I shall feel abundantly satisfied.

I am, sir,

Your obedient servant,

GEORGE E. STEWART.

To Dr Christison.



