

The ethics of mongrelism ... / a correspondence between Thomas Skinner and Robert E. Dudgeon.

Contributors

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THE 20,
ETHICS OF MONGRELISM.

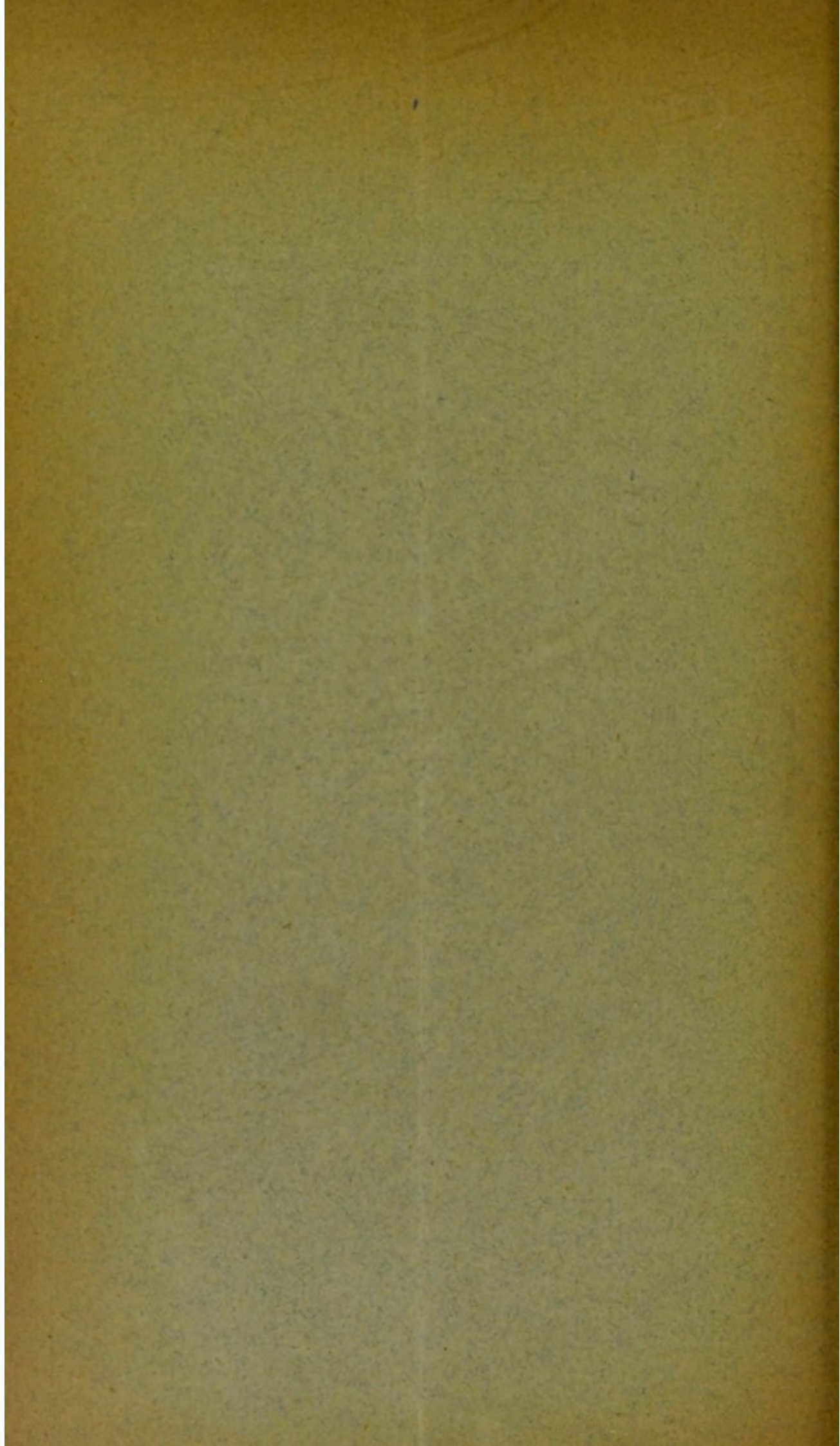
A SUPPLEMENT TO
NO. 1, VOL. IV, OF
THE ORGANON.

A CORRESPONDENCE BETWEEN
DR. THOMAS SKINNER,
LATE EDITOR OF "THE ORGANON,"
AND
DR. ROBERT E. DUDGEON,
EDITOR OF "THE BRITISH JOURNAL OF HOMOEOPATHY."

AUDI ALTERAM PARTEM.

LIVERPOOL:
ADAM HOLDEN, 48, CHURCH STREET.

1881.



THE
ETHICS OF MONGRELISM.

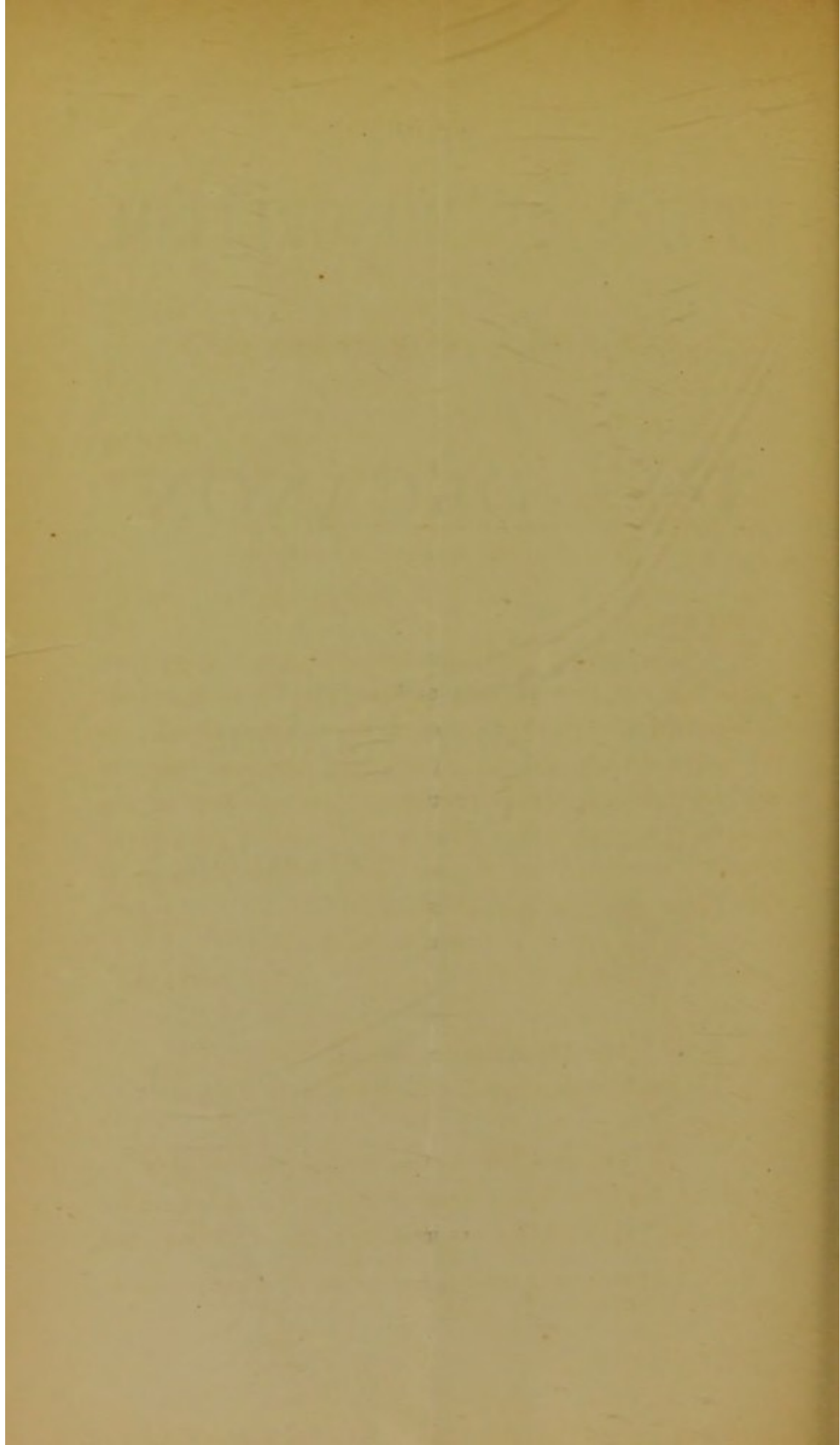
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THE ETHICS OF MONGRELISM.

*Correspondence between DR. THOMAS SKINNER, late Editor of
"The Organon," and DR. ROBERT E. DUDGEON, Editor
of "The British Journal of Homœopathy."*

DR. SKINNER to DR. DUDGEON.

Liverpool, 8th Feb., 1881.

Dear Sir,

As it is just possible that your paper on "Dilutions," in your January number, may bring uncalled-for discredit upon my Potentizer, upon the centesimality of the higher attenuations used by me daily, and even injury to my reputation in the estimation of your readers, to say nothing of my practice, I request that you will publish the letter* I now send you in your April number, or return it with your reasons for not publishing it.

I remain, yours truly,

(Signed)

THO. SKINNER.

DR. DUDGEON to DR. SKINNER.

53, Montagu Square, London, 11th Feb., 1881.

Dear Sir,

I presume, from the contemptuous tone in which you write to the author of our editorial article on "Dilutions," that you did not intend that your letter should

* The letter here alluded to will be found at the end of this correspondence, entitled, "Dilutions."

be inserted in our Journal; for, as an Editor, you must be aware that an article written in such a disparaging style respecting the author of a paper for which the Editors are responsible, would have had but little chance of admission into their pages. Your defence of the centesimality of your potencies will appear much more suitably, and quite as soon, in your own Journal.

In our article we did not assert that your dilutions were not centesimal; we only said you had not proved them to be so by the test you applied to Fincke's, but that should you do so you would be impaled on the horns of a dilemma, which you are pleased to sneer at as a "brilliant syllogistic *denouement*"—whatever that may mean.

This is the point of our observations on your potentizing experiments, and not whether your dilutions are centesimal or otherwise, a matter on which we gave no opinion.

We gave you full credit for frankly confessing your changes of opinion and views, and only reprov'd the overbearing insolence with which you treated those who differed from you at earlier stages of your process.

I return your letter by this post.

Yours truly,

(Signed)

R. E. DUDGEON.

DR. SKINNER to DR. DUDGEON.

Liverpool, 15th Feb., 1881.

Dear Sir,

I have just returned from seeing some patients at Bournemouth, to find your letter of the 11th instant and my MS. letter of 1st February, which is certainly not the first that has been similarly treated, and it is nothing more than I expected,

Had "THE ORGANON" still been in existence, it would *not* have been "a more suitable" medium for the publication of my letter of the 1st Feb. current, because the mischief done to me and to the cause which I have at heart was done in your pages and before your readers. I do not press you to publish my letter; I am perfectly indifferent about it; but I promise you that every reader of *The British Journal of Homœopathy* in this country and in America, as well as the whole Homœopathic fraternity, shall have an opportunity of reading my letter in answer to "Dilutions," and, what is more, the entire correspondence between us on this occasion will be published, *verbatim et literatim*. I presume you do not object to this; if you do, be kind enough to say so before I proceed to press.

I remain, yours faithfully,

THO. SKINNER.

P.S.—You may still choose between publishing my letter in *The British Journal of Homœopathy*—the April number only—and my publishing it with this correspondence and comments. I shall take silence for consent.—THO. S.

DR. DUDGEON to DR. SKINNER.

London, 16th February, 1881.

Dear Sir,

Your very amusing attempt to coax or bully us into publishing your letter in reply to our article "On Dilutions," in *The British Journal of Homœopathy*, by the threat, if we decline, of publishing it and the whole correspondence in such a way that "every reader of *The British Journal of Homœopathy* in this country and in America, as well as the whole Homœopathic fraternity, shall have an opportunity of reading" them, strengthens us in our resolve not to give it a place in our pages.

We were not aware that "THE ORGANON" had ceased to exist when we proposed that you should publish your answer to us therein; but as you now propose to bring the matter before "the whole Homœopathic fraternity," a much larger public than is accessible to either "THE ORGANON" or *The British Journal of Homœopathy*—we cannot but feel grateful to you for taking this trouble, as we are sure the whole Homœopathic fraternity will approve of the course we have taken.

We have, of course, no objection to your publishing the whole of the correspondence, including this letter, and we look forward with pleasant anticipations to your "comments thereon," which cannot fail to be both amusing and instructive.

Yours faithfully,

(Signed)

R. E. DUDGEON.

For Editors *British Journal of Homœopathy*.

To the Editor of the British Journal of Homœopathy.

"DILUTIONS."

Dear Sir,

Permit me to direct your attention to an article in your Journal of last January, in which you or one of your staff attempts to disparage my Centesimal Fluxion Potentizer, and the Attenuations made upon it for my own use, and which have been in daily use by myself, Dr. Berridge, and others, since 1878. The article which I allude to is entitled "*Dilutions*," and I mean to confine the remarks I have to make to a paragraph on page 22, and I shall feel obliged by your publishing them in the Journal which called them forth.

In the first place, the writer has taken every advantage

of the changes—the necessary changes—going on in my mind during the investigation of a subject the most subtle and difficult that it is possible to conceive, the difficulties being increased by my social relation to certain persons.

The writer of the article on “Dilutions” states, page 22 :—
“Dr. Skinner, however, *continues to reckon as centesimal all dilutions made by a potentizer he has himself invented*, because the fluxion is interrupted instead of continuous, the vessel being emptied at every step in the scale. It does not appear, however, that he has extended to his own preparations the testing by which he found Fincke’s, presumably centesimal, to be only unesimal, *and until he has done so, he is hardly safe in counting on them.*” After a few other remarks, taking advantage of the adversary’s “thinking aloud,” the writer winds up with the following brilliant syllogistic *denouement* :—“The only alternative is, *that Dr. Skinner’s*” (*attenuations or dilutions*) “*are as unreal as those of his fellow-potentizers.*” (The Italics are my own.—THO. S.)

My “Centesimal Fluxion Potentizer” is no secret, and, unlike Fincke’s process, it was never patented. It was exhibited at the Liverpool Congress in its original crude form, perfectly home-made, “our own make” truly; and more than one physician who saw it in action said, “This is the very thing the doctor ordered.” The writer of “Dilutions,” before publishing such hasty conclusions, would have shown more wisdom had he examined the Potentizer for himself, and seen it in action. What would he think or say of any one giving an opinion about a newly-invented sphygmograph, without having first seen it, without having carefully examined and tried it in action, to see whether or not it was capable of performing the end for which it was made? The conclusion is inevitable—he was no scientist, or, at best, out of court!

The term "Fluxion," applied to my Potentizer, has evidently led the writer of "Dilutions" to imagine that Fincke's process and my own are the same in principle. This is a "fatal error." The fluxion-process of Fincke is *continuous*, my own is *interrupted*; and this apparently trifling difference creates a mighty difference in all rightly constituted minds, whatever it may do in the mind of the writer on "Dilutions;" because the interruption and emptying of the glass at every charge of 100 minims *converts the unesimal scale of Fincke into the centesimal scale of HAHNEMANN*. By the process of Fincke, every drop which passes out of the glass is attenuated one-half, and it is the same with every 50 or 100 drops; therefore the numerical amount of attenuation is always unesimal, and I have shown elsewhere that this can be corrected by using $666\frac{2}{3}$ minims of water for every potency, instead of 100. By my process of *interrupted-fluxion*—a slower but more certain one—it is *necessarily centesimal*, and requires no more proof of its truth and correctness than that two and two make four, and the rationale is equally simple. The attenuating glass having received a charge of say 100 minims, is immediately and entirely turned upside down, thereby throwing its contents down the waste-pipe. The glass returns for a fresh charge of 100 minims, bringing back what adheres to the sides of the glass, generally reckoned about one minim or thereabout. The force with which the fine jet rushes into the glass is sufficient to mix the 1 minim of the previous attenuation with the 100 minims of the next centesimal attenuation. Pray! what is this if it is not HAHNEMANN'S method or scale of 99 or 100 upon 1 of the previous attenuation—the centesimal scale? while in Fincke's process of *continuous-fluxion* it is 1 upon 1, or 100 upon 100—a very different degree of attenuation from 100 upon 1.

As all my "F. C." attenuations are made as I have stated, they are as centesimal as any made by hand by HAHNEMANN'S method; and if they are doubted, let the writer of "Dilutions" prove the contrary—not by referring to the fact whether they act or not, or whether it is the opinion of this one or that that my potencies and Fincke's or Swan's are equally good—as that depends much more upon the *homœopathicity of the selection* than on the degree of attenuation—but by physical and numerical demonstration, which is certain, and which is not dependent upon the erring opinion or judgment of any man.

The writer of "Dilutions" challenges me to apply the same physical test to my own process of interrupted-fluxion as I adopted in testing the fluxion-process of Fincke. This is not so easy as he seems to think. Where the result is to be centesimal, the fraction of sugar is necessarily small, the first being the 100th, the second the 10,000th, and the third the 1,000,000th of a grain of sugar. As it would be impossible to work upon one grain, I shall try a similar physical test, using 100 grains of sugar and substituting interrupted for continuous-fluxion.

Experiment 1.—I cleaned my attenuating-glass, dried it in a copper oven made by Townson and Mercer, of London, for evaporating liquids, at 212° [Fahr., the temperature of boiling water, and when cool it weighed 112 grains; I dried some powdered loaf sugar, and when it was cool, I weighed out 100 grains—together they weighed 212 grains. I placed the 100 grains of sugar in my attenuating-glass, previously marking it with a file on the exact spot on the outside, corresponding to 100 minims of water. I then put in a little very hot water to dissolve the sugar and expel the air in the interstices. I stirred the contents with a delicate sewing needle, and placed the whole, needle and all, into a sand

bath in the copper oven. I kept it there, gently stirring with the needle occasionally, until thoroughly dissolved. When perfectly cool, I filled the glass, up to the mark made with the file, with cold water, and stirred with the needle until thoroughly incorporated. The needle was weighed when clean and dry, before and after stirring the liquid; the difference in weight was unappreciable. It is surely unnecessary to demonstrate that this solution is the first centesimal of HAHNEMANN, made with 100 grains instead of 1 grain, and that every minim of the 100 contains 1 grain of sugar, the scale being 99 upon 1, or 1 in 100. I place this glass of *syrup* in my "Centesimal Fluxion Potentizer," and I set the latter in action with my hand, slowly and deliberately, as I always do with the first five attenuations—and the more so must I do so in this experiment, as I am manipulating a viscid fluid, which is certain to adhere to the glass in more than double the quantity it would if it were water or spirit and water. The glass is upset, and returns for a fresh charge of 99 or 100 minims, bringing back of the first attenuation what adheres to the inside of the glass. Let us take stock of it. The contents of the glass being evaporated to perfect dryness in the sand-bath in the copper oven and allowed to cool, weighed exactly 117 grains.

Deduct weight of glass..... 112 "

Amount of sugar left in the glass 5 grains.

Considering the viscosity of the "mother tincture," and the quantity of substance operated upon, the result compares favourably with the Fluxion-process of Fincke, where, at the first off-go, we got the same amount of sugar out of 10 grains operated upon. If it had not been for the syrupy character of the mother-liquor, I should certainly have had 1 grain instead of five grains. Let us try again!

Experiment 2.—I take the same glass; washed clean and dried, it weighed 112 grains. I take of dried loaf-sugar 1 grain; combined weights, 113 grains. I dissolved the sugar in the glass in as few drops of water as possible—two or three; I then ran an ordinary charge of my Potentizer (100 minims) into the glass, and caused the machine to upset it in the ordinary way. I placed it in the sand-bath in the copper oven, and waited its evaporation to dryness. The result should correspond to the second centesimal of HAHNEMANN, only remembering that I am operating with 100 grains to begin with instead of 1 grain; therefore I should have the 100th of a grain of sugar in the glass instead of the 10,000th. But as I have no scales fine enough to weigh the one-hundredth of a grain, I had to begin again, so I operated upon 20 grains, and I look for one-fifth of a grain in the glass.

Weight of glass.....	112 grains.
Weight of dried sugar	20 "
Combined weights.....	132 "

Having dissolved the sugar in water so as to occupy 100 minims, without heat or stirring, but by simple agitation, I placed it upon my "Centesimal Fluxion Potentizer," turned the water-wheel with my hand, so as to empty the glass and bring it back for a fresh charge of 100 minims. I then turned on the main and filled the glass in the ordinary way, closing the tap the instant the bells of air and water reached the top of the glass. The level of the water was flush with the mark made by the file, showing how numerically and physically correct the machine is for making centesimal potencies. I then placed the glass in the sand-bath in the oven, and evaporated it to perfect dryness. When cool, I weighed the glass with the remains of the 20 grains of sugar

crystallized on its sides ; it weighed.....	112 $\frac{1}{2}$ grains.
Deduct weight of glass	112 "
Leaves the residue, <i>quod erat demonstrandum</i> * $\frac{1}{8}$ th of a grain.	

If this does not convince the writer of "Dilutions" that my "F. C." attenuations (meaning by the initials Fluxion-centesimal) are not "as unreal as those of his fellow-potentizers," but that they are as truly centesimal as any made by Lehrmann, or by the hands of HAHNEMANN himself, I know not what will. I thank the writer of "DILUTIONS" for challenging me to the test, as I never dreamt that any one who had studied the subject could have doubted the centesimality of my potencies ; he has thereby afforded me a most excellent opportunity of placing them for the future above all question, cavil, or doubt.

These same attenuations I have used every day in my practice since 1878, never using any below the fifth centesimal ; and I know, and you know, that I have published a good number of very remarkable cures with them, and I have hundreds quite as remarkable still to publish. Most of the cures have been effected by very high dilutions, chiefly the fifty-thousandth, the one-hundred-thousandth, and the millionth centesimal—and they act to perfection if selected as HAHNEMANN directs, not otherwise. It is a matter of moonshine to me what I may have said out of compliment or kindly feeling of the potencies of Fincke or Swan—*humanum est errare*—but it would appear that I have made no mistake when I invented my "Centesimal Fluxion Potentizer ;" if I have, perhaps the writer of "Dilutions" will show me wherein I have erred.

* The scales used are eleven inches beam, box-ends ; all the movements are steel-faced, the whole being of the best metal and gilt, to prevent rust. If carefully cleaned and delicately set, I should trust it to turn with the thirtieth of a grain in a still atmosphere.—THO. S.

It is almost needless to add that I shall "continue to reckon as centesimal all dilutions made on my potentizer." I prefer the term attenuator, because the word *potentizer* leads to wrong inferences by the ignorant and vulgar. The potency lies in the law of cure, and not in the instrument. My new instruments are called "Attenuators."

I remain, dear Sir, yours truly,

THO. SKINNER, M.D.

Dunedin House, Liverpool, 1st Feb., 1881.

COMMENTS.—In reply to Dr. Dudgeon's jesuitical remark, "In our article we did not assert that your dilutions were not centesimal, we only said you had not proved them to be so by the test you applied to Fincke's." What does he mean by "*The only alternative is, that Dr. Skinner's dilutions are AS UNREAL as those of his fellow-potentizers.*" He subsequently talks about my being impaled on the horns of a dilemma. It strikes me that those who live in glass-houses should not throw stones. I have no objection to my dilutions being thought "as unreal" as those of Dr. Fincke. Dr. Fincke's dilutions have all been made with the greatest care, and as I have used them for years, and Dr. Dudgeon has never used one of them—even if he knew how, which I very much doubt—I can testify to their great practical worth. As it is only the notation which is wrong, that is easily rectified, when they become as "real" as my own. Dr. Dudgeon, or the author of "Dilutions," did "not assert that my dilutions were not centesimal;" oh, no! he only meant to imply thereby that they were "unreal." By what rule of ethics or of morals he arrives at the conclusion that a thing which is "unreal" can be "the thing," I am at a loss to comprehend: if my dilutions are "unreal," they cannot be centesimal, and

if they are not centesimal, I should simply pour them all down the waste-pipe. Dr. Dudgeon complains of the "contemptuous tone in which I write to the author of our editorial article on 'Dilutions,'" and he refers to my experience as an Editor, that I "must be aware that an article written in such a disparaging style respecting the author of a paper for which the Editors are responsible" (because written by Jupiter-Maximus himself), "would have little chance of admission into their pages." I leave the profession to judge between us who has written in the "most contemptuous and disparaging style" of the other's works, or who has "burked" the other's writings oftenest—as I leave it to them to judge between us on whose side the truth lies. I knew perfectly well what I had to expect, as any one can see who has read the first letter in this series. I fully expected the usual polite kick to the upholders and true followers of HAHNEMANN, with the understood unfriendly pass-word of "*Allez au diable!*" I know I am a nuisance to such as Jupiter-Maximus, and I am likely to remain so until he mends his ways.

Dr. Dudgeon says, "We gave *no opinion* as to whether or not your dilutions were centesimal or otherwise," and yet he, or Jupiter-Maximus, or "Dilutions" vilely insinuated that they were "unreal." This is not offering an opinion! There are some expressions used by Dr. Dudgeon, which are, to say the least, "unparliamentary" and most unbecoming from one gentleman towards another; I mean "overbearing insolence," and "bully." For Dr. Dudgeon's own sake, I trust he will withdraw them, and the more especially, as I have elsewhere apologised to or made amends for aught I have said or written in the heat of argument; and what is more, if any gentleman on either side of the Atlantic considers himself hurt or offended by anything I have said

in the pages of "THE ORGANON," I now express my regret, and I shall be happy to make any more public apology he may require.

In conclusion, one thing is certain, that however much we may have differed, *all have come round to my way of thinking*, and even Jupiter-Maximus himself has been forced to confess in those pages, whatever he may say in his own, that "In our article on 'Dilutions' we did not assert that your's were not centesimal." It is just as well, because they are centesimal; and I defy Dr. Dudgeon, or "Dilutions," or any one else, to prove them otherwise, dilemma or no dilemma!

LAPIS RUBER.

ITS CHEMICAL CONSTITUTION.

(ORGANON, *Vol. III., p. 331.*)

It may be interesting to some, as it is to me, to know the chemical constitution of the piece of Red-Sandstone selected by my patient, and dynamized to the third centesimal trituration by Messrs. Thompson and Capper, of this city. To the Hahnemannian this analysis is of the utmost importance, as he knows that the effect induced in my patient, though markedly those of *Silicea*, in another it might induce the symptoms characteristic of any of the other substances, depending upon their susceptibility or correspondence to each respectively.

ANALYSIS OF LAPIS RUBER.

Iron (as ferric oxide), considerable.

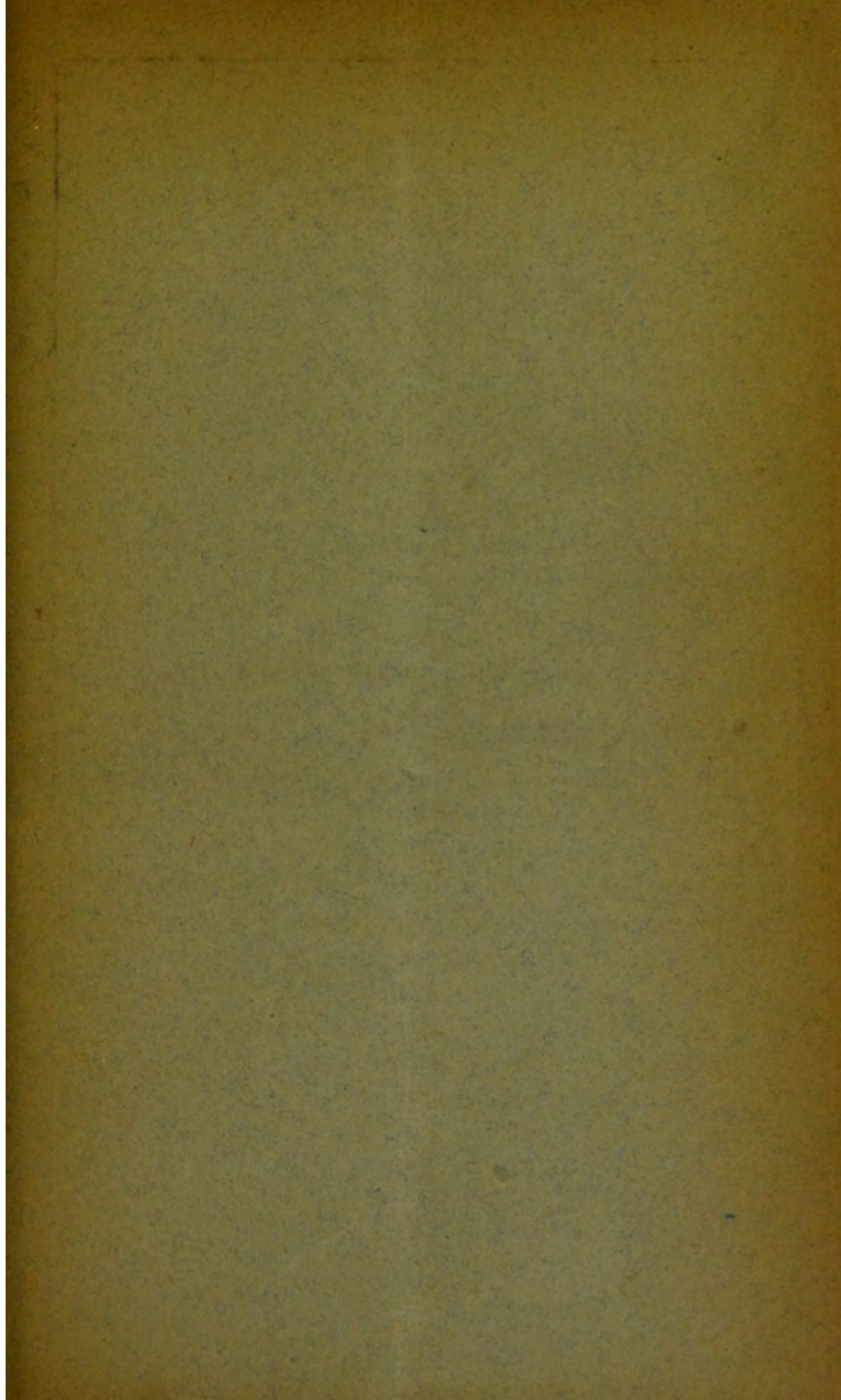
Alumina, a trace.

Calcium, a trace.

Magnesium, considerable.

Silicea, about 95 per cent.

It would appear that "Neural-Analysis," or the Physiological-Test, will come ultimately to be infinitely superior to Spectrum-Analysis, if it is not so already. It is Dr. P. M. Latham's wise remark, oft-told by me, that, "after all, remedies are the best analyzers of disease," and, as a corollary, disease is becoming the best analyzer of remedies, when viewed through homœopathic spectacles.



NOTICE.

The Publisher of "THE ORGANON," MR. HOLDEN of 48, Church Street, Liverpool, is ready to supply back numbers or volumes at 4s. or \$1 each number, and 16s. or \$4 each volume, bound in cloth, post free.