

exceedingly plausible. I therefore did my best to point out its fallacy, partly with a view of convincing him. He replies, not by arguments, but by a personal attack on me, the acrimony of which I deplore but cannot explain, and by re-asserting his opinion as "a fact long accepted as beyond question . . . by authorities too numerous to name." In short, it appears to be "a fact" which the proverbial schoolboy ought to know; but I am never more pleased than when the schoolboy with his universal knowledge is brought out against me. It is generally a sure sign that the assumed "fact" has no other foundation than that intelligent youth's imagination.

In the present case I say that if a man buys sewage he should buy it as he would any other manure, on the basis of a chemical analysis. I say that this is the *only* safe and reliable basis on which to found a calculation of its money value. And I say that if a farmer puts so much money's worth of manure into his land, he is simply a bungler if he does not get it out again with its proper increment of profit, whether he buries it in his land by means of a plough, or of a spade, or of water. Dr. Paul, on the other hand, says that if a man pays so much money for so much sewage because it contains a certain percentage of ammonia he will be ruined. Why? Because of the water.

Of course we all know that water applied to certain crops in certain stages would spoil them; so would manure. If, therefore, a farmer applies sewage under such conditions, he is a bungler. His skill, as a sewage farmer, is shown in so arranging his land and crops as never to injure but always to benefit them by the application of his sewage. This is simply a question of good *versus* bad management. It is one to be decided on the farm and not in the laboratory. Incredible as it may appear to Dr. Paul, on taking a lease of the sewage of the town of Romford, although bound, under penalty, to use it every day in the year, I stipulated for an additional dilution of the sewage to the extent of twenty gallons of water per head of the population *per diem*, and this although I have not got the proportion of "twenty-five acres for every 100 persons," as Dr. Paul says I recommend, but which I never recommended, and should be the first to condemn.

Having defended my opinion, I will now, with your permission, defend my character. Dr. Paul regrets that I should "declare myself a partisan of one particular solution of the town-refuse problem." It may be that he has so recently commenced the study of this large question that he has, as yet, formed no opinion upon it except that by irrigation the value of the manure cannot be recovered; but I have laboured at it for many years, and it is not possible that in those years I should not have formed some very distinct opinions. Will any one else regret that I should "declare" what those opinions are? I think not; and I think that such a declaration is straightforward and honest, though why Dr. Paul should affect to think that I have made it now for the first time I cannot say. My views on this subject have been publicly expressed for many years, and have been so expressed in his presence. Dr. Paul complains that I desire "the promotion of a project at any price." I do. I desire to see sewage utilised all over the country, and by irrigation, if possible, because I believe it is the right thing, and I am gratified to find that my views are in exact accord with those expressed in the unusually careful and able report just published by the Royal Commission on Pollution of Rivers. But when I joined the Committee of the British Association on Sewage, I at once suggested that the scope of their inquiry should be enlarged so as to include a full investigation into every proposal for the utilisation of sewage which presented any appearance of practicability. I also suggested a source from which the necessary funds might be obtained. My suggestions were approved both by the Committee and by the Council of the Association, the funds have been in great part obtained, and the inquiry is about to be prosecuted. Whether Dr. Paul is justified in the personal attack he makes upon me, I will therefore leave to the judgment of your readers.

I will merely add, as regards my opinions, that they were condemned in the most unqualified and unmeasured language by Baron Liebig five years ago; but I have lived to prove Liebig mistaken in this instance, and, on the practical farming part of the question, I think I may claim to be a very much better authority than Dr. Paul. At all events, as the German philosopher said of the author of the Epistle to the Corinthians, "I do not agree with your Dr. Paulus," and I have yet to learn that such a disagreement involves the breach of any law, human or divine, although Dr. Paul is evidently firmly convinced that

while *he* has an undoubted right to express "a foregone conclusion," such an expression on *my* part is a sign of great moral depravity. This is a common form of superstition, but it is scarcely scientific, and seldom adds much weight to a man's opinions.

W. HOPE

Parloes, March 6

Transactions of Scientific Bodies

I WISH it were possible to induce our learned societies to be a little more liberal; it should be their aim to spread knowledge, not make it a luxury for the wealthy. I happen to wish to read a paper by Professor Tait on "Rotation," published in the Transactions of the Royal Society of Edinburgh. The only libraries I have access to are those of the British Museum and London Institution. At the Museum there is no volume of the "Transactions" later than 1864; at the London Institution no volume later than 1862; so that if I persevere in my intention of reading the paper, I must buy the volume containing it, for which I must pay 2*l.* 2*s.*—that is, I must buy thirteen papers I don't want in order to be able to read one which I do want: these include one on the temperature of newly-born children, and another on tetanus in cold-blooded animals.

All papers should be published separately; this would lead to a much wider diffusion of them, and the Societies would benefit by their increased sale.

London, March 7

G.

Sir. W. Thomson and Geological Time.

THE *North British Review*, for July last, thoroughly exposes the inaccuracy of the quotation from Prof. Thomson, referred to by your correspondent G.H., in its article devoted to the consideration of Geological Time.

J. S.

YOUR correspondent G.H. will find in one of Thomson's papers something very like the assertion "that there was a time when the earth rotated too swiftly for the existence of life," but expressed in a manner at once more precise and less pleonastic. "The existence of life" reminds me of a phrase which I heard a few days ago from a female beggar; she lamented that her husband had "fallen into habits that are habitual." Well; the required reference is the paper "On Geological Time," in the Transactions of the Geological Society of Glasgow, vol. iii. Part I. pp. 15 and 16 (§§ 19 and 20). A thousand million years ago, says Thomson, "there must have been more centrifugal force at the equator due to rotation than now, in the proportion of 64 to 49. . . . If the earth rotated seventeen times faster, bodies would fly off at the equator. . . . If you go back ten thousand million years ago—which, I believe, will not satisfy some geologists—the earth must have been rotating more than twice as fast as at present—and if it had been solid then [which he thinks improbable], it must be now something totally different from what it was." Such a state of things he seems to consider inconsistent with any organic life such as we know of. Surely the connection of this question with the argument from retardation by tidal friction is too plain to need exposition.

Ilford, March 11

C. M. INGLEBY

How large seems the Moon?

READING Mr. R. A. Proctor's communication under the above heading in NATURE of March 3rd, reminds me of an experiment I tried some time ago. I imagined I should get all sorts of answers to the question, varying from "a fourpenny-piece" upwards, without any particular size being more frequently pitched on than any other. I did not collect more than about twenty or thirty replies, but they were sufficient to show that, contrary to my expectation, *one to two feet* was assigned more frequently than other sizes. Mr. Proctor says the estimate of a foot in diameter assigns a distance of 115 feet to the moon. If he were to try to convince the observer of the soundness of this deduction, the latter would probably meet him with vehement reiteration that he only means the moon *looks* a foot large. It seems to me fairer to say that such a man thinks a two-foot rule 115 feet off, a fit and proper measure for celestial objects. I think many, who are aware of the futility of attempting to convey their ideas to other minds by these comparisons, yet involuntarily make them in their own. I am conscious of a lurking idea that the moon is more like a fourpenny-piece in size than anything else. The question is, what is the cause of the "personal equation" which determines for each individual the distance of