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

Parsoes, 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

the imaginary two-foot rule—and why is about 115 feet more common than other distances?

I think it probable the sort of objects with which a man is familiar in his daily life may have some influence on his judgment in this respect. The men I questioned were for the most part engaged in warehouse or out-door work. I should like to know what answers watchmakers or jewellers would give. This theory, however, fails to account for the different estimate formed by the same individual, when the moon is high above, or on the horizon; but I imagine, in the latter case, the imaginary rule is superseded, or more properly modified, by the terrestrial objects which are in the field of vision with the moon.

Cardiff, March 7

GEORGE C. THOMPSON

Cuckows' Eggs

MAY I be permitted to make a few observations upon Mr. Sterland's letter in your issue of the 27th of January, relative to the cuckows' eggs' controversy.

In answer to Prof. Newton's query, "If the eggs in question were not cuckows', what birds laid them?" Mr. Sterland says, "My reply is simply that they were laid by the birds in whose nests they were found."

Besides the well-known fact mentioned by Mr. Newton (NATURE, p. 266), "that when birds lay larger eggs than usual the colouring is commonly less deep," which tells so strongly against Mr. Sterland, I will only mention the following instances.

1st. The egg No. 9 in the series given by Herr Baldamus, (see *Zoologist* for April 1868), which the Royal Forester, Mr. Braune, found in the ovary of a just-killed cuckow, and which "was coloured exactly like the eggs of Hypolais."

2ndly. The egg No. 26 in the same series, belonging to the collection of Dr. Dehne, described as a "light-greenish blue egg without any markings," and "might have passed for the egg of either species of the redstarts," which specimen "was laid in a cage by a cuckoo that was caught in a hay loft." (The italics are mine.)

3rdly. The two instances given by Mr. H. E. Dresser, (NATURE, p. 218) of two eggs of the cuckow "closely resembling" those "of the common bunting (*Emberiza miliaria*)," one found in a blackbird's, the other in a robin's nest.

Can Mr. Sterland explain away the 1st and 2nd instances? and how does he reconcile the 3rd instance with his affirmation? Will he venture to say that the two apparent bunting's eggs were laid by a blackbird and a robin respectively, or, will he risk the remark that a common bunting had taken a cuckow-like freak into its head and been laying its eggs in other birds' nests? As either alternative is too absurd to be worth a moment's consideration, we can only conclude that they are cuckows' eggs, unless there has been some mistake as to the nests from which they were taken—scarcely likely, if Mr. Dresser's remarks are carefully read.

Therefore I think Mr. Sterland must admit, if he accepts these facts as authentic, that the cuckow's eggs *do* vary to a large extent, and doing so, he has little foundation for doubting the identity of the specimens mentioned by Herr Baldamus as taken from nests whose eggs they resembled.

For my own part I have every confidence in the discrimination of that ornithologist, and am not afraid that he had been carried away by a pet theory that led him to *imagine* this or that egg taken "out of the nest of the hedge-sparrow or tree-pipit" to be a cuckow's merely because it is "an egg rather larger than the rest, but marked and coloured in a similar manner." If Mr. Sterland will carefully examine Herr Baldamus's evidence he will find that it is not of such a superficial character.

I agree with Mr. Sterland that it is certainly singular that British and Continental observers should come to such opposite conclusions as to this variation of the cuckow's egg in their respective countries; but this is no reason for impeaching (merely because our experience differs) the testimony of the eminent Continental oologists who affirm this extreme variation, and to some of whom Herr Baldamus's theory is probably unknown; as instance, in the two quotations by Prof. Newton (NATURE, p. 266) from *Des Murs* and *Degland et Gerbe*.

Can it be that such extreme variation really does occur on the Continent, and is yet comparatively absent in Britain? I leave it to abler hands than mine to discuss; but if it should prove so, it will be another feature in the already remarkable habits of the cuckow.

Tadcaster, Feb. 7.

FRANCIS G. BINNIE

MR. RUSKIN ON RIVER CONSERVATION

IN his recent Friday evening discourse on Verona and its Rivers, at the Royal Institution, Mr. Ruskin, speaking of the Adige and the Po, said: "I want to speak for a minute or two about these great rivers; because in the efforts that are now being made to restore some of its commerce to Venice precisely the same questions are in course of debate which again and again, ever since Venice was a city, have put her Senate at pause—namely, how to hold in check the continually advancing morass formed by the silt brought down by the Alpine rivers. Is it not strange that for at least six hundred years the Venetians have been contending with those rivers at their *mouths*—that is to say, where their strength has become wholly irresistible—and never once thought of contending with them at their sources, where their infinitely separated streamlets might be, and are meant by Heaven to be, ruled as easily as children? And observe how sternly, how constantly the place where they are to be governed is marked by the mischief done by their liberty. Consider what the advance of the delta of the Po in the Adriatic signifies among the Alps. The evil of the delta itself, however great, is as nothing in comparison of that which is in its origin. The gradual destruction of the harbourage of Venice, the endless cost of delaying it, the malaria of the whole coast down to Ravenna, nay, the raising of the bed of the Po, to the imperilling of all Lombardy, are but secondary evils. Every acre of that increasing delta means the *devastation of part of an Alpine valley, and the loss of so much fruitful soil and ministering rain*. Some of you now present must have passed this year through the valleys of the Torcia and Ticino. You know, therefore, the devastation that was caused there, as well as in the valley of the Rhone, by the great floods of 1868, and that ten years of labour, even if the peasantry had still the heart for labour, cannot redeem those districts into fertility. What you have there seen on a vast scale, takes place to a certain extent during every summer thunderstorm, and from the ruin of some portion of fruitful land the dust descends to increase the marshes of the Po. But observe further—whether fed by sudden melting of snow or by storm—every destructive rise of the Italian rivers signifies the loss of so much power of irrigation on the south side of the Alps. You must all well know the look of their chain—seen from Milan or Turin late in summer—how little snow is left, except on Monte Rosa, how vast a territory of brown mountain-side heated and barren, without rocks, yet without forest. There is in that brown-purple zone, and along the flanks of every valley that divides it, another Lombardy of cultivable land; and every drift of rain that swells the mountain torrents, if it were caught where it falls, is literally rain of gold. We seek gold beneath the rocks; and we will not so much as make a trench along the hillside to catch it where it falls from heaven, and where, if not so caught, it changes into a frantic monster, first ravaging hamlet, hill, and plain, then sinking along the shores of Venice into poisoned sleep. Think what that belt of the Alps might be—up to four thousand feet above the plain—if the system of terraced irrigation, which even half-savage nations discovered and practised long ago in China and in Borneo, and by which our own engineers have subdued vast districts of farthest India, were but in part also practised here—here, in the oldest and proudest centre of European arts, where Leonardo da Vinci—master amongst masters—first discerned the laws of the coiling clouds and wandering streams, so that to this day his engineering remains unbettered by modern science; and yet in this centre of all human achievements of genius no thought has been taken to receive with sacred art these great gifts of quiet snow and flying rain. Think, I repeat, what that south slope