

judicially-minded person would attach much weight even to a report of his own, drawn up under such circumstances, and all judicially-minded persons will regret its introduction here. Prof. Osborne Reynolds's reasoning proceeds on the hypothesis that the gas is not polarised. The only real question here is, Is Prof. Schuster prepared to maintain that this reasoning is correct?

Prof. Schuster, in reporting his reminiscences, first recites a kinetic principle which is quite consistent with there being as much force sideways as perpendicularly to the disc, and which therefore contains no explanation of the phenomenon; and when he comes to the first essential point, viz., that which requires him to show that "an increased pressure on the cold side of the vanes of a radiometer will not counterbalance the force acting on the blackened sides," all that he has to say on the subject is that "he does not think that such is the case!" This is the essential thing to be *proved* before the explanation can be accepted, and he recites experiments which show that it is essential.

Prof. Schuster concludes this part of his letter with the admission that "he does not see how [on his theory] an increase in the force can take place" as the exhaustion proceeds. So much the worse for the theory, since experiment indicates that such an increase in the force does take place. In proof of this I may allege, in addition to Mr. Crookes's experiments, several series of experiments made by Mr. Moss, one of the most judicially-minded, patient, and dexterous manipulators I have met with. The experiments were made with the apparatus described in a communication from him and myself, published last spring in the *Proceedings* of the Royal Society, and the effect of the convection current was with extreme care excluded in two ways—by placing the swinging disc where the influence of the convection current on it before and behind was balanced, and by observing the motions that arose before the convection current had time to reach the disc. Both methods concurred in showing that, as in Mr. Crookes's experiments, the force on the disc uniformly increased with increasing attenuation of the gas up to the limit to which we pushed the exhaustion. Mr. Crookes has shown that beyond that limit it begins to decrease. Prof. Schuster will do a real service to science if he will devote his great skill for some months to repeating these and other concurrent experiments, and either confirm them or point out why they should be set aside.

Prof. Schuster thinks that "any theory of the radiometer which makes the action depend on the comparatively large [small] ratio of the mean free path to the dimensions of the vessel, must necessarily be wrong." Has not Prof. Schuster here overlooked the minuteness of the phenomenon which has to be accounted for? Spheroidal drops of less than a millimetre diameter are easily formed of several light liquids. The Crookes's stress which supports these is an excess of vertical stress over horizontal stress in the supporting layer of polarised gas, amounting to about the 12- or 15-thousandth part of the whole stress. This compares favourably with the minute ratio to which Prof. Schuster refers.

I will not at present enter on that part of Prof. Schuster's letter in which he criticises my published views on penetration, as he refers me to the researches of Messrs. Kundt and Warburg, which I have not yet seen. G. JOHNSTONE STONEY

Glaciation of Orkney

LAST spring Prof. Geikie informed me of a correspondence which was going on in the columns of NATURE as to the question whether the Orkney Islands bore evidence of having been glaciated. It was with much surprise that I heard that there could be any question on this point at all, but I refrained from submitting my opinion to the public—unhesitating though that opinion was—on account of my being then just about starting for my native county, and thus having an opportunity of very specially directing my attention anew to the matter. As the observations I then made *without exception* tended to confirm me in what really required no confirmation, I think I may now come forward as one who has for long known those islands, and who has made a very special geognostic survey of them, during many years. And I would first say, as regards the question, "whether Orkney does or does not give proof of having been covered by a great ice-sheet?" that I believe that no one who has educated his eye—not by looking at pictures in books, but among the rocks themselves—to the *apprehension* and recognition of the hill-contours of an ice-scalped country, would hesitate to declare Orkney to be such. Let such a one take his stand, at a sufficient altitude, anywhere along the north coast of

Sutherland, with a scratched and polished boss under his feet, rolling up into rounded hillocks on every side, and sweep his eye from the two Ben Griams over to Hoy, and he could not but exclaim, "There is a country which has suffered sore."

In having to differ from Mr. Laing, I join issue with him on two points—boulders and foreign stones, and boulder-clay. I have also to corroborate Prof. Geikie as to glaciation near Stromness; for I, during last summer, saw to the immediate north-west of Stromness a surface of gneiss, say ten feet by three, most unmistakably glaciated—both scratches and polishing being shown.

Now as regards "boulders and foreign stones," Mr. Laing will find—I wonder at his not knowing of it—about 100 yards to the west of the House of Saval, in Sanday, one of the finest boulders in Scotland. This boulder, of great size, consists of hornblende gneiss; for long I was unable to identify it with any variety of the hornblende gneiss of Sutherland; but this very year's work enables me to say that it is *very similar* to that of a locality near Dufrin. In all probability, however, its parent rock lay east, not west.

Another boulder I have heard of, but not seen; it was described to me under the name of the "Eagle Stone"; it lies upon the side of a hill in Westray, near Pierowall, and is said to be peculiar as a loose stone, both on account of its toppling position, its being different from any rock in Orkney, and from there being no rock near it.

As to there being "foreign stones" in Orkney, I shall only say that I have at present in my collection polished jaspers, picked up in rolled masses in Orkney; and that fragments of broken agates are found not unfrequently, on the hill tops and sides, in Hoy. These are quotations, *ex grege*.

Mr. Laing's very precise observations on the *clay beds*—let us call them—do call for special investigation.

If the conclusions arrived at by other observers are found to coincide with his—while they could not affect the ultimate decision as regards the ice-clad country—they certainly would strike these clay beds out of the category of boulder-clays. But, sir, I have seen these clays, and I did not see what Mr. Laing saw; and what I did observe leads me to doubt the correctness of his conclusions. For I found it to be a notable circumstance as regards these Orkney clay-beds that they are very markedly *clay-beds*; i.e., that the amount of clay relatively to that of the stoney matter therein is very much greater than that of most boulder-clays.

Now this is a fact which saps the very foundation of Mr. Laing's observation—an abnormally argillaceous clay bed being the result of the disintegration of a normally siliceous sandstone is difficult to conceive. Nay more, although the cement of certain of the Orkney beds is silicate of alumina, forming the blue argillaceous flag, it is an unquestionable fact, that these flags do not disintegrate by the action of the weather. Even the Picts knew that when they built their Broughs thereof. Silicate of alumina is not affected by carbonated waters.

Upon—nearly all along—the west shore of Shapinsha there are cliffs—sea-cliffs of these clay beds, which lie *betwixt the rocks*, or the last visible rock, and *the sea*; that last rock is a red ferruginous loose-grained sandstone, with little or no cement, what there is being micaceous; the clay beds are ochre yellow. The disintegration of this rock never could have yielded these clay beds.

But Mr. Laing may argue that they resulted from the decay of an overlying argillaceous bed. The argument will not stand. Firstly, because the dip is the wrong way; the rock dips at a high angle to the east; the clay *slightly* caps it, and stands as a bank between its escarpment and the sea. Secondly, because a friable yellow freestone, destitute of argillaceous cement, should overlie the red beds. Thirdly, because on the other side of the bay where the argillaceous flags do appear they are quite permanent. Ice might *grind them up*—the "weather" does not rot them down. But here no clay beds are seen.

Finally, sir, I would request my talented countryman—whom I have great pleasure in breaking a lance with in this field—to consider how or why it is that these clay beds are found only *on one side* of the long depression which runs up the centre of the islands?

M. FORSTER HEDDLE

University, St. Andrews, December 19, 1877

Northern Affinities of Chilean Insects

I THINK I may be allowed to express my surprise at Mr. McLachlan's statement that this subject has never yet been

"even more than casually alluded to in works on geographical distribution," and is "ignored in the principal ones," when I have devoted no less than six pages of my book on "The Geographical Distribution of Animals" (vol. ii. pp. 42-48) to a discussion of the main facts—quite as much as could be properly given to it in a general work. It is, however, well worthy of a detailed study, which I am very glad is being undertaken by so competent an entomologist. I hope Mr. McLachlan will endeavour to obtain collections of coleoptera and other orders of insects from the higher tropical Andes, where, I feel confident, some northern forms will also be found.

ALFRED R. WALLACE

Mr. Crookes and Eva Fay

A FEW words from myself seem to be called for by the recent letter of Mr. Crookes in reply to Prof. Carpenter, published in your journal. As far as I am concerned, the breach of etiquette complained of can only apply to my obtaining the publication of the letter Mr. Crookes addressed to me in the *Banner of Light*. The subsequent *facsimile* that appeared I am not in any way responsible for.

The part I took in the matter is very simple, and may be briefly explained as follows:—On Mrs. Fay's return from England to this country the genuineness of her mediumship was very much doubted, and was the subject of controversy not only in the spiritual journals, but in other papers as well. Having, whilst in England, satisfied myself that the manifestations were real, I defended her to the best of my ability, and on seeing it stated in the *Boston Herald* that Mr. Crookes had withdrawn his confidence in her, I thought it prudent to write to that gentleman, mentioning my reason for so doing. In due time I received a courteous reply, which I at once took to the *Banner* office, never dreaming that Mr. Crookes could have any possible objection to its publication after the articles he had himself published on the subject in the English journals. Months elapsed, when one day to my surprise I met with the *facsimile* letter in the *New York Daily Graphic*. On mentioning the subject to the editor of the *Banner* he also expressed surprise, and stated his inability to account for the publication of the *facsimile*. He at first was of opinion that I had taken the letter away and mislaid it; but on searching, the document was subsequently found in the office. Hereupon both Mr. Colby and myself wrote to the editor of the *Graphic*, requesting him to state how he obtained possession of the original letter, so as to get the *facsimile* prepared; but neither of us received a reply. I then got a gentleman residing in New York to call on the *Graphic* editor on the subject, and was informed that the said editor declined to say how he obtained possession of the letter. Thus the matter stands, and is as inexplicable to-day as it was at the time it happened.

I entirely exonerate the editor of the *Banner* and his associates from any complicity in the matter, and I trust Mr. Crookes, after this explanation, will see that his imputation against American honour is wholly unfounded.

The publication of the letter in the *Banner* I alone am answerable for; and as I explained in my letter to Mr. Crookes that my object was to meet a statement in a public journal, I of course thought that he must have felt that the reply he forwarded would in all probability be made public use of.

Boston, U.S.A., December 7, 1877

ROBERT COOPER

P.S.—Mr. Crookes errs in speaking of me as "a Boston gentleman." I am an Englishman temporarily located here.—R. C.

Philadelphia Diploma

IN NATURE, vol. [xvii. p. 153, it is stated that "A 'Dr.' Harmuth, in Berlin, who received his diploma from Philadelphia, was lately sentenced to pay 300 marks for using the prefix publicly." It is but just to so old and respectable a university as that of Philadelphia to point out that "Dr." Harmuth's diploma could not have been genuine. So-called "Philadelphia degrees" of all sorts are sold by agents, but they have no connection with the University of Philadelphia, nor have they, at present, any connection with the city, though the author of this scandalous imposition once lived there and carried on a disreputable practice as a quack doctor. The public should still be on their guard against Bogus degrees, for diplomas purporting to issue from several American and German universities are still to be had, in

some cases on examination *in absentia* and payment of the fee, in others by a money payment only.

C. M. INGLEBY

Valentines, December 26, 1877

Royal Dublin Society

IN justice to myself I beg to state that my function as editor of the Natural Science papers in the "Scientific Proceedings of the Royal Dublin Society" begins only with Part 2 of that journal, and that I had no knowledge whatever of the material contained in Part 1 until it had been printed and circulated. By publishing this I shall be greatly obliged.

ALEX. MACALISTER

Anatomical Museum, Trinity College, Dublin

The Meteor of November 23

I HAVE just seen Capt. Tupman's letter in NATURE (vol. xvii. p. 114). I can give a fairly accurate estimate of the direction of the meteor from Llandudno at the time it burst. Sitting in a lighted room my eye was attracted by a bright bar of light across the hearth-rug similar in shape to a gap in the Venetian blind caused by a broken tape. The light slowly faded out in about the same place, which was easily remembered. I listened intently for a report for perhaps about a minute, gave it up, and then heard what was somewhat like the report of a ship's gun at a short distance.

It was easy afterwards to estimate the direction of the light as two points west of (true) north, and thirty-five degrees above the horizon.

I regret that the time between the fading of the light and the report I can only guess very roughly. It may have been about two minutes.

T. S. PETTY

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THE SUN'S MAGNETIC ACTION AT THE PRESENT TIME

PERHAPS no result in magnetism has excited so much interest as that which has connected the varying diurnal oscillation of the magnetic needle, and the frequency of the aurora polaris, with the spotted area of the sun's surface, in a common cycle of ten and a half years. Various investigations have been undertaken in order to determine whether other phenomena could not be found which would take a place in this chain.

That the movements of the magnets and the corruscations of the aurora are due to the cause which produces the immense chasms in the sun's envelopes there can be little doubt; but we know nothing of the mode in which the sun acts on our earth to produce these effects, and we have reason to believe that this ignorance has prevented us hitherto from tracing to the same cause atmospheric variations which have been attributed altogether to the solar heating action.

Any facts, then, as to what the sun is doing at the present time with the earth's magnetism will not be without value, whether we regard the facts alone, or as connected with their hypothetical relations to atmospheric phenomena. It should always be remembered, however, that the variations of magnetic oscillations in the decennial period, shown at any one station on the earth's surface, agree generally with those shown over the whole globe, while the meteorological phenomena are so much affected by conditions of position that it is difficult to distinguish what is due to local and what to cosmic causes.

It is well known to those who have studied this subject, that the interval from the time when the sun has fewest, till that when he has most, spots has been less than that from the maximum to the minimum; and that the same fact has been observed in the case of the magnetic oscillations. The way in which the changes of the latter occur near the times of successive minima has not, how-