

LETTERS TO THE EDITOR

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[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Natural Science for Schools

I WAS glad to see that "Science Master" had pointed out some of the difficulties in the way of applying the principles laid down in Prof. Armstrong's valuable paper in your number for November 6 (p. 19). The difficulties to which he has adverted relate mainly to those gratuitously thrown in the way of sound and useful practical teaching in grammar-schools by boards of examiners. Another difficulty I ventured to point out in the brief discussion upon Prof. Armstrong's paper at the Educational Conference of the International Health Exhibition, but it did not receive the attention which I think it deserved—partly, perhaps, owing to press of business, and partly, perhaps, also to the fact of the naturally somewhat strong representation of South Kensington interests at a conference held within the shadow of the Brompton Boilers. Prof. Armstrong appeared specially to recommend his mode of teaching "in science classes, such as those held under the auspices of the Science and Art Department," and towards the end of his paper he seems to recognise only one difficulty in the way of introducing it generally, viz. it "undoubtedly involves more trouble to the teacher than that ordinarily followed," and he appears to hint that the present method is mainly due to the incapacity of the teacher, as he says, "I do not believe that it is because the Department consider it" (the system) "a satisfactory one; but they know full well that it would be unwise to legislate far in advance of the intelligence and powers of the majority of the teachers." There are many teachers who are only too anxious to teach, not chemistry merely, but physics and other branches of science upon a sensible system, and who would willingly take considerable trouble to attain that end, but the difficulty is that, were they to do so, they would not get paid for their work. The insane system of *payment by results* is responsible for the greater part of the bad and indifferent teaching of science in this country, and the real trouble is, not that something better is in advance of the intelligence and powers of the majority of teachers, but that it is in advance of the intelligence and powers of the majority of examiners. The Department accept as their primary axiom that no teaching is to be paid for except that which can be exactly tested and appraised by certain examiners; and so no teaching, whatever its educational value, is counted worth anything by them except that which is susceptible of being weighed and measured. I took the liberty at the discussion of asking Prof. Armstrong whether he had ever taught a class on his methods, and if that class was presented to the Department for examination, and if so what was paid for it, and I made bold to express my own opinion that the result would be either *nil* or despicably small. My question received no answer, but I got plentifully snubbed—firstly, that a science teacher should even think of such a subject as remuneration, and secondly, I was informed that practical teaching always paid best. But as it appeared that my critics had misapprehended the point at issue, and were not speaking of the kind of teaching advocated by Prof. Armstrong at all, but thought that *practical* teaching meant allowing the class to see certain experiments performed by the teacher himself—a mode of teaching which I am quite agreed with the reader of the paper in considering quite *unpractical*—I did not feel satisfied that my question was answered, and with your permission will again propound it. It is not a sufficient answer to say that the most practical teachers earn the best results—I am a science teacher of quite sufficiently long experience to know that—*provided it is strictly on the lines laid down by the Department*. What I doubt is whether *sensible* practical teaching would produce any pecuniary results.

Certainly, in what is called (*lucrus a non lucendo*) practical chemistry it would not: there nothing but test-tubing can be weighed and measured; and whereas in former years a knowledge of the modes of preparing and experimenting with certain of the more common elements and compounds counted for something in the elementary stage, it has lately, by successive alterations in that direction in successive issues of the Directory, become more exclusively test-tubing.

In physics I presume the intelligent teacher would be glad to teach his class in light, heat, and sound, to make some of the more important measurements, to verify the laws of reflection and refraction, to measure the refractive index of glass, to calculate the foci of various lenses, to determine the latent heat of water and steam, and the specific heat of one or two substances and a few other similar things, not many of which could be introduced in a course of thirty lessons of one hour each; in electricity and magnetism, to establish the laws of intensity, to construct an electroscope, a galvanometer, and a Wheatstone's bridge, to measure the resistance of a few lengths of wire, to determine the E.M.F. of a "cell," &c., in which case the same limits would soon be reached. But would such a course pay? I venture to say not, and the Department have not even given to practical physics the scant encouragement which they afford to so-called practical chemistry. I say *scant encouragement*, because, by a series of red-tape regulations, which are strictly adhered to, they do their best to render the study of practical chemistry needlessly expensive to the committees and unremunerative to the teachers.

I shall probably be told—firstly that the teacher of a science class has no need to limit himself to thirty hours for a course; and secondly, that he should not make remuneration his first consideration. On the first point I reply that he is practically limited in most cases by the length of time during which it is possible to get students to attend: the month of September is as early as it is practicable to commence a course, and the examinations are early in May, so that one lesson a week, allowing for necessary holidays, cannot much exceed thirty lessons. To give two lessons per week would be to occupy the time of two classes for the remuneration—generally poor enough—of one; this, of course, virtually brings us to the second point, as to which I would say that, as in other professions men do not work for inadequate remuneration, I do not see why the science teacher should be expected to be more philanthropic; that neither the clergyman, the lawyer, nor the physician professes to regard money as his chief consideration, yet that the remuneration of each of these professions is far before that of the science teacher, at all events of him who works for the Science and Art Department; and lastly, that that particular line of criticism does not usually come from those who are themselves working from philanthropic motives, but from those who are pretty well paid for their labours, and who would despise the modest reward of the "*payment by results*" teacher.

I hope I shall not be misunderstood as disagreeing with Prof. Armstrong's views; it is, on the contrary, because of my full agreement with them and that I am anxious that those science teachers who are sufficiently advanced in intelligence (and I am persuaded that they are not so rare as Prof. Armstrong seems to think) to adopt a truly educational mode of teaching, should have no needless obstacles thrown in their way, that I venture to address you and to repeat before a larger audience those arguments which I made use of before the smaller auditory at the Health Exhibition.

I for one should be only too glad to see the scope of the science teaching under the Science and Art Department widened, and to know that encouragement was given to the intelligent and advanced teacher to get out of the grooves in which it appears to be the present policy of that Department to retain him.

WALTER A. WATTS

Farnworth Grammar School, November 20

Do Flying-Fish Fly?

I CANNOT pretend to the great experience of Mr. R. W. S. Mitchell in observations on aerial movements of the flying-fish when for a brief space he leaves his native element; but during one voyage from the Isthmus of Panama to England *via* the West Indies I lost no opportunity (of many) of watching these beautiful creatures, sometimes very close indeed to our steamer. The opinion I formed at the time and still retain was that there was constant very rapid motion of the great lateral fins whilst out of the water, so rapid, indeed, that the strokes of the fins could not be counted. From what Mr. Mitchell says, he evidently counted the strokes of the wings (pectoral fins), not by seeing the movements of these, but by the "impressions made on the oily surface of the water," impressions apparently similar to those made by a cormorant or other diver when taking wing from the sea.

The movements of the side fins whilst the fish was in the sea or touching the surface, would be much slower than would be the

case when it was wholly in the air, because, to be of any use then, the strokes would have to be so rapid as to be scarcely countable, as is the case with certain sea fowl (notably auks) which use their wings (with a comparatively slow stroke) whilst swimming *under* water, but when flying move them so rapidly that the strokes can either be counted with difficulty or not at all. On watching flying-fish whilst in the air, I noticed a flickering of the fins, indicating what I believe to have been rapid motion.

As Mr. Mitchell's observations, on which he chiefly relies, were made when looking down from the high bows of a steamer, the "waving from side to side" of the tail of the fish, being a lateral motion, was clearly seen, whilst the movements of the side fins would be less easily discernible.

Finally, could the impetus acquired by the fish, when springing from the water, carry it through the air "50 or 100 yards" (Mr. Mitchell's estimate) without the aid of any additional propelling force during its flight? If so, the initial velocity must have been very great.

JOHN RAE

4, Addison Gardens, W., November 27

The "Jeannette" Drift

IN NATURE of November 20 (p. 66) you give an account of the finding of some relics of the *Jeannette*, which have been picked up on an ice-floe at Julianhaab, in lat. 61° N., long. 46° W., near the south point of Greenland, and which must have drifted from the New Siberian Islands in lat. 75° N., long. 155° E., where the *Jeannette* was squashed three years ago. This I consider a most important find with regard to Arctic navigation and discovery. The question arises, How did the ice-floe get to Julianhaab? I propose the following solution. The Siberian Islands bear nearly due north from Julianhaab, and in a straight line up Davis's Strait, Baffin's Bay, Smith's Sound, Lincoln Sea, &c., and across at a distance of about 250 knots from the Pole. I think it most probable that the floe may have drifted through "the unknown," or what Osborn calls "the land of the white bear," the large unexplored area to the west of Banks's Land, and have got into Baffin's Bay through one of the sounds on its west coast—either by Jones's Sound, where the tide runs eastward at the rate of two knots an hour ("Inglefield," p. 77), or by Banks's Strait and Lancaster Sound.

We know that the icebergs come *down* Baffin's Bay and Davis's Strait into the Atlantic, and the floe has had fair success in navigating through a distance of 2700 knots to reach Julianhaab in three years.

I cannot for a moment suppose that this ice-floe has come from the Siberian Islands, *via* Francis Joseph Land, Spitzbergen, Iceland, to Greenland, a distance of 3600 knots, for that course would have been directly *against* the Gulf Stream, in which no floe could last three years, even if it were 26 feet thick, as Inglefield found them in Jones's Sound, or 40 feet thick, as McClure found them in Banks's Strait. McClure found the current west of Point Barrow going 2 knots per hour to the south, evidently making for Behring's Strait, while of the north-west point of Banks's Land the drift was north-east 1 knot an hour, evidently going towards Lancaster Sound.

I therefore conclude that the *Jeannette* relics could not have come westward, they *must* have come eastward, and this proves that there is a course open which is unknown to us.

I would suggest that a number of very strong buoys, capable of resisting ice-pressure, should be set adrift on ice-floes on various parts of the Siberian coast, each numbered indelibly, so that when recovered it could be ascertained whence they started, and their course *might* possibly be ascertained from the Eskimo, who may have seen them, or by other means. This experiment is worth a trial.

R. S. NEWALL

Ferndene, November 30

A Meteor Visible in the Daytime

AT Waterpark, just below Waterford City, about 4 p.m. on October 15, my attention was attracted by a flash of increased light. Looking up, I saw in the south-south-west, about half way between the horizon and the zenith, a bright meteor slowly sailing nearly due west, its apparent size about half that of the full moon, intensely white in colour at the centre, passing into blue at the circumference. It described a low arc, and was in sight for several seconds, leaving a trail of indigo blue with lighter

luminous edges. The meteor disappeared behind some clouds which concealed the sun at a considerable altitude above the horizon.

JAMES GRAVES

November 27

Noon-Glow

WHILE waiting at the telescope shortly before noon this date to note place of sun-spots at meridian passage, masses of cloud formed suddenly in a clear sky overhead, and drifting slowly due south, obscured a peculiarly brilliant sun. No sooner had direct light been intercepted than the upper air above cloud and sun's place appeared filled with the latterly common white glare (as of attenuated peat-smoke highly illuminated), which soon became suffused with the now familiar rose-tint, apparent also between the clouds and on south-south-west horizon, but not beneath the sun on meridian. The sun's apparent meridian altitude being 16° , the superior limit in altitude of rose-tint was 39° ; the colouring being monotone throughout, and not to be confounded with that of halos. Fearing ocular deception, as often happens from fatigue of eyesight, I asked an intelligent companion to verify observation, more especially as the diffused white glare at first slightly masked the tinting as compared with that of other "glows." It seems, however, plain that the terms "fore-glow" and "after-glow" no more cover the entire field than the Krakatoan dust.

D. J. ROWAN

Kingstown, November 24

Rosy Glow about the Moon

AFTER watching for some time this evening a lovely twilight in the west, which though bright and luminous was not remarkable for strong colour, I turned toward the south-east, when the moon, now well up, was shining through detached fleecy clouds, and was surprised to see about her a rosy-coloured glow, very like that so often seen about the sun; the nearer clouds, though very high, telling as a cold almost greenish grey upon it. This glow, of course, was much lower in tone than that about the sun, but both in character and extent just like it, and quite distinct, and broader than those prismatic hues often seen about the moon, and called by sailors "cock's eyes" or "peacock's eyes." This was at 4.45 p.m., and as the twilight faded the glow disappeared, from which I infer that it was caused by vapour lying high enough in the south-east to catch some of the very last rays of the sun, but too far east to give a glow in the west.

I see that a correspondent of the *Standard* telegraphed that on the evening of the 25th "a sunset equal in splendour to those of last autumn was seen over the Yorkshire wolds. The predominating hue was a rich crimson." The weather here was cloudy that evening, but between narrow openings in the clouds the sky was the colour of rich painted glass of a ruby-red tint about 4.30 p.m.

ROBERT LESLIE

6, Moira Place, Southampton, November 28

Wild Fowl Decoy

MAY I ask if any of your readers who are interested in wild-fowl decoys will send me the names and positions of any past and present ones they may happen to know or have heard of. I am endeavouring to save the history of decoys from oblivion, and though I have many hundred letters, maps, and sketches connected with this interesting subject, still I may have a great deal of information yet to obtain. I think the subject deserves a standard work, or I would not trouble you.

RALPH PAYNE GALLWEY

Cowling Hall, Bedale, November 24

Prehistoric Man

DURING October last, the sanitary authorities of Gloucester City had occasion to make some excavations in the timber-yard of Messrs. Booth and Co., and in the Bristol Road adjoining this yard, for the purpose of laying down a new sewer. In the course of this operation the workmen disinterred, from a bed of plastic clay, three human skeletons, occupying a position which appears to suggest that the remains in question are probably those of prehistoric man.

Arriving accidentally on the spot some two or three days after the actual find, I learned, to my great regret, that the skulls, two of which had passed through my friend Mr. Wm. Booth's