

LETTERS TO THE EDITOR

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Oceanic Circulation

As some of your readers may be misled by Mr. Croll's reiterated and uncontradicted assertions, that he has *demonstrated* the fallacy of the doctrine of Oceanic Circulation advocated by me, I think it right that they should be made aware that these assertions receive no support from the physicists and engineers, who must be much better judges than either Mr. Croll or I can be, as to the value of the *data* on which his computations are based. His arithmetic may be perfectly correct; but if his fundamental assumptions are wrong, or inapplicable to the case, his demonstration utterly fails.

The report, now in the press, which I have presented to the Royal Society, as to the Deep-sea Researches, on which I was occupied during the autumn of last year, contains my reply to Mr. Croll's argument. And I profited by the unexpected delay in its publication to bring the principal points of that reply before the Mathematical and Physical Section of the British Association at its recent meeting, with the view of eliciting the opinion of the authorities there assembled, as to the soundness of my argument.

I do not think that I claim too much in saying that this opinion was given by Sir William Thomson, and other distinguished physicists, most explicitly in my favour.

Having also had an opportunity of bringing the question under the consideration of Mr. Hawksley, whose experience as a hydraulic engineer is probably second to that of no one living, I found him entirely of my mind; and Sir John Rennie has repeatedly expressed himself as altogether concurring with me.

So far as I know, therefore, Mr. Croll, in his reiterated assertion that water will *not* find its own level—for that, in plain English, is the position he takes—stands "alone in his glory."

The *facts* which I embodied in a paper presented at the same meeting to the Geological Section, in regard to the contrast of temperature between inland seas and the ocean with which they communicate, seem to me inexplicable on any other view than that of a *deep underflow* of Polar water towards the Equatorial area; and this necessarily involves, as its complement, an *upper flow* from the Equator towards the Poles.

When Mr. Croll shall have given some other *rationale* of these facts, he may fairly claim consideration for it. At present I venture to submit that all the *facts* at present known are in my favour; and that Mr. Croll's asserted refutation is purely *theoretical*.

October 4

WILLIAM B. CARTENTER

Consciousness and Volition

IN an interesting review of Lankester's "Comparative Longevity in Man and the Lower Animals," which recently appeared in the *Times*, is the following sentence:—"Once commenced, its continuance (that of the act of walking) is quite involuntary, and may even be unnoticed by the consciousness." I am anxious to ascertain, from those readers of NATURE who have paid special attention to psychology, whether this is, in their opinion, a correct statement of the condition of the mind during such an habitual act as that of walking. There are undoubtedly certain actions of the body, those denominated reflex, which are performed without any exercise of the will whatever; but these are all either momentary, or, if continued, are nearly uniform and unchanging. To me it seems impossible that any action which is constantly varying can be wholly involuntary. Take, for instance, the motion of the fingers in writing: we are absolutely unconscious of the exercise of the mental faculties by which each successive change in the position of the pen is regulated; but yet is it not certain that each up-stroke and down-stroke is the consequence of a separate effort of the will? There is here an evident connection between the state of mind at the time and the action of the body; and by what other means is it possible to suppose that the mental act which conceives the word we are writing can convey its instruction to the fingers? Certainly not by any process of instinct. A better instance perhaps is in the motion of the muscles of the face and throat in speaking. These muscles are entirely under the control of the will; and every separate motion of them must surely be effected by a distinct voluntary effort, of which however we are entirely unconscious. The same

seems to me the case in walking. When we sway the body out of the perpendicular in turning a corner, I am at a loss to understand how this can be performed involuntarily. The explanation seems to me to be that consciousness cannot, so to speak, work so fast as volition, and therefore cannot take cognizance of a large number of rapidly successive acts of the will. The question is not so much one of a nice metaphysical distinction as simply of a correct use of terms, although I am afraid I am opposing the views of such high authorities as Huxley and Carpenter. We have been made familiar with the term Unconscious Cerebration. Is there not also an enormous field of Unconscious Volition?

London, Sept. 28

ALFRED W. BENNETT

Phosphorescence in Fish

I HAVE noticed the phosphorescence in fish on two occasions. Once on a calm night, wind light and sea smooth, in the S.E. trades, lat. 18° S., a shoal of porpoises was playing about under the bows of the ship, and darting under her keel for a space of nearly half an hour. Each looked like a piece of burnished silver on blue velvet. They presented the most beautiful appearance. We were not within "soundings." Every wavelet was covered with phosphorescence during the whole of that night, before and after the porpoises were seen. They manifestly could not have been the cause of this. As I leaned over the side and watched them, it seemed to me that their phosphorescence resulted from the condition of the water. Off the coast of South America, in about lat. 22° S., long. 30° W., weather much the same, a small shark accompanied the ship for some time, and presented the same appearance. The sea was brilliantly phosphorescent. The fish could be seen deep down. In both cases I saturated paper with the sea water. When dry the microscope failed to detect any organic matter, nothing but crystals.

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ARTHUR BURNIS

On a Measuring Apparatus for Direct-Vision Spectroscopes

§ THERE are few who possess Browning's "miniature," or other small direct-vision spectroscopes, but must have felt the want of some means of measuring the positions of spectral lines; and, indeed, little useful spectrum work is possible without it. I think, therefore, a description of a simple arrangement I have used for some time for this purpose may prove useful.

In the "miniature" spectroscope the outer face of the prism is inclined to the axis of the instrument at an angle of about 40°. Opposite this a hole of about 2 mm. diameter is drilled in the sliding tube, care being taken to avoid injury to the prism. It is obvious that, through this aperture, lateral objects will be visible superimposed on the spectrum. If now a scale be set up opposite to the hole, it will be clearly seen, and the lines may be easily measured by it. The most convenient scale is one of transparent lines on a dark ground (photographed on glass from a scale drawn in black upon white paper), and illuminated by a lamp behind. A paper scale, preferably drawn in white upon black paper, may also be employed. In this case a common retort-stand clamp serves conveniently to hold the spectroscope, while the scale is laid horizontally on the table below. A sheet of paper, may be substituted for the scale, and the instrument used as a camera-lucida. Observation of faint spectra will be much assisted by shading extraneous light with black velvet, and covering the scale on paper with a black sliding screen, with an aperture through which only one or two divisions of the scale can be seen at once. A double scale, of which one gradation corresponds with the upper and the other with the lower edge of the spectrum, is also advantageous.

With a miniature spectroscope, and scale of millimetres at 25 cm. distance, the separation of Na and Li is about 13 divisions, and can easily be read to 0.2 mm.

The arrangement would, I think, be very applicable to the microspectroscope. The small hole is no inconvenience in ordinary use, and can easily be covered by the finger or by a small piece of black paper inserted in the tube.

In conclusion, I would urge the necessity of noting a sufficient number of reference lines at each observation, and the desirability of reducing measurements to the wave-length scale, for which purpose Dr. Watts' "Index of Spectra" will be found invaluable.

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