LETTERS TO THE EDITOR.

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The Intake of Carbon Dioxide—a Correction.

WILL you give me the opportunity of making the following correction in my Presidential Address to the Chemical Section of the British Association.

I stated incidentally that Mr. F. F. Blackman, in his well-known experiments on the intake of carbon dioxide into the two sides of an assimilating leaf, employed air enriched with that gas to the extent of 4 per cent. and upwards.

Mr. Blackman has pointed out to me that in the experiments in question he used air containing only from 1·8 to ·33 per cent., and that since the publication of his earlier results he has still further reduced this amount. In fact he is of the opinion that his method is applicable to the measurement of the intake of carbon dioxide of even a much greater degree of dilation.

The error was an inexcusable one on my part, but does not affect the main argument that the natural rate of intake cannot be directly deduced from experiments in which the carbon dioxide content of the air materially departs from the normal amount 0·03 per cent. HORACE T. BROWN.

52 Nevern Square, Kensington, S.W., September.

Geological Time.

IN his Presidential Address to Section C at Dover, Sir A. Geikie has offered a bold challenge to Lord Kelvin and those who agree with him by calling upon them to give due weight to geological phenomena in forming an estimate of geological time. Permit me to say what I think about it.

It seems to me probable that, when the grand idea of the universal dissipation of energy had occurred to Lord Kelvin, he saw that the principle must be applicable to the earth, and that, if the law of conduction of heat could be used, he might from it obtain an estimate of the world's age. He then instituted his important experiments to determine the conductivity of rocks *in situ*, and found the value 400 (more or less), the units being a foot a year, and a degree Fahr. But it was necessary, for his calculation to succeed, that he should assume the earth to be solid. If I do not misjudge him, I think he then sought for arguments to prove this point. Now, I cannot but think that the proofs of solidity on which physicists rely are by no means convincing; and if the earth is not solid, Lord Kelvin's estimates are without foundation. Moreover, it is not sufficient that the earth should be solid at the present, to which these proofs refer, but it needs to have been so from the beginning of the time to which his estimates go back.

Prof. G. Darwin, in his book on the tides (p. 237), has done me the honour of referring to my "Physics of the Earth's Crust" as if I am an arch-heretic on this question of solidity. Whether my arguments are beneath notice, or whether there is a difficulty in answering them, I do not know; but they have never been refuted, while they are held to be of decided force by some geologists, and among these by the Indian Geological Survey.

Harlton, Cambridge, September 25. O. FISHER.

The Terrestrial Gegenschein.

I DO not know whether the phenomenon I am about to describe has ever been noticed. The circumstances under which it is noticeable must occur rarely. They are these:—

I spent some time of the summer of 1898 on an isolated mountain peak, surrounded by lower mountains whose sides were densely wooded. The result was that near the time of sunset the shadow of my own mountain peak was visible on a mountain side which might have been three or four miles distant. One evening I amused myself by watching the shadow of the peak as the sun was descending. My attention was attracted by an illumination in the direction opposite the sun so strikingly resembling the astronomical gegenschein, that at the first glance I saw in it an explanation of the latter. It consisted of a somewhat bright glow, which might be a degree or two in diameter, but which shaded off by such imperceptible gradations that a definite extent could not be assigned. A little study, however, showed an explanation. As I have said, the mountain on which the glow was seen was densely wooded. In such a case the shadows of those leaves and branches which the sun's rays first reached fell upon the interior foliage and obscured it. But an observer looking from the exact direction of the sun will see through the foliage as far as the sun's rays extend. In other words, the visible surface on which he is looking will be entirely illuminated by the sun's rays, whether this surface is formed of the outer strata of foliage or of a strata ever so far inside, which can be seen only through the crevices in the outer stratum. The shaded interior will be entirely invisible to him. But if his point of view is in a direction ever so little oblique, he will see only the outer foliage illuminated, while more or less of the interior foliage which he sees will be in the shadow. Thus the region exactly opposite the sun will be seen in its full brilliancy, while the neighbouring region will be a mixture of light and darkness. At a distance of several miles this compound of light and darkness will be fused into a single half-shade, strongly contrasting with the full brilliant light of the opposite point.

It is clear enough that we cannot have such a state of things as this in the case of the astronomical phenomena. Yet the phenomenon seems to be of sufficient interest to warrant its being placed on record. S. NEWCOMB.

The Cause of Undercurrents.

IN NATURE of August 3, p. 316, is given a letter from Rear-Admiral Sir William Wharton, in which he states that he is diametrically opposed to my opinion about the double currents in the Straits. He says that "Admiral Makaroff considers that difference of density of the water is the primary, and, indeed I gather he thinks, the only cause of these opposing currents; but he brings no evidence beyond theoretical considerations in support of his belief"; further, in his letter, Admiral Wharton refers particularly to the double current of the Bosporus, of which I spoke in my lecture at the Royal Society of Edinburgh. I cannot leave unnoticed remarks from so distinguished a hydrographer, who, during his long work, has contributed so much to the advance of science. My researches about the Bosporus are published only in Russian, in a book named "On exchange of water between Black Sea and Mediterranean" (St. Petersburg, 1885). Should Admiral Wharton know my language, he would easily come to the conclusion that my opinion about double currents in the Bosporus are based upon the observations made in 1881 and 1882. I then invented an instrument for measuring the current at different depths, and gave the name of "fluctometer" to it. The instrument consists of a propeller revolving on a horizontal spindle. A bell is attached to the propeller, and at every revolution of the propeller it strikes twice. As water is a very good conductor of sound, the number of revolutions could be counted through the bottom of the ship (provided the ship is not sheeted with wood) at all depths to which the instrument was lowered (40 fathoms). I used to anchor in the middle of the Bosporus for a couple of days at a time, and make a series of observations every two hours. In order to obtain more detailed data, I used to take the samples of water from the same depth to which the fluctometer was lowered. Twice I used to go along the Bosporus from the Black Sea to the Marmora Sea in order to learn in what depth is the limit of two currents. In volume xxii. of the *Proceedings* of the Royal Society of Edinburgh, Plate I. shows a position of the limit of two currents, mean velocity of both currents, and specific gravity of water. In Plate II. is given a sketch of my "fluctometer."

I am sorry that the limits of this paper do not allow me to give particulars of my observations, but I believe some of my deductions, worked out from direct observations, would be interesting to English readers.

Mean velocity of the upper current, 3¼ feet per second. It varies from 0 to 10 feet per second in certain places.

Velocity of the upper current diminishes with every fathom of depth.

Limit between two currents close to the Marmora Sea is at 11 fathoms. It gradually goes down to 27 fathoms close to the Black Sea. Limit between two currents is influenced by winds and by barometrical pressure, but not very much.

Lower current has close resemblance with the river. Its velocity does not vary very much. We never found anywhere