entered by any insect of considerable size, which must inevitably have carried away the pollinia with it. The fact that the Bee Orchis, the most "imitative" of all our native plants, is never visited by insects, is a very suggestive one. If, as might well have been assumed, the object of the "mimicry" is the attraction of bees, the device appears to have signally failed.

London, July 17 ALFRED W. BENNETT

### Saturn's Rings

As Lieut. Davies has thought it necessary to refer to your remarks about the satellite theory of Saturn's rings—and in so doing has named my work upon Saturn (which you had *only* referred to without naming) it may be as well for me to mention, that I nowhere in that work claim the theory as mine—and that, whenever I have seen it referred to as mine, I have as publicly as possible disclaimed all title to it.

Permit me to add, that, whatever opinion we form of Lieut. Davies's views, he deserves our thanks for bringing out a treatise so full of work, from cover to cover, as his "Meteoric Theories." Such examples are a good deal needed in these days.

8, Wellington Villas, Brighton RICHARD A. PROCTOR

### Ocean Currents

I FIND that Dr. Carpenter does not consider his experimen probative. Judging from the air of triumph with which, both in his lectures and writings, he has announced its success, I had certainly imagined that he did. But if not probative, what is it? Dr. Carpenter says it is only intended to be illustrative. What does it illustrate? It does not illustrate any currents formed in the ocean by differences of temperature; for it does not illustrate the differences of temperature to which he attributes these currents. In his letter in NATURE of July 6, he proposes an unwieldy modification of his former well-known experiment, but which still, I would submit, in no way avoids the difficulty to which I have called attention. He describes a strong freezing mixture applied to the surface through one-tenth of the length of a trough half a mile long, and heat applied to the surface also through one-tenth of the length, measured from the other end: between the cold and the hot surface there is, then, an intervening space of four-tenths of a mile, or 2,112 feet; that is to say, there is a thermometric gradient of about 50° in 2,000 feet, or 1° in forty feet. This is small enough, and we may perhaps doubt whether such a gradient could give rise to any appreciable movement; but it is 15,000 times greater than the gradient observed in the ocean, which is about 1° in 100 nautical miles; and any movement shown by an experiment which, in its details, bears no reasonable proportion to the reality, cannot be accepted as an illustration of a movement in the ocean.

Mr. Proctor, in the same way, speaks of his proposed experiment as an illustration; and, in the same way, I would say that the distortion produced by magnifying 6,000,000 times that particular detail on which he wishes to lay an emphasis, precludes our accepting it as an illustration at all. Mr. Proctor says that it is intended specially to throw light on the easterly and westerly movements; it is surely unnecessary for me to point out to him that any easterly or westerly movements, as illustrated in a cylinder such as he describes, revolving continuously and uniformly, are direct consequents of the outward or inward movement due to the differences of temperature, and are, therefore, in the strictest sense, dependent on the thermometric gradient. If, with a thermometric gradient of $\frac{1}{600000}$ of a degree in one foot, and with an angular velocity of 360° in 24 hours, Mr. Proctor succeeds in showing any appreciable movement, I and (I think I may add) many other readers of NATURE will be glad to learn the result. But this is, after all, the point I raised in my last letter (NATURE, June 29), and which Mr. Proctor considers would require many columns for its full discussion. I do not myself see that there is any room for discussion at all; and any difference of opinion that may exist can only be met by experimental demonstration.

Dr. Carpenter appears to wish to support his theory on "authority," and especially on that of the recent letter of Sir John Herschel. This is a point on which I touch with great reluctance; but I would point out, in the first place, that "authority" in matters of science carries very little weight; and, secondly, that Sir John Herschel, in the letter referred to, merely admits what he and everyone else have all along admitted, that hot water and cold, in juxtaposition, will establish a circulation. It was not for him, in a letter of private courtesy, to enter again on a discussion of the infinitesimal nature of the gradients—a discussion which he had already worked out very fully in his "Physical Geography."

But, leaving this consideration on one side, I maintain that, at the present time, the *onus probandi* rests with the supporters of the temperature theory. Its opponents have offered what is, at any rate, a rational, consistent, and tolerably complete explanation of all the known ocean currents; and they say, in so many words, that the explanation offered, in accordance with the theory of temperature and specific gravity, is neither complete, nor consistent with itself or with geographical observation. The theoretical objection of the infinitesimal nature of the thermometric gradients and of the differences of specific gravity, which has, indeed, formed the subject of these letters, is not one which I was inclined to put forward in any prominent degree. I preferred, and still prefer, to base my objection on the utter discrepancy between fact as observed, and fact as described by Captain Maury and Dr. Carpenter, in accordance with their theory.

I have elsewhere dwelt on this at great length, and do not intend to go over the same ground here, even if you were willing to afford me the space to do so; but this discrepancy, which actually and very markedly exists, does call attention to the thermometric gradients in the ocean; and when we find the same discrepancy between observation and description in the case of aërial currents, it leads to the conclusion that the infinitesimal nature of the thermometric gradients is as sound an objection to the temperature theory of atmospheric circulation, as it is to the temperature theory of oceanic circulation. I refer here to the last sentence but one of Mr. Proctor's letter. The last sentence, I must confess, I do not understand. I do not see what effects solar light can produce, or even be supposed to produce, on the depths of ocean, to which no light penetrates; still less do I see how to integrate them.

J. K. LAUGHTON

### Formation of Flints

IN your report of the discussion that followed the reading of my paper on Flint, before the Geologists' Association on June 2nd, Prof. Morris is said to have asserted that the views I suggested were first propounded by Dr. Brown of Edinburgh. I think the Professor must have been slightly misrepresented in this; at all events I must most decidedly decline to be coupled with Dr. Brown, or to allow myself to be associated with his very remarkable statements. These may be found in the Trans. Roy. Soc. Edinb., vol. XV. He asserts that carbon is transmutable into silicon; at p. 229 he says, "Carbon and silicon are isomeric bodies, and that the former element may be converted into a substance presenting all the properties of the latter." At p. 244, "3·04 grains of silicic acid were extracted from 5 grains of paracyanide of iron;" at p. 245, "5·4 grains of silicic acid were procured from 30 grains of the ferrocyanide of potassium," and "there were obtained 9,334 grains of silica from 3,240 grains of ferrocyanide, although some of the product was lost in two of the operations." The view I advocated as explanatory of the formation of flints was the *substitution* of silicon for carbon, not a transmutation, and I distinctly showed the source from which the silicon was derived. Dr. Brown's statements are so extraordinary that I could scarcely believe them serious. I find, however, in the same volume of the "Transactions" that they were most patiently examined and confuted by Dr. George Wilson and Mr. John Crombie Brown, and they say, "We tried the greater number of Dr. Brown's processes, and rejected them one after another without pursuing their investigation further, on finding they would not yield quantitative proofs of the conversion of carbon into silicon. The limited time, which from various circumstances we could devote to the subject, obliged us to follow this course; and the confident expectation we entertained till a recent period that each new process would supply what the rejected ones had failed to afford, led us to neglect noting many particulars of our early trials which otherwise we should have recorded. . . . In conclusion, we need scarcely say that we have been unable to supply any proof of the transmutability of carbon into silicon."

I have one more objection to make to the report. I did not say that flints were merely silicified sponges. I believe that such is the case with *some* flints, but certainly not with all. I hope you will find space for this rectification of manifest errors.

M. HAWKINS JOHNSON

379, Euston Road, July 11