

these makers deserve great credit for the help they have thus given to the success of the expeditions.

In all observations of the moon for determining the longitude there are of course numerous corrections which must be applied. Among these none is more important than the correction for the parallax of the moon.

RECAPITULATION.—In the case of every nation depending upon De l'Isle's method and in the case of every expedition when only one contact is observed, the longitude must be determined with very great accuracy. This can be done by any of the following methods :—

1. By rockets, or flashing signals.
2. By a trigonometrical survey.
3. By the aid of chronometers, in which it would be unwise to neglect the method lately introduced of adding to the chronometers one which is uncompensated.
4. The telegraphic method, in which it is not desirable to use relays, since very long lines with a Thomson's reflecting galvanometer will give good results, while the employment of relays is objectionable.
5. By observations of the moon's position which may be made by either of the three following methods :—
  - (a) By occultations of the moon.
  - (b) By transit observations of the moon and moon-culminating stars.
  - (c) By aid of an alt-azimuth.

GEORGE FORBES

(To be continued.)

#### OCEAN CURRENTS

I OBSERVE that in NATURE, vol. ix. p. 423, Dr.

Carpenter re-states and maintains his opinion that polar cold rather than equatorial heat is the *primum mobile* of his general oceanic circulation. In my papers in the *Philosophical Magazine* for Oct. 1871 and Feb. 1874 I have proved, I trust, to the satisfaction of any physicist who will be at the trouble to examine what I have written on the subject, that this notion is based upon a confusion of ideas in regard to the way in which difference of specific gravity produces motion. It is not my object at present to enter into any further discussion of this elementary matter; but I wish briefly to refer to a new and somewhat plausible-looking objection advanced in Dr. Carpenter's article against the views I advocate in reference to under-currents. The following is the paragraph to which I refer :—

"According to Mr. Croll's doctrine the whole of that vast mass of water in the North Atlantic, averaging, say, 1,500 fathoms in thickness and 3,600 miles in breadth, the temperature of which (from 40° downwards), as ascertained by the *Challenger* soundings, clearly shows it to be mainly derived from a polar source, is nothing else than the *reflux of the Gulf Stream*. Now, even if we suppose that the whole of this stream, as it passes Sandy Hook, were to go on into the closed Arctic basin, it would only force out an equivalent body of water. And as, on comparing the sectional areas of the two, I find that of the Gulf Stream to be about 1-900 that of the North Atlantic underflow; and as it is admitted that a large part of the Gulf Stream returns into the Mid-Atlantic circulation, only a branch of it going on to the north-east; the extreme improbability (may I not say impossibility?) that so vast a mass of water can be put in motion by what is by comparison a mere rivulet, the north-east motion of which as a distinct current has not been traced eastward of 30° W. long, seems still more obvious."

The objection seems to me to be based upon a series of misapprehensions: (1) that the mass of cold water 1,500 fathoms deep and 3,600 miles in breadth is in a state of motion towards the equator; (2) that it cannot be the reflux of the Gulf Stream, because its sectional area is 900 times greater than that of the Gulf Stream; (3) that

the immense mass of water is, according to my views, set in motion by the Gulf Stream.

I shall consider these in their order: (1) That this immense mass of cold water came originally from the polar regions I of course admit, but that the whole is in a state of motion I certainly do not admit. There is no warrant whatever for any such assumption. According to Dr. Carpenter himself the heating power of the sun does not extend to any great depth below the surface; consequently there is nothing whatever to heat this mass but the heat coming through the earth's crust. But the amount of heat derived from this source is so trifling that an under-current from the Arctic regions far less in volume than that of the Gulf Stream would be quite sufficient to keep the mass at an ice-cold temperature. Taking the area of the North Atlantic between the equator and the tropic of Cancer, including also the Caribbean Sea and the Gulf of Mexico, to be 7,700,000 square miles, and the rate at which internal heat passes through the earth's surface to be that assigned by Sir William Thomson, we find that the total quantity of heat derived from the earth's crust by the above area is equal to about  $88 \times 10^{13}$  foot pounds per day. But this amount is equal to only 1-894th of that conveyed by the Gulf Stream, on the supposition that each pound of water carries 19,300 foot pounds of heat. Consequently an under-current from the polar regions of not more than  $\frac{1}{894}$  the volume of the Gulf Stream would suffice to keep the entire mass of water of that area within 1° of what it would be were there no heat derived from the crust of the earth. That is to say, were the water conveyed by the under-current at 32°, internal heat would not maintain the mass of the ocean in the above area at more than 33°. The entire area of the North Atlantic from the equator to the Arctic circle is somewhere about 16,000,000 square miles. An under-current of less than  $\frac{1}{17}$  that of the Gulf Stream coming from the Arctic regions would therefore suffice to keep the entire North Atlantic basin filled with ice-cold water. In short, whatever theory we adopt regarding oceanic circulation, it follows equally as a necessary consequence that the entire mass of the ocean below the stratum heated by the sun's rays must consist of cold water. For if cold water be continually coming from the polar regions either in the form of under-currents or in the form of a general underflow, as Dr. Carpenter supposes, the entire under portion of the ocean must ultimately become occupied by cold water, for there is no source from which this influx of cold water can derive heat save from the earth's crust. But the amount thus derived is so trifling as to produce no sensible effect. For example, a polar under-current one-half the size of the Gulf Stream would be sufficient to keep the entire water of the globe (below the stratum heated by the sun's rays) at an ice-cold temperature. Internal heat would not be sufficient, under such circumstances, to maintain the mass 1° F. above the temperature it possessed when it left the polar regions.

(2) But suppose that this immense mass of cold water occupying the great depths of the ocean were, as Dr. Carpenter assumes it to be, in a state of constant motion towards the equator, and that its sectional area were 900 times that of the Gulf Stream, it would not therefore follow that the quantity of water passing through this large sectional area must be greater than that flowing through a sectional area of the Gulf Stream, for the quantity of water flowing through this large sectional area depends entirely on the rate of motion.

(3) I am wholly unable to understand how it could be supposed that this underflow, according to my view, is set in motion by the Gulf Stream, seeing that I have shown that the return under-current is as much due to the impulse of the wind as the Gulf Stream itself.

I am also wholly unable to comprehend how Dr. Carpenter should imagine that because the bottom temperature of the South Atlantic should happen to be lower,

and the polar water to lie nearer to the surface in this ocean than in the North Atlantic, that therefore this proves the truth of his theory. This condition of matters is just as consistent with my theory as with his. When we consider the immense quantity of warm surface water which, as has been proved,\* is being constantly transferred from the South into the North Atlantic—a quantity which to a large extent is compensated by cold currents from the Antarctic regions—we readily understand how the polar water comes nearer to the surface in the former ocean than in the latter. In fact the whole phenomena is just as easily explained upon the principle of under-currents as upon Dr. Carpenter's theory.

Dr. Carpenter lays considerable stress on the important fact established by the *Challenger* expedition, viz. that the great depths of the sea in equatorial regions are occupied by ice-cold water, while the portion heated by the sun's rays is simply a thin stratum at the surface. It seems to me that it would be difficult to find a fact more hostile to his theory than this. Were it not for this upper stratum of heated water there would be no difference between the equatorial and polar columns, and consequently nothing to produce motion. But the thinner this stratum is the less is the difference and the less there is to produce motion. I have been favoured by the Hydrographer to the Admiralty with a series of temperature soundings taken along the equator, and from these I find that to so small a depth does the super-heating extend that the surface of the ocean at the equator requires to stand only four and a half feet above that at the poles in order to the ocean being in perfect equilibrium. In this case if we suppose, in order to constant circulation, that the polar column is kept in excess of the equatorial by the weight of say two feet of water, there would then remain only a slope of two and a half feet between the equator and poles.

There is another point to which, with some reluctance, I am compelled to refer. Dr. Carpenter is continually representing that eminent physicists have adopted his theories while none of them share in my objections. I can assure Dr. Carpenter that such is not the case. Only a few weeks ago one of the most eminent mathematical physicists of the present day stated to me that no one familiar with the elements of physics and mechanics, who would be at the trouble to make himself acquainted with Dr. Carpenter's theories, could ever adopt them.

JAMES CROLL

#### BIOLOGY AT CAMBRIDGE

ON the evening of Monday, 11th inst., Cambridge biologists mustered at least a hundred strong at the meeting of the Philosophical Society to hear a communication from Prof. Huxley, one of the honorary members of the Society, on the morphological conclusions to be drawn from the distribution of the cranial nerves, with especial reference to those of the seventh pair. Prof. C. B. Babington, F.R.S., president of the Society, occupied the chair. Prof. Huxley took occasion to refer in terms of the highest commendation to the researches of Stannius more than twenty years ago, on the morphological teaching to be derived from studying the distribution of nerves, and also spoke of the deductions drawn from nerve-supply by Gegenbaur, especially in his work on the "Skulls of Plagiostomous Fishes." Prof. Huxley sketched in considerable detail the distribution of the portio dura or seventh cranial nerve in man, and compared it with the homologous nerve in the frog, showing how the arrangements of branches, especially the course of the chorda tympani, which seemed anomalous in man, were a necessary consequence of perfectly obvious and natural arrangements in the lower vertebrates. He also demonstrated how the morphology of the parts might be learnt from such homologies;

\* Phil. Mag. for March 1874, p. 170.

logies; how a circuitous and apparently useless path taken by a nerve was full of meaning and instruction, and when studied in connection with facts of development and function would lead to an explanation which might be very much trusted. The relation of the tympano-eustachian tube to the bifurcation of the seventh nerve was dwelt upon, as leading to the identification of the comparatively small and simple auditory passage of the frog with the complex one of the mammal, and further to the homological identity of these passages with the spiracle of the Plagiostomes. The distribution of the fifth and seventh pairs of cranial nerves was held to agree with the view, suggested by development, that the trabecular arch is a pre-oral visceral arch, and that the pterygo-palatine is but an outgrowth of the mandibular arch.

The paper, which was illustrated by black-board drawing, with the professor's well-known aptitude, and which was a model of lucidity and careful reasoning, was loudly applauded. In a discussion which followed, Prof. Humphry drew attention to labours of his own having the object of showing the value of the teaching of nerve distribution. He acknowledged the strong case which was now made out in favour of the trabecular arch taking its position in the series of visceral arches, and thought that Prof. Parker's paper on the development of the pig's skull made it almost equally clear that the pterygo-palatine arch was similar in homology. It was also remarked that the same conclusions seemed deducible from Prof. Parker's paper on the development of the salmon, where the pterygo-palatine arch was distinct from the first and in all respects like the other visceral arches.

The practical class for the study of elementary biology, conducted by Dr. Michael Foster and Dr. Martin, is very successful this term. When thirty students entered last year the number was thought very large, and it was made up of men of several years who had previously had no opportunity of attending such a course. It was expected that a much smaller number would attend this year; but the large number of nearly forty have availed themselves of the course, and work proceeds in a most satisfactory and instructive fashion. Adequate superintendence is provided at all hours of the working day by the co-operation of four advanced students in addition to the lecturers. These are Messrs. P. H. Carpenter, Trinity College, A. M. Marshall, B.Sc., and Langley, St. John's College, and S. H. Vines, B. Sc., Christ's College.

G. T. BETTANY

#### NOTES

ON Tuesday, Sir Samuel Baker delivered the Rede lecture in the Senate House, Cambridge, before a numerous assemblage, which included all the leading men of the University in residence, and many ladies. The subject of the address was "Slavery," and Sir Samuel's narrative of his personal experiences in Africa was listened to with much interest.

It is said to be in contemplation to confer honorary degrees at the Cambridge commencement upon Sir Bartle Frere, Sir Garnet Wolesley, Sir James Paget, and Prof. Helmholtz.

It is stated that if the authorities of Owens College, Manchester, can show that they really require it, Government are prepared to make a considerable grant of money to the College.

THE Founder's Medal of the Royal Geographical Society has been granted to Dr. Schweinfurth, and the Victoria Medal to Col. P. E. Warburton, who recently succeeded in crossing the interior of Western Australia.

By later advices from Australia we learn that Major Warburton accomplished exactly what he set out to do. He traversed the continent from the MacDonnell Ranges to the coast north of