

extended by the enthusiastic local collector. The number of species described as undoubtedly British is one hundred and twenty, including the slugs, which, "though generally regarded as shell-less, have the shell placed beneath the mantle."

*A Manual of the Mollusca.* By S. P. Woodward. Third Edition. (London: Lockwood and Co., 1875.)

IN noticing a third edition of the late Mr. S. P. Woodward's well-known "Manual of the Mollusca," our object is only to indicate wherein it differs from its predecessors. The body of the work is unaltered; whilst the new editor, Mr. Ralph Tate, in order to bring the work up to the present state of our knowledge, has added an appendix, containing the description of those recent and fossil genera which, either from more recent discovery or oversight, are not to be found in it. This appendix, with its separate index, occupies eighty-five pages, and is illustrated with twenty-seven woodcuts, including drawings of *Clydonites costatus*, *Cochloceras fischeri* (Haver), *Euclyclus goniatus* (Desl.), *Nucleospira ventricosa* (Hall), &c. Its separate existence we do not object to, on account of the expensive typography of a work of the kind; nevertheless, the outlay involved in an incorporation of the two indexes into a single whole would have been fully made up for by the extra facility of reference afforded, and the diminution in the chance of any additional remarks on previously described genus being overlooked. In the preface to the second edition, which is retained in that under notice, it is remarked that "the chapter on Tunicata has been omitted, since they are more nearly allied to the Polozoa than to the Mollusca proper, and since the Molluscoidan group would have made the work inconveniently bulky." Such being the case, we cannot help asking why the Brachiopoda are not also removed. Is it not because they have shells, whilst the Ascidians are deficient in in-destructible parts; not, by the way, that Ascidians are Molluscoidan now-a-days. Additional remarks will be found on the nature of *Belemnites*; that *Crioceras* must merge into *Ancylloceras* is shown to be certain; the genera *Vermetus* and *Siliquaria* are placed in a family by themselves, at the same time that their differences from the mimetic *Serpulidæ* are explained. Several of the families are re-arranged, at the same time that the newly added genera are introduced. The work with the appendix is as accurate a representation of the state of conchology in 1871 as was the first edition on its publication. We put it thus because we can find no difference between this third edition and the second, which has latterly been bound up with Mr. Tate's appendix in exactly the same form as it appears in the newly produced work.

#### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

##### Oceanic Circulation

I HAVE just read Dr. Carpenter's letter in NATURE (vol. xii. p. 454) in reference to my paper on the *Challenger's* crucial test of the wind and gravitation theories of oceanic circulation, read before the British Association, and am somewhat astonished at the nature of the objections which he advances.

"The doctrine," says Dr. Carpenter, "to which he (Mr. Croll) applied his test, was not mine, but a creation of his own. For his whole argument was based on the assumption that the ocean is in a state of static equilibrium; whereas the theory I advocate is, that the ocean never is and never can be in a state of equilibrium, so long as one part of it is subjected to polar cold and another to equatorial heat, but that it is in a state of constant endeavour to recover the equilibrium which is as constantly being disturbed."

Those who were present at the meeting and heard my paper read, or who have since seen it in the September number of the

*Philosophical Magazine*, will no doubt feel surprised that the following paragraph should have escaped Dr. Carpenter's notice:—

"It will not do as an objection to assert that according to the gravitation theory the ocean never attains to a condition of static equilibrium. This is perfectly true, as I have shown on a former occasion;\* but then it is the equator that is kept below and the poles above the level of equilibrium; consequently the disturbance of equilibrium between the equatorial and polar columns would actually tend to make the difference of level between the equator and the Atlantic greater than  $3\frac{1}{2}$  feet, and not less, as the objection would imply."

If Dr. Carpenter will refer to my examination of the mechanics of the gravitation theory in the *Philosophical Magazine* for October 1871, "Climate and Time," chaps. ix., x., alluded to in the above paragraph, he will find page after page devoted to prove that a constant disturbance both of level and of static equilibrium is a necessary condition to circulation by gravity. Physicists may differ from me in regard to whether or not the present difference of temperature between the ocean in equatorial and polar regions is sufficient to produce circulation, but I do not expect that anyone familiar with mechanics, who has been at the trouble to read what I have written on the subject, will do so materially in regard to the way in which difference of temperature is conceived to produce motion.

It is singular that Dr. Carpenter should not have observed that his objection strengthens my argument instead of weakening it. For if it be true that the equatorial column, though in a state of constant upward motion, never attains to the height required to balance the polar column, then it must follow as a necessary consequence that the rise from the equator to latitude  $38^\circ$  in North Atlantic must be greater than I have estimated it to be; and, therefore, so much the more impossible is it that there can be any surface flow from the equator to the pole due to gravity.

The next objection is as follows:—"The only objection raised by Mr. Croll which has even a show of validity is based on the supposed 'viscosity' of water, which he asserts to be sufficient to prevent the disturbance of thermal equilibrium from exerting the effect which the gravitation theory attributes to it."

What possible connection can "viscosity" have with the crucial test argument? Suppose water to be a perfect fluid and absolutely frictionless: this would not in any way enable it to flow up-hill.

The crucial test argument brings the question at issue, in so far as the North Atlantic is concerned, within very narrow limits. The point at issue is now simply this: *Does it follow, or does it not, from the temperature-soundings given in Dr. Carpenter's own section, that the North Atlantic at lat.  $38^\circ$  is above the level of the equator?* If he or anyone else will prove that it does not, I shall at once abandon the crucial test argument and acknowledge my mistake; but if they fail to do this, I submit that they ought at least in all fairness to admit that in so far as the North Atlantic is concerned, the gravitation theory is untenable.

The Atlantic column is lengthened by heat no less than eight feet above what it would otherwise be were the water of the uniform temperature of  $32^\circ$  F., whereas the equatorial column is lengthened only four feet six inches. The expansion of the Atlantic column below the level of the bottom of the equatorial not being, of course, taken into account. How then is it possible that the equatorial column can be above the level of the Atlantic column? And if not, let it be explained how a surface-flow from the equator pole-wards, resulting from gravity, is to be obtained.

JAMES CROLL

Edinburgh, Sept. 29

##### Dehiscence of *Collomia grandiflora*

THE following account of some observations of mine on the dehiscence of *Collomia grandiflora* may possibly prove interesting to some of your botanical readers. I can find no allusion to the singular mode in which the capsules as well as the seeds of this plant become liberated. The fruit is a three-celled capsule, and is almost wholly included within the tube of the calyx. When quite ripe it is of a pale straw colour, and becomes cartilaginous and highly polished, as does also the internal surface of the calyx tube. The latter is ribbed with fifteen prominent lines disposed in threes, each set pertaining respectively to the five sepals, and extending into their free portions. These ridges may possibly help to give direction to the capsule during its exit. Dehiscence

\* Phil. Mag., Oct. 1871; "Climate and Time;" chap. ix.