

case, the task will be found to be impracticable; but even when the overlap of the small disc is greater, the task can only be achieved by actually making new cusps out of the irradiation fringes. (A figure would make this explanation much simpler.)

Prof. Newcomb says that he is decidedly of opinion that the irradiation of an extremely minute thread of light is not the same with that of a large disc. He does not seem to notice that if this is so, Venus just before, at, and just after internal contact, *must* be distorted. This even if—admitting the enlargement of the sun's disc—he denies that the disc of Venus is reduced by irradiation.

He fails also to observe that a peculiarity such as distortion, or the formation of a ligament, may escape the notice of inferior or not very attentive observers, and so all his negative observations be explained. It is no proof of superior skill in observation to see no signs of an illusory effect. Until we have observers who recognise no traces of irradiation when looking at the solar disc, we must believe that (as Mr. Stone has, I think, already asserted) the non-recognition of distortion or ligament formation is due to inattention, or want of observing skill. That this should be more common than close and careful scrutiny is not a very surprising circumstance, and proves nothing. RICHARD A. PROCTOR

Oceanic Circulation

IN NATURE of August 17, I have just seen the report of the discussion on Dr. Carpenter's paper on the above subject read at the late meeting of the British Association.

Dr. Carpenter, explaining the movements on thermodynamic principles, states that he has "found the *primum mobile* of this circulation was not in equatorial heat but polar cold," and explains that "(1) As each surface-film cools and sinks, its place will be supplied, not from below, but by a surface influx of the water around; and (2) the bottom stratum will flow away over the deepest parts of the basin, while, since the total heat of the liquid is kept up, there will be an upper stratum which will be drawn towards the cold area, to be precipitated to the bottom and repeat the action. Apply this principle to the great oceanic area that stretches between the equator and the poles, we should expect to find the upper stratum moving from the equator towards the poles, and its lower stratum from the poles towards the Equator. That such a movement really takes place is indicated, as it seems to me, by various facts."

It does not appear, however, that Dr. Carpenter has well established his claim to the theories in question, while, in a pamphlet on the same subject, published in 1869 by Dr. Adolph Mühry of Göttingen, we find such passages as the following:—"As the cause of the latitudinal circulation we have assumed the difference of temperature in the water between the equator and the pole." He honestly gives Arago the credit of being, perhaps, the first to put forward this view in 1836; and after remarking (p. 11) that it might be considered doubtful whether it is the upper warm current from the equator or the under cold one from the pole that ought to be considered the primary, he says (p. 12) "For us the primary 'arm' is the heavier, *i.e.*, the colder polar stream, which, in obedience to gravitation, falls in a horizontal direction toward the lighter water of the hot zone; and the secondary 'arm' is the returning antipolar. It moves to replace what flows away, and is, therefore, the compensation-arm."

Here, without following Dr. Mühry any further, we find the thermodynamic theory advanced by Dr. Carpenter, and his *primum mobile* as well; but by giving him credit for ignorance of Dr. Mühry's work, we may excuse him for laying claim to what is there put forward, and accepting therefore the commendation of others as unknowing as himself. J. B.

Ice Fleas

DURING a recent ramble upon the Morteratsch Glacier, I also observed a large number of the minute black creatures described by Prof. Frankland in NATURE, No. 100. My attention had been directed to them ten years ago by Lord Anson on the "snow-bones," near the summit of the *Ægischorn*. They are only nominal "cousins" of the flea (*Pulex*) of civilised life, and are not at all related to *Daphnia*, the "water flea," but are closely allied to the minute insects which are often seen on the surface of stagnant water, resembling grains of gunpowder, and skipping partly by help of their forked tail, folded under them so as to serve as a foot, hence their name *Podura*, or "skip-tail." They have been named by Agassiz *Desoria saltans*. Their food, I conjectured with Prof. Frankland, consists of "red snow" and

other microscopic *algæ*. Not being myself within reach of a good library, I can only furnish your readers with a key to further information. C. A. JOHNS

IN NATURE of 28th September, Prof. Frankland, in introducing the ice flea to the readers of NATURE, uses the expression "if known at all," and concludes by asking information about it. The glacier flea, *Desoria glacialis*, was noticed and described by Prof. Agassiz as far back as 1845, in his Ascent of the Wetterhorn on the 29th of July of that year. Not having Agassiz's work at present beside me, I cannot refer to it, but these fleas are noticed in an extract translated from an account of the ascent, and published in *Hogg's Weekly Instructor* for Dec. 1845, vol. ii. p. 221. On the Aar Glacier they are described as being scattered over the "surface of the snow in millions," elsewhere, "as being collected in masses under the stones on the ice."

R. C.

The New Dynameter

THE letter from the Rev. T. W. Webb in your last number is a very tantalising letter. He tells us, and we could not wish to have a better authority, that a new dynameter has been invented by the Rev. E. Berthon, but he does not tell us how it is constructed or where it can be obtained.

I may take this opportunity of mentioning a makeshift dynameter which I have found to answer very well when extreme accuracy is not required.

I have a pocket telescope fitted with a Cavallo micrometer, *i.e.*, a slip of finely divided mother-of-pearl screwed to the diaphragm next the eye-glass. Unscrewing the two last draws of this telescope the end of the second is applied to the eye-piece of the telescope of which the power is to be measured, and the first draw pushed in till the image of the object-glass comes sharp upon the mother-of-pearl. The diameter of the image is thus given in divisions on the mother-of-pearl, the value of which, in hundredths of an inch, has been previously ascertained.

W. R.

Notaris on Mosses

WITH reference to the notice of De Notaris' book on Mosses, I am informed by Dr. Dickie that the genus *Habrodon* was discovered in Great Britain several years ago by the late Mr. McKinlay, of Glasgow, and that he had received from Mr. Wilson about two years ago from his district *Conomitrium julianum*. Dr. Dickie sends specimens of *Habrodon Notarisii* gathered at Killin by Dr. Stirton. M. J. BERKELEY

* * * In the review referred to, Prof. De Notaris was erroneously described as of Geneva, instead of Genoa.—ED. N.

"Newspaper Science"

MY attention has just been called to a letter from Mr. David Forbes which appears in NATURE, Sept. 21, under the head "Newspaper Science," and in which that gentleman, writing from Boulogne, pathetically describes the emotions with which he read a recent "article" in the *Globe* on "Krupp's" Gun-manufactory at Essen. I need hardly say how deeply I deplore the shock which I have unwittingly been the agent of inflicting on your distinguished correspondent. It will be some small satisfaction if you will allow me to express the hope that the "desired result" of Mr. Forbes's "reluctant" compliance with the advice of his "medical man," and most wise resolve "to eschew everything scientific for the next few weeks at least, in order to recruit before the winter labours commenced," may not be utterly defeated by the perusal of "a specimen of English scientific opinion," of which I am unhappily the author. It would be a terrible reflection indeed, that a stupid error on my part had, perhaps, imperilled the accuracy and success of Mr. Forbes's "winter labours." The blunder (or rather blunders) occurred as follows:—I, too, was "knocked up with work," but being myself a "medical man" naturally only in part carried out my own prescription. I would, for the sake of Mr. Forbes, and the credit of "English scientific opinion" in the estimation of his "French acquaintance," I had exercised a little more discretion. However, unfortunately, I stumbled on the Krupp factory, and all forgetful of my dilapidated mental condition, wrote a note-paragraph (I never write "articles"), which I vainly imagined might have been innocent and interesting. It is not always possible to compress even the manuscript necessary for a paragraph on to a single sheet of paper, and I grieve to say that after my paper had passed the editorial eye three words