

as to the nature of the law by which the density increases internally.¹ In the article in *Nature*, I adduced the argument on which Mr. Williams comments, as a slight corroboration of the conclusions as to the physical constitution of the planet, which have been derived from telescopic inspection, and from observation of the ellipticity of figure.

From the latter part of Mr. Williams's letter I must beg leave to dissent. If one were to describe the oceanic tides on the earth as a reeling motion of the solid earth within the sea, it would surely be a somewhat obscure description of the facts, but the reeling of the Jovian nucleus *can* only be a tidal phenomenon.² Now the masses of the Jovian satellites are so small, that they can only raise very small tides, except indeed on one hypothesis, of the truth of which we have no evidence, and which would not tend to explain the belts if it existed. The tide raised by a small satellite can only be large when the "free" period of oscillation of the gaseous or liquid ocean is nearly the same as the "forced" period. If this were the case with one of Jupiter's satellites, it certainly would not be so with the others. Although tides accompanied by *fluid friction* do tend to produce a longitudinal current adverse to the planetary rotation, yet no current of a millionth part of the velocity requisite for the production of the belts could possibly be occasioned by the tidal friction due to Jupiter's satellites.

For these reasons I quite dissent from Mr. Williams's explanation of the belts, and of the unequal solar rotation.

Sir William Thomson has recently pointed out, in a paper read before the Physical Society of Paris, a probable cause of the reinforcement of an atmospheric tide in the earth, due to an approximate agreement of free and forced periods of oscillation. He remarks that the semi-diurnal constituent of the barometric oscillation is nearly everywhere very much larger than was to be expected, and he shows that the sun and earth together constitute a thermodynamic engine whereby the earth's rotation is accelerated. Rough numerical calculations are given, wherefrom it appears that the amount of this acceleration may not be entirely negligible, when we consider the degree of refinement to which modern astronomy has arrived. G. H. DARWIN
R.M.S.S. *Medway*, Southampton, Feb. 2

The Search for Coal under London

IN a recent communication to this journal I dwelt upon the importance of a systematic search being made for the Carboniferous rocks under London, by a series of borings running from north to south, and only a few miles apart; but I pointed out at the same time that much of the expenditure required for such a search might be saved by a judicious selection of sites for the first two or three borings. I then quoted the opinions of Mr. Godwin-Austen and Prof. Prestwich as to the localities at which such explorations might be undertaken with the greatest chance of success. My friend, Prof. Prestwich, has written to me expressing general agreement with the views I have put forward on the subject, but calling my attention to some other suggestions of his as to the points at which borings might be executed, with fair hopes of success. Writing in the Reports of the Coal Commission in 1870 (p. 162), Prof. Prestwich expressed himself as follows:—

"The direction of the great underground coal trough is, we think, likely to be on a line passing through North Wilts, Oxfordshire, thence across Hertfordshire, South Essex, the north-east extremity of Kent, onwards towards Calais, near to which place it is thrown out by the rise of the underlying rocks, but resumes again at Théroutane. Or in case of the anticlinal axis taking a more southern course we should look for the coal basin or basins along a line passing from Radstock, through the Vale of Pewsey, and thence along the North Downs to Folkestone and near to Calais."

Some years later Prof. Prestwich wrote as follows:—

"In short, while there is every reason to hope that on the south of London we may yet find in the *Lower Greensand*, beneath the Tertiary Strata and Chalk, a source of large and valuable water-supply for metropolitan purposes, there is strong

¹ Monthly Notices of R.A.S. Dec. 1876. "On an Oversight in the Mécanique Céleste, and on the Internal Densities of the Planets."

² The expression "reeling" would one to suppose that a diurnal tide is referred to, in which the fluid parts are carried relatively to the nucleus in the direction of the disturbing satellite, but without change of superficial form, technically a spherical harmonic deformation of the first order. But it is well known that this class of displacement must be non-existent, and therefore it must be presumed that Mr. Williams does not intend this.

reason to believe in the probability of the discovery to the north of London of *Carboniferous Strata*, including possibly productive Coal-measures." . . . ("On the Range of the Lower Greensand and Palæozoic Rocks under London," by J. Prestwich. From *Quart. Journ. Geol. Soc.* for November, 1878. p. 911.)

The discovery of Upper Devonian strata, both at Turnford and at Tottenham Court Road, in both cases dipping at high angles, lends not a little support to the view that a trough of Carboniferous strata may exist between those two localities. Prof. Prestwich authorises me to state that what he would now recommend would be a boring "a mile or two north of Kentish Town, not directly north, but north-east or north-west, so as to avoid the hills—say about Edmonton on the one side, or near Edgware on the other." On the south side of London he would prefer to avoid the Lower Greensand, and would recommend a boring "just beyond its outcrop at Red Hill—somewhere between there and Horley." But he thinks that if Coal-measures were found to extend beneath the Lower Greensand, means might be found to sink through the latter, by the new appliances of which the Belgian engineers have so largely availed themselves. JOHN W. JUDD

Researches on Animals containing Chlorophyll

I. DR. BRANDT'S observations (*Sitz. d. Berlin Physiol. Gesellsch.* Nov. 11, 1881) are upon the green bodies of *Hydra*, *Spongilla*, a fresh-water planarian, and numerous infusors. He finds that these green bodies are masses of hyaline protoplasm, containing a nucleus and a chlorophyll-granule. Sometimes two to six are present, these he considers are states of division. He regards these facts as proving that those bodies are unicellular algae, and erects the genus *Zoochlorella*. He finds them survive isolation, and even develop starch in light. Specimens from *Spongilla* were taken in by infusors, but were either digested or ejected: those from a dead *Hydra* were, however, retained by *Paramœcium*, *Coleps*, &c. He believes that the chlorophyll never belongs to the animals, but always to algae.

My observations deal with the yellow cells of quite different animals. I have, however, ventured the opinion that in most of the above cases, the green bodies do belong to the animals, and are not algae, and I do not yet see sufficient reason for withdrawing that view.

2. For the yellow cells of Radiolarians and Coelenterates (for the alga nature of which Dr. Brandt so ably argued in his former paper) he proposes the genus *Zooxanthella*. Here Dr. Brandt has doubtless priority.

3. He observes that large Radiolarian colonies show no signs of digesting foreign bodies; that these and also *Spongilla* can be kept best in filtered water, and that the latter will not live in a half darkened room. These facts are doubtless new.

4. Dr. Brandt concludes that the algae maintain their hosts; that so long as the animals contain few or none, they feed in the ordinary way, but when sufficient algae are present, they are nourished like plants. He indicates an analogy to lichens (an hypothesis which, as I also state in my paper, was first ventured by Semper), and yet points out a distinction, since in a lichen there is an association of an alga with a true parasite, here a "Symbiose" of algae with animals accustomed to independent life, which they, however, give up, and take in no further nutriment. Thus in a morphological sense the algae, in a physiological sense the animals are the parasites.

While welcoming Dr. Brandt's interesting paper, and while not desiring to lay too much stress on such awkward facts for his view as that *Hydra*, *Anthea*, *Velilla*, &c., are quite as voracious as their congeners unprovided with chlorophyll, or that the animal may possess its chlorophyll from development, and while giving him and his predecessors all due credit for their valuable observations and theoretic insight, I must point out that (1) the *demonstration* of the truth of the view that the yellow cells of Radiolarians and Coelenterates are algae, (2) the development of the hypothesis of the lichenoid nature of the alliance between alga and animal into a theory of mutual interdependence, and (3) the transference of that view from the region of probable speculation into that of experimental science, remain with my paper. For it will not do to ignore, with Dr. Brandt, such weighty opposing evidence as (1) the recent direct statement of Hamann that the yellow cells of Coelenterates are not algae, but unicellular glands, (2) the observation of Krukenberg that *Anthea viridis* did not evolve oxygen, or (3) the failure of himself and others to prove the presence of cellulose and chlorophyll, or even to