provisionally drawn; and until some reason is given, I think the *Leda myalis* Bed may be left with the Crag. A land surface, as we know from the Purbecks and Coal-measures, does not necessarily mark a break in the series.

While agreeing with Mr. Blake that the term "Forest Bed" is a misnomer, the suggested alteration to "Rootlet Bed" seems a good deal worse. As well might we class together the London and Oxford Clays, because at the present day the roots of the same species of trees penetrate both. The rootlets of the Forest Bed penetrate whatever happens to be underneath them; sometimes the Weybourn Crag, sometimes higher beds. Even if names are not quite correct, it is better to accept them with a slightly altered meaning than to upset all our nomenclature for every fresh theory. Therefore I think the name "Cromer Forest Bed," having now been in use for over 50 years, ought not be changed, but should be accepted with the meaning that it consists of a series of sub-aerial, lacustrine, and estuarine beds formed in, and from the débris of, a forest-clad country.

Mr. Blake uses the name "Bure Valley Beds" for what was termed the *Leda myalis* Bed; but I have already shown that Messrs. Wood and Harmer's typical Bure Valley fauna comes from the Weybourn Crag beneath, instead of above, the Forest Bed,[1] while at present the *Leda myalis* Bed has not been recognized in the Bure Valley. The test of thickness is of no value in these shallow-water beds; for after they have once reached the sea-level, they may remain for an indefinite time without either erosion or deposit. In our British Pliocene beds it should be remembered we have only the feather edge of a formation, which must be much thicker where the water was sufficiently deep, and perhaps might equal the 700 or 800 feet of the Sicilian Newer Pliocenes. I am astonished at Mr. Blake's statement that the thickness of the beds between the Cromer Till and the Chalk never exceeds 30 feet; the *average* measured thickness exceeds that amount, and at Happisburgh I have reason to believe that the Forest Bed alone is more than 60 feet, for I have dredged and found it in place in 10 fathoms near the shore, and it extends upwards to high water.

CLEMENT REID.

HORNSEA, HULL, *6th June*, 1881.

OBLIQUE AND ORTHOGONAL SECTIONS.

SIR,—If Mr. Day will examine the figure given with his letter in the March Number of this MAGAZINE, he will perceive that Mr. Fisher's 'cavils' are well founded. Not only has Mr. Day interchanged the symbols α and β, but his angle ϕ has no connexion whatever with anything in Mr. Fisher's paper. Mr. Fisher might no doubt have given a simpler proof of each of his equations (2) and (3) by the method indicated by Mr. Day, but one figure would not then have sufficed for the whole proof.

Mr. Day's suggestion of casting a shadow in sunlight, in order to find the form of outcrop, is, as Mr. Fisher readily admits, useful, but he does not tell us how to carry out the inverse process, viz., given

[1] See GEOL. MAG. Dec. II. Vol. IV. p. 300; and Vol. VII. p. 548.

an outcrop, to find form of furrow; an operation to which Mr. Fisher's equations are at once applicable. The rest of his remarks appear to have been written in great haste, and are singularly inaccurate. Does he mean to say that when he has again interchanged α and β, his equation proved on p. 142 even looks like Mr. Fisher's equation (2)? Again, how can he suppose that (p. 142, l. 10) Mr. Fisher assumes the trail to lie in one plane, when it has been expressly stated to lie in a "surface which may be formed out of a folded plane." His figure, on p. 142, represents an altogether different set of angles to Mr. Fisher's. It can, however, be used to prove equation (2) if the following description be substituted for that given in the text.

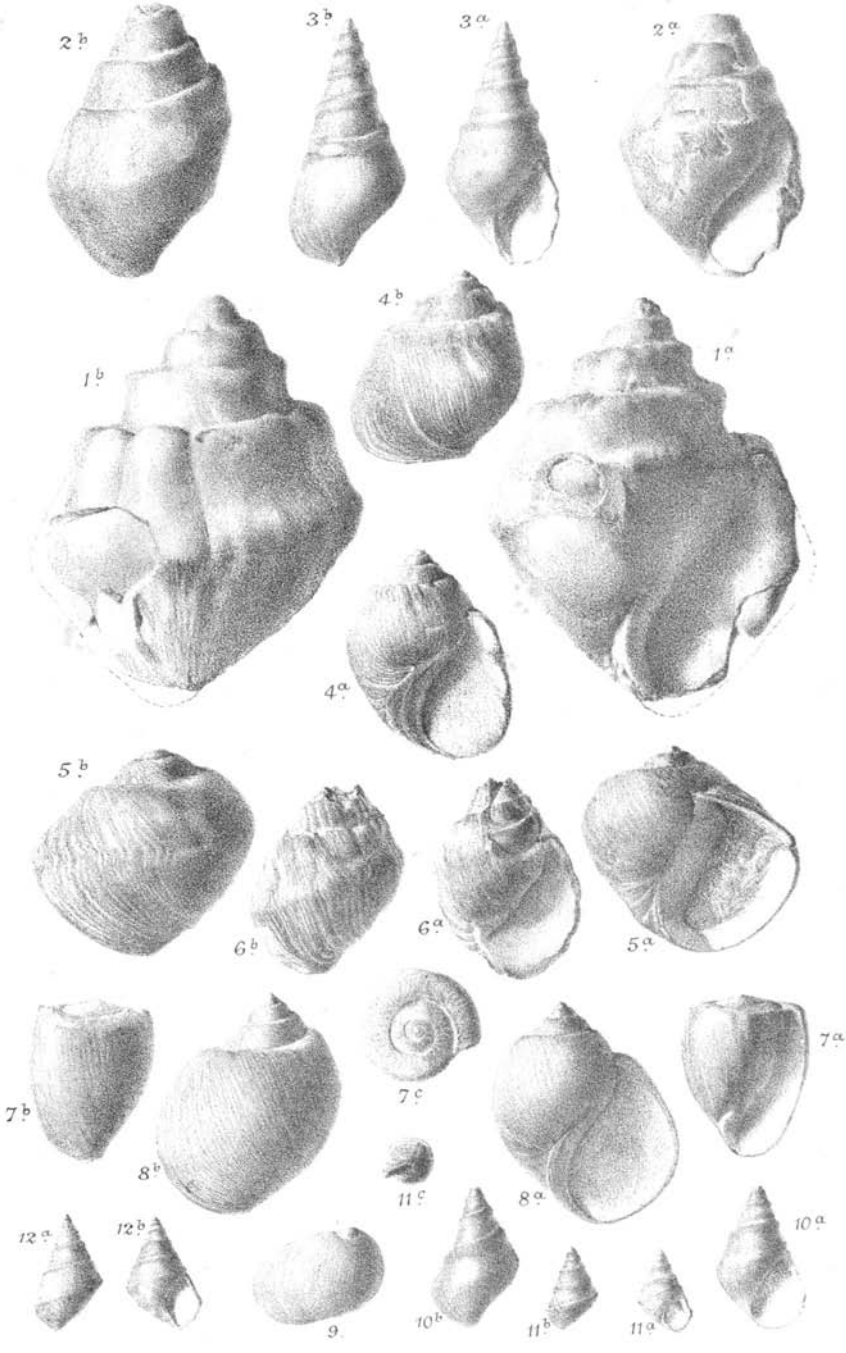
Let AB, CD be horizontal lines in the inclined plane.
AF, BE lines perpendicular to the inclined plane.
$CDEF$ a horizontal plane.
CE a line of strike, supposed horizontal.

Then $ECD = \alpha$; $BDE = \beta$; $BCD = \phi$

And $$\tan \phi = \frac{BD}{CD} = \frac{DE \cos \beta}{CD} = \cos \beta \tan \alpha.$$

Christ's College, Cambridge. A. F. Griffith.

Colliery Explosions.—A Parliamentary paper which was issued yesterday throws very important, and in one point unexpected, light on the causes of colliery explosions. After the Seaham accident last Sept. Sir William Harcourt requested Prof. F. Abel, the chemist to the War Department, to report on some samples of dust which had been collected in the workings where the explosion took place. Prof. Abel has now reported the results of his experiments, which entirely confirm those which Mr. W. Galloway has described to the Royal Society. Mr. Galloway showed that though a mixture of air and coal dust was not explosive, it became so when a very small, and apart from the coal dust, an innocuous, quantity of fire-damp was mixed in the air. Prof. Abel's experiments show that not coal dust only, but any dust, even calcined magnesia, will act in the same way. The proportion of fire-damp which is needful to bring dust of any kind into operation as an exploding agent is below the smallest amount which can be detected in the air of a mine, even by the most experienced observer, by the means at present in use. Coal dust shows a tendency to become inflamed and to propagate flame when it comes in contact with a large volume of flame, such as is made by the firing of a shot, and may thus convey the fire from a safe part of a mine to an unsafe part. These discoveries, which finally confirm a long existing opinion, impose a new duty on colliery owners and inspectors. They offer an explanation of many mysterious colliery accidents, and suggest the means of preventing them in the future. There are two sources of explosion, and we have been only guarding against one of them. Henceforth, it is not only fire-damp, but what we may call fire-dust, that must be looked after, and a great decrease of explosions will probably result.—*Daily News*, June 21, 1881.



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Gasteropoda from the Portland Rocks.

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