better preservation of lake-basins in glaciated countries from silting up and from becoming thus obliterated, while in some glaciated regions lakes are wanting.

CORRESPONDENCE.

THE BRIDLINGTON CRAG.

SIR,—There seems to be some misapprehension about the Bridlington Crag.

In the Geological Record for 1878, on page 3, there occurs the following passage: "Confirms the succession given by Mr. Lamplugh that the 'Crag' bed lies on the blue clay or basement bed, but is below the snuff-coloured laminated clay, while the purple clay is above the last."

Now Mr. Lamplugh has conclusively shown that the Bridlington Crag is not a bed at all, but a series of patches, boulders in fact, included *in* the so-called Basement Clay.

The bed or beds whence the shells were originally derived, if still existing, has never yet been seen. I say "or beds" because shells that lived at different depths have been brought together at Bridlington.

J. R. DAKYNS.

MELMERBY, PENRITH.

DR. RICKETTS.—ON SUBSIDENCE AND ACCUMULATION.

SIR,—In connexion with the subject of Mr. Jamieson's paper on the Cause of the Depression and Re-elevation of the Land during the Glacial Period (GEOL. MAG. Sept. and Oct. 1882), it may be interesting to re-direct attention to a paper by Dr. Charles Ricketts, On Subsidence as the Effect of Accumulation (GEOL. MAG. Dec. I. Vol. IX. p. 119); and to his presidential address to the Liverpool Geological Society in 1872, on Valleys, Deltas, Bays, and Estuaries. He has, in the latter paper, expressed his opinion that during the Glacial period the combined weight of ice and boulder-clay would produce subsidence of the land; and again, in speaking of deltas, he concluded that the steady accumulation of mud would in the end cause subsidence, gradual and imperceptible at first, but under certain conditions perhaps sudden.

The observations of Messrs. G. and H. Darwin, noticed in a late number of the GEOLOGICAL MAGAZINE, by Prof. Milne, show that the crust of our earth is more susceptible than we imagined when, ten years ago (GEOL. MAG. Vol. X. pp. 88 and 141), we ventured to criticize somewhat unfavourably the views then put forward by Dr. Ricketts.

H. B. W.

TERRACES IN DORSET.

SIR,—There are to be seen on the sides of the valleys in Dorset a number of terraces, which are, I believe, a peculiar geological feature of that and a neighbouring county. I have never yet seen a satisfactory theory as to their origin. It has been asserted that they are old fortifications; also that they have been formed by the plough. Whether the sea-beach or lake-beach theory has ever been

advanced I cannot say; but all, I maintain, would be found wanting by a careful and competent judge. If any of your readers can throw light on the subject, it would be interesting to those geologists who happen to have observed the peculiarity to which I refer.

CLEVELAND LODGE, LOWER SYDENHAM. S. H. WRIGHT.

The nature and origin of these Terraces is, we think, now generally very well understood by geologists.

We recommend to Mr. S. H. Wright's consideration an excellent little article upon them which appeared in the GEOLOGICAL MAGAZINE for 1866, Vol. III. p. 293, by the late G. Poulett Scrope, Esq., F.R.S., F.G.S., than whom we could hardly cite a more competent observer or more trustworthy geological guide.—EDIT. GEOL. MAG.

THE RIGIDITY (?) OF THE EARTH.

SIR,—It has given me much pleasure to read Mr. Close's remarks referring to my lament over the disagreement between mathematical physicists and geologists touching the condition of the interior of the earth. His letter gives promise that a further discussion of the question with him may serve to elucidate it.

Mr. Close has not, I think, exactly apprehended my meaning. I wrote of the conclusion arrived at by mathematicians, "that the earth is excessively rigid from its centre to its surface." Mr. Close, on the other hand, writes of the disagreement between them and geologists respecting "the rigidity of the body of the earth." It is important to be precise as to what we are discussing. As a physical geologist I seek to explain the phenomena exhibited by the masses which constitute the *surface;* its continents, mountains, plains, valleys, oceans, and volcanos. Still, these phenomena require us to speculate upon the condition of the interior down to a considerable depth; yet not necessarily to a depth which bears any large proportion to the entire radius. In short, I am willing to relegate the "body of the earth" to the physicist pure and simple, as a region beyond my province, and respectfully to accept his conclusion that it is extremely rigid. Possibly this rigidity may be no more than that *viscous* rigidity which Mr. Close so accurately describes, showing in his letter how such a condition of the interior would be capable of explaining many of the facts relied upon to establish rigidity. It certainly also appears to suit, better than absolute rigidity, with one to which he has not alluded; namely, that the *present* ellipticity of the earth agrees so well with the present period of diurnal rotation.

I will now state some objections, which, on geological grounds, I would offer against the contention of Mr. Close, that a general viscous rigidity, such as I understand him to advocate, would meet the requirements of the problem; and I will point out one instance of the neglect of geological phenomena by a mathematician. I maintain that the surface phenomena require that the cooled crust of the earth should be far more rigid than what it rests upon. For instance, they require that the substratum should be sufficiently fluid to admit of the crust being shifted over it towards the mountain ranges; that it should likewise be in a condition to flow upwards into narrow chasms, and form igneous dykes, and to furnish the