As in other areas in the Lake District, the usual evidences of glaciation cease about the 2,000 ft. contour, probably on account of frost action during the latter part of the Glacial Period and since the disappearance of the ice. During the period after maximum glaciation, but before the ice was definitely confined to the present valleys, its motion appears to have ignored the present drainage system completely and evidences of cross-channels cut by these high-level glaciers are very common on the mountain-tops.

One episode in the later Glacial Period was the formation of a lake in the Duddon Estuary, which, for some time, discharged its water across South Furness into Morecambe Bay. Low Furness is an area of deposition and is largely covered with Drift. This is of two kinds :—

1. The Irish Sea Drift, divisible into three horizons : (*a*) The Upper Boulder Clay ; (*b*) the Middle Sands and Gravels ; (*c*) the Lower Boulder Clay. These contain erratics from many places on the west coast of Cumberland and probably still further north. It has been fully described by Mackintosh.

2. The Local Drift, a heterogeneous series of deposits, mostly of limited extent, and probably with no simple relation to one another ; certainly not divisible into three series, as has been claimed by previous writers. The line of demarcation between these two drifts runs across Low Furness from Dunnerholme to Newbiggin. North of this line no erratics have been found which can be ascribed to the Irish Sea Drift.

CORRESPONDENCE.

GAULT AND LOWER GREENSAND NEAR LEIGHTON BUZZARD.

Sir,—We have read Mr. G. W. Lamplugh's paper, "On the Junction of Gault and Lower Greensand near Leighton Buzzard (Bedfordshire)," which has recently appeared in the Geological Society's *Quarterly Journal* (vol. lxxviii, part i). Mr. Lamplugh's main interpretation, violating, as it does, the most elementary principles of zonal palæontology and entailing the correlation with one another of four distinct deposits of widely separate ages, appears to us to be so fantastic as to be unworthy of serious consideration. The fallacy of this attempted correlation must be patent to all who are acquainted with the faunal characterization of the zones in question. Rock-specimens and fossils from these beds may be seen at the office of the Geological Survey by any geologists who are interested. The specimens provide clear evidence that Lower Gault and Upper Gault have been confused by Mr. Lamplugh, and no amount of special pleading will lessen the force of their testimony. As regards the limestone-lenticles found at Shenley Hill, amply shown by their fauna to be of Cenomanian age, the matter is equally simple to those who are competent to make a proper use of the evidence.

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The publication of work so fundamentally unsound is the affair of the author and of the Geological Society. Of more concern to us is the misleading character of many of Mr. Lamplugh's numerous references to us and to our published views. Misrepresentation is so frequent throughout the paper that we feel bound to protest. A full analysis would occupy many pages of print; but we may mention briefly a few examples by way of illustration. On p. 13 of his paper, referring to a particular mass of "greensand" below the Gault seen by him in 1904, Mr. Lamplugh suggests that we expressed our opinion that this bed is of Upper Greensand age without having seen the rock. He omits to say that our view was based on a set of specimens obtained by himself and deposited in the collection of the Geological Survey. These specimens are always available for examination, and anybody who understands the subject will find that they speak for themselves. Mr. Lamplugh curtly dismisses the considered opinion of Mr. T. H. Withers, on some cirripede-remains from this bed. Mr. Withers is a leading authority on fossil cirripedes and their evolutionary characters, concerning which Mr. Lamplugh is not qualified to speak. We have complete faith in the opinion of Mr. Withers on this subject.

Again, on p. 15, when referring to a certain mass of clay ascribed by us to the Upper Gault, Mr. Lamplugh's words, "and therefore including this section in the area supposed to be inverted", wrongly introduce an implication that we have never adopted. Once again (p. 8): "Dr. Kitchin and Mr. Pringle record an ammonite of the auritus-group, supposed to be *Hoplites catillus* (J. de C. Sowerby) . . ." (See also p. 78.) We neither ascribed any ammonite seen by us to that species nor referred the species to the *auritus*-group. And, as a further example, on p. 18 Mr. Lamplugh states that we have described and figured "the tailing out southwards" of the Silty beds at the entrance to the Miletree Farm pit. What we actually described and figured was a striking unconformity between Upper Gault and Lower Greensand at the opposite (northward) end of the pit. Although this is the most important feature of the section, it is left unmentioned by Mr. Lamplugh.

Such a mode of representing our views must tend to discredit us unfairly in the eyes of uninformed readers. The same misleading method is too noticeable also in Mr. Lamplugh's presentation of geological facts. For example, on p. 41 he refers to fossils of the Shenley Hill limestone found many years ago by the Geological Survey at Long Crendon, and leads the reader to believe that these came from "calcareous stone" found there below the Gault. In reality there is no particle of evidence for that belief. In the original manuscript record of the section from which the specimens were obtained the presence of Gault is not mentioned. The Gault of that locality is underlain by a Purbeck limestone; but the fossils were stated to have been found just above Portland limestone, which was then worked at its outcrop in openings immediately adjacent to a

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Gault clay-pit. It seems clear that Jukes-Browne regarded these specimens with suspicion. Although he was provided with a list of these fossils, which were collected for him, he did not mention them in connexion with his section published in 1900 (reproduced by Mr. Lamplugh). He probably suppressed mention of them, for he certainly knew better than to assign them to a horizon below the Gault Clay. In any case, the lowest Gault of Long Crendon has been shown to be Upper Gault, with an unconformable base, and the *mammillatus*-horizon, to which Mr. Lamplugh wrongly ascribes the limestone, is unrepresented there. We have no doubt that the fossils, all of which may have occurred in a single piece of limestone of small dimensions, came from the surface. Fragments of "red chalk" and other extraneous material have been recorded from the Drift of Oxfordshire. There is Drift on Long Crendon hill: the matrix of these fossils is not unlike "red chalk".

On p. 28 of his paper, when comparing the section in the old pit near Heath House with that in Harris's pit at Shenley Hill, Mr. Lamplugh says that the Gault in these two exposures agrees in all essential particulars, a statement quite contrary to fact. The lowest 15 feet of Gault in these two sections is so well contrasted, both lithologically and palæontologically, as to preclude any idea of correlation. This contrast is fatal to Mr. Lamplugh's view. He also states as a fact that Lower Gault fossils were formerly obtained from the Heath House section (p. 79), though there is no evidence of this. Jukes-Browne recorded *Ammonites interruptus* from a nodule-bed there which contains a rich Upper Gault fauna; but certain hoplitids of the Upper Gault were at that time usually determined as "*A. interruptus*", and the record is without any value. To accept it as evidence of the presence of Lower Gault is merely absurd, in view of the fossils ("*Ammonites rostratus*," "*A. varicosus*," "*Inoceramus sulcatus*" and others) with which the species was stated to be associated.

On p. 20, Mr. Lamplugh's diagram illustrating the section at the northern end of Miletree Farm pit is so drawn as not to show the important unconformity that occurs there. The reader must inevitably obtain a false impression as to the relation between the Gault and the Lower Greensand at that locality. The case is poor indeed that depends on such methods of advocacy as are illustrated by this and the other examples mentioned above.

In his discussions of the palæontology Mr. Lamplugh shows little understanding of essentials. Names seem to appeal to him more than the facts and principles of evolution, about which he appears to know nothing. His method is to labour the points that he believes (not always correctly) to be favourable to his view and to pass adroitly over those that are unfavourable. Speaking of the *Inocerami* found in the lower part of the Gault at Harris's pit (p. 78), believed by him to be Lower Gault, Mr. Lamplugh states that one of these is a form common in the "lower part of the Gault" elsewhere,

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specifying Muzzle (West Norfolk) and Speeton (p. 51). He is evidently unaware that he is here referring to Upper Gault deposits. The *Inocerami* in question are found at those localities in overlapping Upper Gault, and are not known to occur at any place near the true base of the Gault.

The age of the Gault in Harris's pit can only be determined by means of its fossils; yet the example just cited shows that Mr. Lamplugh is incapable of interpreting the evidence by which alone a true correlation can be made. This statement applies also to his discussion of the ammonites from the lower part of the clay in that pit (pp. 78–9). His remarks show such superficiality and confusion of ideas that they are ridiculous as well as valueless. No informed palæontologist having any regard either for his own reputation or for sound stratigraphy could be persuaded to assign the lower part of the clay at Harris's pit to the Lower Gault, or even to a horizon as low as the basal part of the Upper Gault. A stratigrapher, however, ascribes it without scruple to the Lower Gault, with the benison of the Geological Society of London to support him (1903, 1922).

Mr. Lamplugh's method of dealing with the fauna of the Cenomanian limestone is equally futile. Why trouble to say so much about *Terebrirostra neocomiensis* and *T. arduennensis* when the forms found in the limestone at Shenley Hill are identical with the later, extreme evolutionary types characteristic of a low Cenomanian horizon in France and in this country? Why pass so lightly over *Catopygus columbarius*, *Nucleolites lacunosus*, *Cidaris bowerbanki*, *Rhynchonella grasiana*, *Pecten curvatus*, *Isoarca obesa*, *Cyphonotus incertus*, and other telling species? If, as Mr. Lamplugh believes, *Leymeriella regularis* occurs indigenously in the same bed as these, then the whole basis of zonal palæontology, as practised daily by professionals like ourselves and by innumerable amateurs the world over, is false. But practical results prove that the contrary is the case. The firm foundation upon which accurate stratigraphy has been built is not yet shaken; the method of William Smith is not to be discredited by the assumptions of Mr. Lamplugh.

Mr. Lamplugh also speaks of *Inoceramus concentricus* as coming from the limestone. Whether the limestone be ascribed to the "*mammillatus*-zone" or to a Cenomanian horizon, it is in either case safe to assert that this species does *not* occur in the bed; it may probably have been collected from the basal bed of the Upper Gault, with which the limestone-lenticles are found accidentally associated. Mr. Lamplugh states that the absence of certain species speaks against our view concerning the age of the limestone, apparently unaware that some of these (for instance, *Pecten asper*) are not found at the horizon in Wiltshire with which we correlate that rock.

Mr. Lamplugh's failure to determine correctly the age of the limestone and the clay that overlies it invalidates the whole of his paper. Judging by the above examples of palæontological ineptitude

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and the many other errors and fallacies in the paper (we can furnish a long list, if desired), it can only be assumed that before undertaking publication the Council of the Geological Society failed to submit the manuscript to any capable palæontologist familiar with the zones in question.

In the course of the paper we are mentioned by name more than forty times, not always without derogatory implication in the context. It is most unusual to find the *Quarterly Journal* utilized as the medium of publication for a paper of this character, in which the collective effect of the repeated personal references and the misrepresentations is sometimes that of a not well-veiled *argumentum ad hominem*. We have always understood that one of the chief functions of the Publication Committee of the Geological Society is to prevent the publication of matter which may give just cause of offence, and to provide some guarantee to the Fellows that the substance of communications issued by the Society shall be sound in essentials. Apart from any personal considerations, we, as Fellows, regret that the Council has created so questionable a precedent by the publication of Mr. Lamplugh's elaborate and costly paper in the form in which it has appeared.

F. L. Kitchin.
J. Pringle.

THE AGE OF THE SHENLEY LIMESTONE.

Sir,—In February of last year I published a short report on the Echinoids of the now notorious Shenley Limestone lenticles; and in the following month Mr. Lamplugh asked that "judgment in respect of [my] deductions" should be suspended. His "judgment" has now been pronounced and published (*Q.J.G.S.*, lxxviii, pp. 76–7), and I beg leave to exercise the prerogative of comment before sentence is passed.

I wish to say at the outset that I have neither desire nor intention to be drawn into a controversy on matters beyond my own observation—the stratigraphical relations of the Shenley limestone are none of my business—and that I am concerned solely with the facies presented by the Echinoid fauna. It is true that most of the specimens are too poorly preserved for rigorous determination; but it is equally true that a few of them are as satisfactory as could be desired. Is it a coincidence that every one of such specimens indicates an horizon at or above the top of the "Upper Greensand?"

Cidaris bowerbanki was recognized on three (probably six) radioles, and Mr. Lamplugh suggests that "the determination can hardly be reckoned conclusive". With all deference I would submit that in this case the radioles are vastly more distinctive than the test. At least, they belong to a *Tylocidaris* (type *C. clavigera*), and that genus is not known before the Cenomanian. Arguments based on its generic range (after the style adopted by Mr. Lamplugh in his criticism) would make it reasonable to assume that the Shenley

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