Actinoceras sulcatulum and *Solenocheilus latiseptatus* are common in the Cement-stone (Upper division of the Carboniferous Limestone) of the West of Scotland. I have myself collected them in the Arden Quarry, at Nitshill, near Glasgow (see Cat. Foss. Ceph. Brit. Mus. (Nat. Hist.), pt. i. 1888, Suppl. p. 319).

The present species has been well described, but poorly figured by Armstrong (Trans. Geol. Soc. Glasgow, vol. ii. pt. i. p. 74, pl. i. figs. 6-7) under the name of *Nautilus (Discites) nodiferus*. Its identity with Römer's species is, however, beyond question.

The following is Armstrong's description:—"Shell discoidal, composed of about three gradually enlarging, contiguous, nearly subquadrate whorls, completely exposed in a moderately shallow umbilicus; back broad, rounded at the edges, and traversed in the middle by a wide and deep channel, on the sloping sides of which are two fine thread-like ridges. The remainder of the space on the back and sides of the shell is occupied by six rows of closely set, prominent, obtuse tubercles, elongated in the direction of the aperture, one row of tubercles of large size bounding the back, a double row of equal size between it and the channel, and three on the sides, which decrease in size towards the umbilicus. Surface marked with coarse, squamose, wavy lines of growth, which are arched backwards in the dorsal [=ventral, or peripheral] channel. Septa numerous, their edges arched considerably backwards on the periphery, and slightly so at the sides. Siphuncle $\frac{1}{2}$ inch diameter, central. Dimensions—width of greatest diameter $3\frac{1}{4}$ inches, width of outer whorl near the aperture, 1 inch."

The specimen presented by Mr. Seward is a valuable accession to the British Museum collection, which hitherto possessed only some small fragments of this beautiful species. These are described in part ii. of the Catalogue of Fossil Cephalopoda (1891), p. 139, where also Armstrong's description of the species is quoted.

II.—ON SOME RECENT CONTRIBUTIONS TO PRECAMBRIAN GEOLOGY.

By J. F. BLAKE, M.A., F.G.S.

THE last two numbers of the Quarterly Journal of the Geological Society contain three communications concerning rocks which are, or are thought to be, Precambrian. Two of them are in direct attack on work of mine, and the third incidentally opposes some portion of it. The materials of any reply I may have to make are, for the most part, already published in my papers, but the bearing of these facts seems to require to be pointed out.

Taking the communications in order, the first is the Anniversary Address of the President. In this he deals, so far as my work is concerned, first with Anglesey. Sir A. Geikie states that he was not prepared by previous writings for certain results he obtained. He was much astonished to find in central Anglesey so striking a counterpart to portions of the old gneiss of the north-west of Sutherland and Ross. Speaking of the rocks which he here refers to, I wrote, "The question is suggested whether we may not touch here a

piece of genuine Archæan.”¹ This question Sir A. Geikie answers offhand—by the guidance of petrographical characters—aided by the hummocky surface of the ground, which latter, however, is not confined to Archæan gneisses. These petrographical characters I have described,² and state that the rocks have “much the aspect of a Highland gneiss”; but I do not think these characters may safely “be taken as a guide” to age. There is certainly at this moment a controversy whether they can be or not.

Sir Archibald then speaks as if *no* order of succession has been made out in the Anglesey rocks, and as if their metamorphism was the only argument for their Precambrian age in the strictest sense. I would point out that near Beaumaris the Lowest Cambrian has been shown to overlie them.

But the most important matter is the decision of the age of the rocks of the Northern District which the Director-General now considers to be Silurian. He states in the first place that “the necessity for inserting” the bounding “fault apart from any actual visible trace of its occurrence arose when the conclusion was arrived at that the rocks at the extreme north of Anglesey were essentially altered Cambrian rocks.” That is to say, it is a theoretical fault. Dr. Callaway has given evidence for this fault, and my own statement is: “In this Northern District a very definite sequence of rocks may be demonstrated, We can follow the strike with considerable accuracy. Now, whatever part of the series we may be on from the lowest to the highest, and therefore whatever class of rock is on the northern side of the fault, as soon as we overstep that boundary we are immediately landed in black shales, which have a pretty uniform character throughout. This leaves no alternative but a fault or an unconformity; if it were an unconformity, the upper group would run parallel to the boundary, which it does not.” Whoever denies this fault, let him answer this argument, and also explain how masses of Cemmaes Limestone come to be found in the basal Silurian conglomerates.

In fact, however, this line is not in modern language a fault, but a “thrust.” The Director adds: “Where the supposed elliptical fault reaches the shore at Carmel’s Point, the two groups of rock seem to me to follow each other in unbroken sequence.” So they do to me, but things are not always what they seem. At Craig-an-Knockan, Ross-shire, and elsewhere, there is an *apparent* “conformable upward succession” between Silurian rocks and the overlying gneisses. The little bands of black slate caught up in the motion in Anglesey are just what we might expect in a similar case.

Dealing next with the nature of the upper part of the rocks, some of which have yielded fossils, he states that they include “courses of black shale containing Lower Silurian Graptolites”; also “it has been supposed that the higher bands of black shale may also have been brought into their present position by faults” . . . “but this suggestion is completely disproved by the coast sections, which exhibit many thin leaves of black shale, sometimes less than an inch thick.”

¹ Q.J.G.S. vol. xlv. p. 497.

² Rep. Brit. Assoc. for 1888, p. 32.

I am not aware that there are any *courses* (in beds parallel to the rest) of black shale; the faults which let down the masses which are said to have yielded Graptolites may be seen on both sides. If there are thinner black shale bands which I have not observed, that is, no doubt, a point to be considered; but it could only be decisive if they contained Graptolites of known Silurian species.

The Director sums up by saying that this area "is proved by the evidence of fossils at its base, towards its centre, and at its top to belong to the Lower Silurian series." This sounds like a settled matter. But may I ask (or rather can I get an answer to the questions?) *What* is the lowest bed of the series above the Black Shales? What are the *names* of the supposed Silurian fossils it contains? and where may they be seen? All Sir A. Ramsay says is, "On the north by the shore a few poor fossils in irregular bands of limestone clearly indicate the Caradoc or Bala age of some of the beds." What are the names of the Graptolites which occur towards the centre? and what is the proof that *Orthis Bayleana* is a Bala fossil?

The arguments generally for the Monian age of these northern rocks are these: The volcanic group is more or less repeated in various parts of the island. It must be admitted, however, that those that most resemble these northern rocks do not clearly show their stratigraphical relations. The quartz knobs and limestone masses which are found amongst them also characterize parts which are admittedly Precambrian. It may be said against this that these may be of several ages; but it is strange that they occur only in Anglesey and amongst these rocks. Bala rocks are known in the island as black shales proved to be Bala by their *named* fossils (see Mem. Geol. Survey, vol. iii. p. 225), including *Orthides*; but none of these are *O. Bayleana*, and it would be strange that the volcanic rocks of different ages should be so similar north and south, and rocks of the same age so different in the centre. If all these arguments could be met and the volcanic group were accepted as Bala, the next alternative would be that of Sir A. Ramsay, to consider part Silurian and part Cambrian, the line of separation being obliterated by the squeezing. Otherwise, if all were Bala, we should have *identical* chloritic schists at Llanflewlin and Abersant, the former Bala, the latter overlaid unconformably by Arenig grit, to say nothing of *similar* rock overlaid by the same at Carmel's Point and Treiorwerth.

I have next to notice the reasons given for rejecting the term "Monian" for a series of rocks in Scotland, of which it can be written, "No one familiar with the Dalradian rocks of Scotland and Ireland can fail to be struck with the close resemblance which these younger Anglesey schists bear to them, down even to the minutest details," and "if we are justified in grouping these Anglesey rocks with the Dalradian schists." I will not say that, if by "Dalradian" is meant the whole of the seventeen groups enumerated, these reasons *as a whole* are not well founded, since in that case it may include more than one system, of which, to judge from the statements made

and if petrography can be shown in this case to be a safe guide, the Monian is certainly one. But on the next page (75) it is stated that Dalradian is proposed for the "crystalline schists" alone. In a note he says that the Monian includes Archæan gneiss. This it does not do, for if the rocks which I suggested *might* be Archæan gneiss were proved to be so, that fact would exclude them from the Monian; and if they are not Archæan gneiss they are foliated granite, and as such form no part of the system. Also that it includes "strata, volcanic and fossiliferous, of undoubtedly Bala age." But even if this could be proved, these strata could be eliminated from the Monian without destroying its existence or making it much smaller.

I now pass to the next paper by Dr. Callaway "On the Unconformities between the Rock Systems underlying the Cambrian Quartzite in Shropshire."

The first question dealt with is the age of the "Volcanic series" or Uriconian. This I concluded to be younger than the Longmynd shales. The evidence I brought forward for this was entirely original, for Dr. Callaway *never discussed the question at all*, to my knowledge, and even now he does not bring forward a shred of *positive* evidence that it is older. He says they are faulted together, in which case either might be the younger. The fact is he proved the Uriconian to be older than the Cambrian Quartzite, and *assuming* the Longmynd shales to be Cambrian also, that carried the point. Even now, I can scarcely regard its younger age as definitely proved; it may be that they are more or less contemporaneous in spite of any difference of strike. All I can say is that all the evidence available is most in favour of the volcanic series being younger; and actually Dr. Callaway's own statements are *primâ facie* to the same effect. Thus he refers to a locality on the S. E. of Ragleth (which after careful searching I failed to find) where shale like the Longmynd shale is *overlaid* by red felsite. Again, he states that near Hazler cottages the Longmynd slate near its junction with the Uriconian "has a burnt appearance and the cracks are injected with red felspar."

He next refers to the Pontesford Hill area, and says that the rocks next the rhyolite are not altered. Assuming that they are not, though to my mind they certainly are, the section is still proved by stratigraphy to lie above the whole of the Longmynd series. It is difficult to understand the gist of the concluding sentence: "As this crucial case breaks down on examination, I thought it needless to re-examine the other masses of Uriconian which appear on the line of the great Pontesford Linley fault." If it means that because in his opinion I am in error about the alteration (in common with Sir R. I. Murchison, who seems to have examined this district more thoroughly than any one else), therefore I am unworthy of credence, his paper might have conveniently stopped there.

Turning next to the unconformity in the middle of the Longmynd, he selects for sole examination the section at Narnells rock. I give the junction as it is actually seen, and this is what I say about it.

"The grit is here seen to be unconformable, but the evidence is scarcely satisfactory or conclusive. If it had been conformable, the motion which is indicated by the slickensides might easily have produced the amount of unconformity apparent." Dr. Callaway says just the same, "The grit appears to have been a little squeezed into the slate, so that here and there a trivial unconformity is apparent." According to his section, which, by the way, is not what is seen at any one spot, but is made up from several exposures, all the rocks have the same dip, so there is an apparent conformity. It is not easy to see how an apparent conformity combined with an apparent unconformity can "absolutely disprove" the reality of either one or the other. My deduction is that the evidence here is not conclusive. But other evidence is, or at least is put forward as such, but it is not tackled by the critic.

With regard to the conglomerates and grits found on the "Volcanic Hills" being superficial or otherwise, it will be well to state what I regard as evidence of a rock being superficial. When the boundary of a rock is of an irregular kind, like the outline of a map, and is surrounded on all sides by other rocks which have a more definite strike, particularly if its boundary is nearly horizontal and follows approximately the contours of the present valleys, I take it to be superficial. In proportion, of course, as these features are wanting, the evidence is less satisfactory. They are all fulfilled by the grit on Caidington Hill. I am not at all sure that Dr. Callaway has seen this patch, for the rock which composes it is *not* like the Woodgate quarry rock. As to Charlton Hill, Dr. Callaway gives a section, but this section is not seen, but inferred. The map I give is what is actually *seen*, and this proves the inference to be wrong. I am sorry to say that this map, on p. 410, is so small that it requires a lens to examine it. Small circles on it indicate exposures, and the boundary lines are drawn to include exposures of similar rock. Let Dr. Callaway show that this is wrong. It is drawn carefully to scale on the One-inch Ordnance Map, and there cannot be the slightest difficulty in knowing the exact spots intended. The same remark will apply to the two patches of igneous rock "near the south-eastern end of the Wrekin," as to which he says he is in the dark; a lens will show him *exactly* where they are on the One-inch Map.

On the crystalline rock of the Ercal, it will be sufficient to compare the two following passages: "Prof. Bonney said the rhyolite was clearly intrusive in the granitoid series" (Q.J.G.S. 1879, p. 669). "He" (Prof. Bonney) "cannot find any distinct proof of the intrusion of the felsite in the granite" (Q.J.G.S. 1891, p. 118).

Finally, with regard to the difference in strike of the rocks of the Volcanic Hills and the Longmynd rocks, it does not appear to me to be of any great consequence, and in any case does not prove the former to be older. Many of the rocks in the former have no strike at all, and the strikes noticed are very variable. Volcanic rocks, even when clastic, are apt originally to have a quâquâversal dip, and more than all, the rocks on the east side of the great fault have had to bear the brunt of all the earth-movements, and they have yielded to them in no simple way.

The third paper I wish to speak of is by Miss Raisin, "On the Lower Limit of the Cambrian Series in N.W. Carnarvonshire." This I have only here to refer to as it deals with supposed *Pre-cambrian* rocks. One general observation is necessary. The authoress continually speaks of my conclusions as "new." They are in fact essentially the conclusions of Sir A. Ramsay, and the only novelty about them is the reconciliation of these with *some portion* of the more recent conclusions of other writers.

The proofs which I gave of the general correctness of Sir A. Ramsay's conclusion Miss Raisin has sought to invalidate, and in one case I have now to acknowledge she has done so with success. On re-visiting Bryn-Efail I find that her reading of the section is the true one, and my own is erroneous. I would make this acknowledgment in any case, but I do it now with all the more readiness, that I find that the mistake is an entirely gratuitous one. If my reading had been correct, it would have constituted a difficulty, rather than an aid in the true reading of the district.

The fact is, that when I wrote my paper I was studying Pre-cambrian rocks, and as soon as I found that there were none in the district—for the proof of which the other observations are amply sufficient, and had led me to this conclusion before ever I saw Bryn-Efail—I grudged any more time for the subject, and was glad of a short cut, which has proved to be disastrous. I saw that to understand the real history of these volcanic masses involved a survey of the whole of the Cambrian rocks of the district—a task from which I shrank. That survey I have, however, now made, and hope shortly to present the results. By this I have learnt, that a great number of previous supposed results, some of my own among them, are entirely wide of the mark. The only thing that becomes clearer than ever is that there are no stratified Precambrian rocks in the ordinary sense in North-West Carnarvonshire. These results will render it useless to reply to any other points in Miss Raisin's paper.

III.—A MINIATURE ILLUSTRATION OF NORMAL FAULTING.

By T. MELLARD READE, C.E., F.G.S.

WHEN examining the Glacial drift at Nevin, Carnarvonshire, I observed in a banded silty clay occupying the top of the cliff of drift at a point between Porth Nevin and Porth Bodeillas, an excellent illustration of my theory of Normal Faulting.¹

The bed in question consisted of an extremely finely laminated silty clay, sometimes called book-leaf clay, the laminations being grouped in ribands about half an inch wide and six inches apart, of lighter and darker colours, presenting a striped appearance very conspicuous from the shore. In the upper part of the bed the bands were continuous, though a little wavy: the lower part of the bed, as far as it was visible above the talus of sand, was faulted as shown in the accompanying diagram (Fig. 1). These faults simulated in a remarkable manner sections of faulted coal-fields which

¹ Chap. viii. Origin of Mountain Ranges.