common, though *B. ultimus* occurs sometimes; it is true that all three are often regarded as varieties of one species, but that makes no difference to the fact of the two varieties being specially characteristic of the Gault.

Ostrea acutirostris has not, I believe, previously been found below the Chalk Marl, but it is not a common shell anywhere, and is believed to occur in Upper as well as in Lower Chalk, so that it is evidently a species which has a long range.

The statement that *Ostrea vesicularis* has not been discovered below the Chalk is quite erroneous, for it is a common shell in the Upper Gault of Folkestone and elsewhere.

(3) Lastly, the clay above the "coprolite bed" at West Dereham has the lithological characters and general appearance of Gault. It is by no means unusual for the lower part of the Gault to contain fossils in the form of phosphate casts, more or less rolled and worn; such phosphatized fossils are particularly abundant within twenty feet of the base of the Gault in Buckinghamshire, and in the same county the passage-beds between the Gault and Lower Greensand often contain large lumps of sandy phosphate, which resemble those of the junction-bed at West Dereham. There is nothing therefore in the character or contents of the West Dereham clay that is incompatible with its being true Gault. For the reasons above stated we must maintain that all the previous authors were right in accepting the sections near West Dereham as proving the existence of Gault in that part of West Norfolk. How much farther north the Gault extends is another question which we are endeavouring to ascertain, and such evidence as we can obtain will be embodied in a paper which we intend to bring before the Geological Society. We shall also endeavour to account for the apparent passage between the Gault and the Chalk Marl; but we may remark that at Shouldham we found what appears to be the true glauconitic base of the latter, above the clay which Mr. Reid describes as containing *Belemnites* and *Plicatula*.

[*Postscript*.—Since the above was written we have had a boring made in the quarries at Stoke Ferry to a depth of fifty-five feet, and have proved the existence of a bed of Chloritic Marl at the base of the Chalk Marl, and resting on blue clay; the total thickness of the Chalk Marl there is seventy-three feet, and it must be remembered that below this there is at least sixty feet of Gault.]

VI.—NOTE ON THE FOLIATION OF THE LIZARD GABBRO.

By Colonel C. A. McMAHON, F.G.S.

THE appearance of Professor Bonney's letter in the December Number of the GEOLOGICAL MAGAZINE, in which he suggests that Mr. J. J. Harris Teall's proposed explanation of the cause of the foliation of the Lizard gabbro "ought to be regarded as an hypothesis on its probation, and should not be quoted as an undoubted fact," emboldens me to offer some remarks on the subject.

Mr. Teall suggests that the "foliation in the Lizard gabbro is the

result of pressure" acting "after the consolidation of the rock;" or, as he puts it shortly in another place, the foliation is "a secondary structure due to earth-movements acting upon the *solid* rock." [The italics are mine.]

Mr. Teall does not expressly state, in his interesting and valuable paper, how the pressure acts so as to produce the results described; but we are told that at Pen Voose "massive gabbro passes over into gabbro schist at a fault plane," and "the foliation in the gabbro is such as would be produced by a shearing motion parallel with the fault plane."

Are we to understand that foliation has been produced by mere mechanical crushing, and consists of the rearrangement of minerals already formed sliding over each other's backs—a sort of mineralogical leap-frog—in consequence of a "shearing motion parallel with the fault plane;" or are we to understand that the "shearing motion" was transmuted into heat, and the heat so produced resulted in the fusion and subsequent recrystallization of the component minerals?

On either supposition I shall be glad to know how the foliation of the "veins and dykes of gabbro" to the north of Coverack, and the "small dykes of gabbro in the serpentine near Karaklews," is to be accounted for? Do all these veins and dykes concur with fault planes, or do they ramify in all directions in the usual manner of intrusive veins? If they coincide with fault veins, how is it that they are not always foliated? Mr. Teall states that the Coverack veins, and the small dykes near Karaklews, are "often foliated" (which implies that sometimes this is not the case); and further, that "not seldom one may see the transition from massive gabbro to gabbro schist taking place in the space of a few inches." If the veins and dykes in question coincide with fault planes, this occasional failure of "shearing motion" to produce foliation seems to need explanation. On the other hand, if some of the foliated veins and dykes do not coincide with fault planes, I fail to see how the "shearing" hypothesis can account for the foliation.

Again, if the foliation is accounted for on the supposition that the shearing motion developed heat, and recrystallization took place on the subsequent cooling of a rock in which molecular motion had been set up by the heat so developed, I do not at present understand how masses of gabbro "more than a hundred yards in width" (p. 487) have become, "with a few local exceptions, foliated throughout" their "entire mass." If the heat produced was sufficient to fuse the rock, why was its structure, on reconsolidation, not granitic, as it presumably was on its first consolidation, and as the dykes, sheets, and veins of ordinary eruptive rocks are?

Most geologists with field experience will doubtless be able to point to numerous instances of such eruptive dykes, and sheets, that have consolidated under enormous pressure, but which, notwithstanding, do not show a trace of foliation.

The above, I think, are difficulties which need clear explanation before the hypothesis can be accepted that pressure acting on a *solid* rock is sufficient to account for the foliation of the Lizard gabbro.

In the mean while, I venture to offer another explanation for consideration.

After having studied what used to be called the granitoid-gneiss of the Himalayas for some time, I came to the conclusion, from the stratigraphical, petrological, and microscopic facts observed, that what is now called the gneissose-granite of the Himalayas was erupted through lines of fracture in a *partially consolidated* condition.

I do not wish to apply that explanation to all cases, but I suggest a modification of it to account for the foliation of the Lizard gabbro; namely, that the latter was produced by "shearing motion" on the rock after its consolidation had advanced considerably but was still incomplete. This, it seems to me, would account for the observed facts better than any hypothesis yet proposed for acceptance; it would account for the foliation being generally marked at the contact of the gabbro with other rocks along joint planes; it would account for any signs of crushing in the gabbro that the microscope may reveal; and indeed for all the observed facts so far as I am acquainted with them. The great depth to which the foliation sometimes extends might, on this hypothesis, be accounted for either by supposing that the "shearing motion" continued for a long time; or, better still, that it was repeated frequently at intervals of sufficient duration to allow the marginal portions of the gabbro to cool and consolidate. Very slight movements parallel with the fault-plane would be sufficient, on the latter supposition, to produce foliation to a great depth.

If the foliated veins and small dykes, alluded to by Mr. Teall, do not run with fault planes, the foliation might be explained on the supposition that they were, like the gneissose-granite of the Himalayas, intruded in a viscid, or semi-plastic condition, when the shearing of the main mass of the gabbro along the fault planes was going on. Traction action on an imperfectly consolidated mass would in this case account for the foliation observed.

The fact that in some of the dykes a rapid passage from a foliated to a granitic structure is observed, is of course a difficulty in the way of any hypothesis; but I suggest that on my hypothesis it is capable of explanation. In the first place, I think it might reasonably be supposed that the cooling, and consequent consolidation, of the rock proceeded more rapidly in some parts of the dyke than in others. Rocks are rarely perfectly homogeneous; we often find variations in density, and hardness, within short distances even in some rocks of apparently homogeneous composition—a difference not due to weathering, but which weathering, and the agents of erosion, speedily find out. Then the power of conducting heat must have varied much in the bounding rocks. From these two causes the solidification of parts of a dyke might have been in advance of neighbouring portions, and have resisted the shearing motion more than the latter. The portions in which solidification was most advanced might thus have retained their granitic structure, whilst the less solid parts might have yielded to the shear and have become foliated.

In the second place, I think it possible that shearing motion might result in the development, or what would come to the same thing,

the retention of heat (owing to variations in the conducting power of the surrounding rocks), more in some spots than in others; and thus some parts of the dyke might become more fused than other portions, and might lose the foliated structure previously imparted by the shearing motion. The heat thus generated, or retained, would account for the assumption of a granitic structure in portions of a dyke, but not for the foliation of the other portions.

Mr. Teall considers "that the most convincing proof that the foliated structure is the result of movement after the consolidation of the rock is furnished by its relation to fault planes. A rock must necessarily be solid before it can be faulted." But we learn from Mr. Teall's paper that there must have been a lengthened series of earth-movements at the Lizard; thus he speaks of the "superposition of the effects of distinct movements"; the fractures therefore, to which he alludes, may have taken place after the consolidation of the rock; and after the foliation had been produced. Do all the faults in the gabbro present foliated margins? I gather from Mr. Teall's article (read with Professor Bonney's paper in Q.J.G.S. vol. xxxiii.) that this phenomenon is principally to be observed at the junction of the gabbro with other rocks. But even if some of the faults in the gabbro do present this phenomenon, I do not see why, in those exceptional cases, the faulting should not have taken place before the *complete* consolidation of the gabbro. If the fault ran through the rocks above and below the gabbro, the gabbro would surely have divided in the line of the fault as readily as a layer of jam between slices of sponge cake does when the cake is broken. If the gabbro was not completely rigid where thus divided, the shear might have produced a foliated structure in the portions exposed to the tearing effects of the rupture.

In conclusion, I would invite the attention of the readers of the *GEOLOGICAL MAGAZINE* to the sketches of two foliated gabbro veins in the Lizard serpentine drawn by Professor Bonney (figs. 2 and 4), which will be found at pp. 893, 897, of his paper in vol. xxxiii. (1877), Q.J.G.S. Professor Bonney, in his letter published in the December Number of your *MAGAZINE*, tells us that the serpentine is "free from all indications of mechanical disturbance." This being so, it seems physically impossible that the foliation of the gabbro in the veins depicted in his paper can be due to "shearing movements" acting on a "solid rock." Furthermore, it will be observed that the foliation in one of the veins (fig. 2) not only runs with the two sides of the vein, but also with the boundary of the terminal end (*vide* the illustration). Professor Bonney tells us that the foliation runs "roughly parallel to the longer sides of the vein, except towards the top, where it tends to become parallel to the upper surface." This is what I should expect to see if gabbro in an imperfectly consolidated, or semi-plastic condition were forced into, or through, an opening in the serpentine rock; but it is, to me, totally incomprehensible on the hypothesis of "shearing motion parallel with a fault plane."