IV.—NOTES ON THE PROCESS OF SCHIST-MAKING IN THE MALVERN HILLS.

By CHARLES CALLAWAY, D.Sc., M.A., F.G.S.

IN the October Number of this Magazine, a paper by the Rev. A. Irving, D.Sc., calls for some comment. The subject, "The Malvern Crystallines," is one which has engaged my close attention during the last seven years, and two expositions of my views have appeared in the Quarterly Journal of the Geological Society.¹ I am glad to find that Dr. Irving accepts my main conclusion—that the Malvern gneisses and schists are of igneous origin; but there are some important details in which he has formed a different opinion. It will not be necessary for me to discuss these differences with any fulness, since my third and final paper, which is nearly ready, treats of some of his more material objections to my views.

One of the most important points of difference is the *origin of the granite-veins*. I hold that they are injected into the diorites, and therefore posterior in age; Dr. Irving regards them as segregatory and contemporaneous. I notice, with some surprise, that Dr. Irving quotes the late Prof. John Phillips in support of his theory. The words of the Oxford Professor, as recorded in the "Geology of Oxford and Valley of the Thames," appear to me to contradict his claim. Writing on the granites, diorites, and other crystalline rocks of Malvern, Phillips says (p. 62):—"Segregation from a fused mass is often regarded as the cause of these irregular mixtures; in several cases, however, the posteriority of felspathic and granitic veins to the masses which they traverse is quite certain. . . ."

Dr. Irving contends for the segregatory and contemporaneous origin of the granite-veins on the following grounds:

(1) Upon the way in which the diorite and the granite "graduate into one another;" and he alleges a case at West Malvern of a "gradation from diorite through a biotite or hornblende-granite to a very coarse pegmatite."

My experience upon this point lends no support whatever to this alleged graduating. There are few outcrops in the Malvern range which I have not examined, and I have never succeeded in finding a passage between granite and diorite. On the other hand, I have observed hundreds of contacts between those two rocks, in which they were seen to be sharply distinguished from each other, and I have examined scores of these contacts under the microscope with the same result. The supposed "gradation" at West Malvern is probably a case of the action of granite on diorite in contact, of which there are plenty of examples in that locality.

(2) "Upon the way in which the felspathic and the quartzo-felspathic veins ramify in all directions through the diorite."

This might sometimes be the case if the granite was ejected from below. The molten rock would pass along lines of least resistance, which would sometimes be horizontal or even trend obliquely downwards. As a matter of fact, however, a large proportion of the veins

¹ August, 1887, p. 525; August, 1889, p. 475.

are seen to rise from below, thinning out upwards and often forking (see fig. 2, p. 482, *Quart. Journ. Geol. Soc.* August, 1889). For one vein that runs horizontally, there are perhaps a hundred that are almost vertical.

(3) "Upon the fact that very often the hornblende rock is observed to be more completely basic in immediate contiguity with the felspathic veins."

It is true that sometimes there is an aggregation of hornblende or biotite forming a rim to the diorite at the junction with a granite-vein, as I pointed out in 1889 (*loc. cit.* p. 494); but I hold this to be the effect of the injection of the granite, by which a process of mineral aggregation and enlargement is originated, as had been noticed in other localities by Allport, Bonney, and others. Dr. Irving's theory of these basic rims does not explain why, when the hornblende (or biotite) is segregated from the felspar, it should leave on one side of it a band of plagioclase (mainly), and on the other a band which is chiefly orthoclase and quartz. The segregation of hornblende from felspar has, indeed, often taken place in the Malvern diorites, but, so far as I have seen, the felspar remains mainly diorite-felspar, viz. plagioclase.

(4) "Upon the very significant fact, that the quartzo-felspathic veins, even when less than an inch in thickness, have a coarsely-grained structure, this being so manifest as to make it impossible to believe that they crystallized in contact with a colder rock after injection into it."

Dr. Irving is justified in attaching importance to this argument. If it stood alone, it would have great weight. But the facts making in the opposite direction are so strong (*loc. cit.* pp. 494, 495) that I think it will not do to attach much value to a fact which may be susceptible of another interpretation. It is likely enough that when the veins were injected, the diorite was at nearly as high a temperature as the granite, so that the veins may have cooled down nearly as slowly as the large masses of granite with which they were in connection. For my part, I find it incredible that masses of potash-granite, hundreds of yards in diameter, should have segregated out of the diorite, that the segregated matter should be as coarsely crystalline in fine-grained diorite as in the granitoid variety, and that veins of segregation should produce the same contact-effects as veins of injection.

A second important detail is *whether schistosity was produced before or after consolidation*. Speaking generally, Dr. Irving says, "before." Speaking generally, I say, "after." The reasons for my view are given in part in my second paper, and will be continued in my next. I confine myself in this article to a brief reference to the "cakes" of quartz or quartz-felspar which occur in some of the more felspathic masses. Dr. Irving supposes that this flattening out into "cakes" took place when the quartz was in a colloid state. I select this case because it may seem to be a strong point in his favour. There is, however, pretty clear evidence that these "cakes" are merely very large "eyes," such as occur on a smaller scale in augen-gneiss.

The rock surrounding them, wherever I have seen them, is sheared into a well-foliated gneiss, with well-marked granulitic structure. Each "cake" forms a nucleus around which the gneissic folia are arranged concentrically, and would appear to be merely a tough mass which has succeeded in resisting the shearing action of the earth-mill. The granulitic structure would of itself suggest dynamic deformation subsequent to consolidation. The sheared rock was originally binary granite of very coarse grain, into which it often passes by imperceptible gradations, and the "eyes" can be identified as the same kind of granite, though they have often become more highly siliceous.

Dr. Irving does not appear to dispute that the *production of biotite in the diorite* is sometimes the result of contact alteration; but he denies that such is always the case at Malvern, and he instances the rock in the large quarry at the north-west corner of Swinyard's Hill. He states that the "kersantite" shows "no progressive alteration" as it approaches the "basic dykes." I should be very much surprised if it did. My induction was that usually black mica was developed in diorite at the contact with the granite-veins. It is true that no granite is visible in this quarry; but it is also true that one of the largest masses of granite (pegmatite) in the Malvern Hills crops out at a little distance to the east, where it forms the ridge of the hill for several hundred yards.

The *sericite-schists* of the south-western spur of the Raggedstone have attracted the attention of Dr. Irving. He thinks they form "a fragment of the lithosphere belonging to a much later stage of development than the rest of the Malvern chain," and he "failed in his attempt to trace a gradual transition from them into the gneissose rocks adjoining these schists." These schists are of extreme interest, and will be fully discussed in my third paper. In this place I may say that I *have* traced a "gradual transition" between them and the coarse-grey diorite, and I am in a position to prove by means of a series of about forty microscopic slides, cut from the diorite, the schist, and the intervening rock, that this highly acidic schist has been formed out of the diorite by a process of crushing and shearing, acting subsequently to the intrusion of the granite-veins and to the solidification of the mixed rocks.

On the *mica-schist* (formed from felsite) of the south-eastern spur of the Raggedstone, I have merely a word to say. Dr. Irving thinks I ought not to apply the term "schist" to this rock; but he does not deny that "cleavage-foliation" is to be seen in it. If there is true "cleavage-foliation" in the rock, surely it is entitled to be called a "schist" in the strictest sense of the word, if the usage of British geologists, from Jukes onward, is to go for anything. But Dr. Irving appears to use the term "cleavage-foliation" in a peculiar manner, since he compares this Raggedstone schist with certain "laminated sandstones, on the lamination surfaces of which a similar deposit of secondary mica has taken place." I cannot for a moment admit that such a comparison is justified by the facts. If Dr. Irving will turn to my first paper (1887, p. 531), he will see that, while

some of the passage rock is merely a sheared felsite with secondary mica deposited on the shear-surfaces, the central part of the shear-zone consists of true foliated schist.

Dr. Irving seems to employ the term "*shear-plane*" in a sense entirely different from my definition and description (1889, pp. 476, 477). My shear-planes *do* display a "rough parallelism," and they *do not* "cut through the rock-mass at all angles to the quarry face." Dr. Irving says that his "shear-planes" "are often conspicuously slickensided," and from his description of them, I should be disposed to think that they are merely the effects of crushing and sliding taking place long after the schist-making process was completed. This sort of thing is well-known and well-understood, and has no bearing upon the facts and theories set forth in my papers.

It should be clearly understood that it is impossible to see into the mechanism of the schist-making process in the Malvern rocks without the study of large numbers of microscopic slides. As Dr. Irving has not given us any details of microscopic structure, we are left in the dark as to the evidence on which he bases his conclusions.

V.—ON YORKSHIRE THECIDEA.

By JOHN FRANCIS WALKER, M.A., F.G.S., ETC.

I HAVE obtained a considerable number of specimens of *Thecidium ornatum*, Moore, from the Coral Rag, in the bed containing *Phasianella* (*Pseudomelania*) *striata*; *Pseudodiadema versipora*, *Hemicidaris intermedia*, etc., from a quarry between Ayton and Scarborough. My friend, the late Mr. Charles Moore, F.G.S., gave me some specimens of this Brachiopod from the Coral Rag of Lynham, Wiltshire, with which the Yorkshire *Thecidium* perfectly agrees in form. This shell, especially the dorsal valve, is plentiful at Ayton, but it is troublesome to find on account of its small size; it is best obtained by washing the marl; occasionally it is found attached to *Ostrea*. In my paper "On the Yorkshire Brachiopoda," York. Phil. Society Report, 1888, I remarked that a species of *Thecidium* occurs at Suffield, near Scarborough, in the Lower coral rag, generally attached to specimens of *Zeilleria Hudlestoni*. I have lately obtained by washing the sand some perfect shells, and consider that they are probably *Thecidea Moreana*, Buvignier.

VI.—WOODWARDIAN MUSEUM NOTES.

By F. R. COWPER REED.

AMONGST the Carboniferous Crinoidea in this Museum an abnormal form of *Platycrinus pileatus*, Goldf., from the Carboniferous Limestone of Bolland was recently discovered by me whilst cataloguing the collection. This specimen has only four arms and the calyx has suffered a corresponding diminution in the number of its plates. The cup is conical, and when viewed from the base roughly quadrangular: it expands somewhat rapidly so that its greatest diameter is only slightly less than its height. The three