DISTRIBUTION: Calcaire à polypiers (Bathonian.) Ranville, Normandy, France.

TYPE: Brit. Mus. D 181.

AFFINITIES.—The closest allies of *Onychocella bathonica* are four species from the Cretaceous, which have been described under other generic names. It is unfortunate that there is some doubt about its nearest ally, a Maestrichtien species, described by Hagenow; in his monograph he has given two figures¹ which he assigns to the species *Cellepora* (*Discopora*) *koninckiana*; but the structure of the aperture is so different in the two, that I feel bound to assign them to different species: in his first figure the aperture is mucronate and is small; in the second (fig. 11) the aperture is elliptic, with the longer axis longitudinal, the lower margin is entire, the aperture occupies twice as large an area as in the former, and the avicularia are much larger. I therefore make Hagenow's second figure (fig. 11) into a new species under the name of *Onychocella hagenowi*. This is the nearest ally of *O. bathonica*, but it differs in the larger size of both the avicularian and zoöcial apertures.

Onychocella piriformis (Goldf.)² is another ally, but has a lower zoöcial aperture, while the avicularian aperture is larger and the front wall occurs only above and not on both sides of this. *O. santonensis* (D'Orb.)³ has a smaller mouth and longer avicularia; *O. solea* (Novák)⁴ a semi-elliptical aperture with a mucronate lower margin.

The result, therefore, of the present communication is the first description of Bryozoa of the order Cheilostomata in the Jurassic; the species described belong to the two families of the *Membraniporidae* and *Microporidae*; but representatives in this age of the *Cellariidae*, *Smittidae*, and other families are also included in the British Museum Collection.

III.—NOTES ON THE COMPOSITION OF CLAYS, SLATES, ETC., AND ON SOME POINTS IN THEIR CONTACT-METAMORPHISM.

By W. MAYNARD HUTCHINGS, Esq.

(Concluded from the January Number, p. 45.)

AFTER this substance has been well observed in good occurrences, it is always recognized at once, even as isolated small patches in a slide. It has a very characteristic appearance among the other constituents. It is marked off, for instance, by its special refraction from anything else that occurs in these rocks, and has other characteristics not to be missed when once observed, but not very easy to exactly describe. As soon as it ceases to be quite inert in polarized light, as soon as the speckly polarization sets in, it has also a more or less granular

¹ Hagenow, *op. cit.* p. 95, pl. xi. figs. 10 and 11.

² *Eschara piriformis*, Goldfuss, *op. cit.* p. 24, pl. viii. fig. 10; Hagenow, *op. cit.* p. 75, pl. ix. fig. 6; pl. xi. fig. 6.

³ *Eschara santonensis*, D'Orbigny, *op. cit.* p. 109, pl. 603, figs. 1-3; pl. 673, fig. 4.

⁴ *Biflustra solea*, Novák, *op. cit.* pp. 94-95, pl. iii. figs. 12-16.

appearance in ordinary light with lowered condenser, which is very characteristic. In different slides, and often in one and the same slide, various stages of development may be observed in it from the first speckled appearance in polarized light up to a point where flakelets of white mica are recognized as forming in it, lying "criss-cross" in all directions. It passes in this way in some cases almost wholly into mica, with apparently other substances which cannot be identified with certainty, the transition, and connection with the original substance, being all the time distinct and beyond mistake.

In the inner zones, the "hornfels-zones," this substance is usually present in smaller amount, as already stated; but examples of its abundant occurrence are not wanting, as for instance in a hornfels from Spitzenberg, in the Harz. There is a mosaic of quartz and felspar, with brown mica and some white mica. The yellow substance lies in amongst, and surrounds, the minerals of this mosaic. It has no definite form of its own, simply occupying spaces and taking the outlines of the other minerals. It is all either in the minutely-speckly stage or variously developed from this upwards to mica; and the impression made is that if it had all developed as far as some portions of it have done, the whole mosaic-grains of the slides would be mixed in with and surrounded by white mica, as may not infrequently be observed in these contact-rocks.

Before discussing the probable nature of the substance above described, I will recur to the spots in the contact-slates at Shap. These spots are all marked off sharply by being relatively free from biotite in a rock which is very rich in that mineral, so that they appear by comparison in ordinary light as clear spaces, varying from almost colourless to a good deep yellow in some cases. The aggregations in them of anatase, ilmenite, etc., have been formerly described by Harker and Marr and myself (*Quart. Journ. Geol. Soc.* vol. xlvii. 1891; and *GEOL. MAG.* October, 1891).

Examination in polarized light shows them to be of several kinds, and when a sufficient number of specimens are examined it does not appear possible to point out any special development in their nature as taking place steadily as we approach the granite. Indeed, it may be remarked of the contact phenomena in general that they are somewhat erratic as we approach the contact up Wasdale Beck, which may probably be due to irregular contour of the granite below the surface.

Harker and Marr look on these spots as being "evidently imperfect crystals, charged with a large quantity of foreign inclusions," pointing out that some of the spots are sufficiently individualized to extinguish, as to their main constituent, pretty uniformly over the whole area and frequently parallel to the longer axis. They think the mineral present is likely to be andalusite, and I was formerly of the same opinion, but after renewed examination of the slides I am no longer so. Further study of the spots of this nature, and careful comparison of them with some continental occurrences, show that cordierite is the mineral in question. It is tolerably

abundant in some parts of the Shap contact near to the granite outcrop. Some slides from these parts are very closely paralleled by examples from the Elbe Valley contacts described by R. Beck (*op. cit.*), who says of some of them, "Cordierite represents the spots of the Knotenglimmerschiefer, which do not otherwise form any definite mineral." The cordierite of the Shap rocks is not in any way distinguishable from that of some of the rocks from the district described by Beck, sections of which I have examined, and also closely resembles the occurrences in rocks from the Lausitz district of Saxony. This, then, is another case in point where a definite mineral is formed in a rock wherein the real "spots" have not yet wholly died out, though less abundant than in the outer zones. There is nothing really to lend weight to the idea that any of these spots consist of andalusite, and this mineral is not detected in any of the altered sedimentary rocks at Shap.

Other spots, as previously stated, consist almost wholly of white mica, newly formed and full of quartz-grains and other things in the usual manner of this newly-formed contact-mica.

With these, and still more as we recede from the granite outcrop, are the true "spots," more or less closely resembling those above described, containing the substance in question in all its stages from amorphous, through minutely cryptocrystalline up to a definite micaceous development. And the same substance occurs in some slides outside the spots, or where these are quite absent, lying in among the mosaic minerals or surrounding them in the usual characteristic manner.

It remains to consider what may possibly be the nature of the substance seen so abundantly in the contact-slates from widely separated localities, and what are likely to be the processes to which it, and some of the other phenomena observed, owe their origin.

The first thing to remark is that the substance in question, whatever may be its exact nature, is certainly quite a *new formation*. It does not correspond to anything that can ever be observed in normal slates or shales, and we see that when it first makes its appearance in the slates, at the outer parts of contact-zones, it is mainly limited to "spots" and is in some way correlated to the "aggregations of dark grains"; also that these dark grains are very frequently to a large extent small plates of ilmenite, or crystals of anatase, or re-formed crystals of rutile, all of which either did not exist in the normal slate, or existed in a different form and not aggregated. If we consider how these aggregations of recrystallized minerals were caused, we cannot very well conceive of any process by which they migrated, recrystallized, and aggregated together *as such*. Thus, taking the simplest case, that of rutiles, in which a mineral has simply increased in size and aggregated, and not a case in which its material has reappeared in a new form, as rutile changed to anatase, or combined with iron-oxide as ilmenite, —we cannot assume that for each one of the larger and more perfect crystals seen, several of the minute "clay-slate needles" have travelled in the rock and simply combined to make one larger

individual. If this could take place at all it would only be conceivable at a temperature which rendered the rock plastic, and even supposing this condition may be attained close to contact we have every evidence that nothing at all approaching to it occurs at the outer zones. We seem to have only the one explanation which harmonises with all we can observe, that this aggregation of the minerals in question is due to *solution* and recrystallization. We know that no mineral is absolutely insoluble, even in distilled water, under ordinary conditions. Rutile, *e.g.*, has been used by Doelter as an example of this fact in his experiments. We know that increase of temperature and pressure, even in the moderate range we can command experimentally, enormously increase the solvent action of water, and that in saline solutions still greater effects can be produced.

For the observed phenomena of contact action it seems natural to conclude, and probably most people do conclude, that they can be, and have been caused by processes analogous to those in our sealed tubes and digestors. If with the comparatively moderate allowances of pressure, temperature and *time* granted to us, we can in such apparatus bring about the striking results recorded by experimenters in this direction, what may we not suppose to take place in the enormous sealed tube which is represented by deeply-bedded masses of rocks, highly heated for long periods under intense pressure, with water and solutions which cannot escape.

It may be assumed that under such conditions solutions of mineral constituents would be formed of great density, amounting to something like an "aqueous fusion" of the substances involved, and that these solutions could solidify to amorphous and more or less indefinite compounds, or would be capable, under some conditions of cooling, of giving rise to definite minerals.

It seems a not unreasonable supposition that the new substance seen in so many contact-rocks has been formed in this way, and that what we see of it represents various stages of its developments and of its residues; also that the solution supposed would have a most important part in the transformations of the minerals not originally taken up into it.

It would naturally be formed in greatest amount nearest the intruding granite, where temperature would be highest and the heat longest continued. There, also, the conditions of subsequent cooling would usually involve greater length of time and greater opportunity for recrystallization, so that in most cases we should have here least of any indefinite residue, or might often have no such residue at all left over for our observation. In the outer zones of the contacts we should have less of the dense solutions, either formed or, more likely, penetrating from the more intensely affected zones where they were abundant; but as such outer portions would soonest commence to cool again, and cool most rapidly, we should here have a larger proportion of the residues of these solutions left in an undeveloped, or slightly developed, condition.

Comparatively small portions of such solutions reaching these

outer regions would tend to draw together, as little spots and patches, among the ingredients of the otherwise not much affected rock, and would consolidate in this form.

As in contact-regions generally there can frequently be observed anomalous phenomena, such as strata, or portions of strata, which have been much less or much more affected than others close to them, without our being able to explain the fact, so we have anomalies in the behaviour of these supposed dense solutions, and an inner zone may sometimes show a large amount of the less developed residual matter, as in the case of the hornfels mentioned above from Spitzenberg, and other cases of which I have specimens. A notably good example occurs in one of a series of rocks from the Elbe Valley, in which the large amount of the substance present surrounds and envelops the minerals in such a manner that a glance at a section is enough to convince the observer that it was present as a fluid medium when these minerals crystallized.

If we look at all the changes which have taken place in a well developed contact rock, like that at Shap for instance, it seems absolutely necessary, in order to explain what we see, to assume that almost all the original ingredients have been taken up into some sort of solution and completely newly formed out of it. It is not necessary to assume that they were at any one time all in solution at once, or even a very large portion of them.

A solution, or solutions, would be formed of those ingredients of the rock which were most readily acted upon by the imprisoned liquids, and the perhaps only relatively small amount of solution so formed would act as a medium for the slow solution and recrystallization of minerals, just as relatively small amounts of certain solutions and fluxes have been made to act in recorded experiments. The composition of the first solution would not be always the same. It would vary with the varying composition of the slates, etc., acted upon, but probably not in very wide limits, as the *qualitative* composition of most slates is strikingly uniform. The variation might, however, be expected to be sufficient to influence the products which would result from the crystalline development of the solutions. According to my observations the most usual development gives rise to mica in greatest quantity, but feldspar and quartz also result in some cases.

Let us, finally, glance at the principal mineral changes which we know to take place in contact-metamorphism, and see what conclusions we seem justified in drawing as to how they have come about, and how these harmonise with the ideas above suggested.

(1) *Rutile*.—By universal testimony we find that in all contact-areas of slates, etc. (whether the intrusive rock is granite or a more basic rock) one of the most unailing, as well as most sensitive, indications of commencing metamorphism is that the minute "clay slate needles" undergo an alteration by which they decrease in number and increase in size, at the same time that they become blunter in proportion to length and have more definite pyramidal terminations. This occurs so far from the intruding rock that we cannot suppose

any but very moderate thermal or other effects, due to its intrusion, to have acted; and this conclusion is borne out by the very slight effects, if any, which can be seen to have taken place in other particulars. The sedimentary rock being under those conditions of temperature and pressure due to its depth of cover, etc., the intrusion of the igneous rock may have slightly intensified the temperature at these points,—possibly also the pressure, for a time. But this has sufficed to bring about the alteration of the rutiles.

We have already considered the question as to *how* the change is brought about, and simple *solution* of the rutile appears to be the necessary process. The intrusion of igneous rock appears to have given rise to this, and as other effects are absent or so slight, we need not assume anything more than water, or perhaps a weak alkaline solution, to have come into play. The manifold forms in which the dissolved titanate may recrystallize have been pointed out, and we may now add brookite as a further extension of this list, R. Beck having observed its occurrence in the rocks of the Elbe Valley contacts, in a manner exactly similar to that of the Anatase at Shap.¹

(2) *Biotite*.—Perhaps the next mineral change, in degree of sensibility, is the appearance of biotite in the slates undergoing contact-metamorphism, which is also a matter of universal observation. It takes place at considerable distances from the contact, and may be seen already far advanced before the formation of regenerated quartz-mosaic and other minerals is demonstrable.

A large number of slates, etc., contain a good deal of chlorite, which was deposited in them at a period subsequent to their formation as such, by simple infiltration processes. This chlorite is altered into biotite.

This change, like the one last considered, also cannot be explained by *direct* action of raised temperature or pressure. No amount of simple “molecular rearrangement” of chlorite under heat or pressure will make biotite of it. A portion of its water could be removed, as is required; but it has also to take up potash, and here again we must look to solutions, which in this case must contain some alkaline salt.

There are, again, many slates, shales, etc., which are wholly or almost free from chlorite, but which contain a sufficient amount of magnesia to correspond to a good deal of biotite, this magnesia appearing to have entered into combination in the new micaceous minerals existing in the slate, etc. Under contact-action biotite may be, and is, developed in such slates, and this could take place by easily conceivable molecular rearrangements.

Whether it be possible for flakes of chlorite, lying in among flakes

¹ Even this list of changes does not exhaust the number of transformations the titanate may undergo. At later stages of the metamorphism it appears that part of it can be taken up into the newly-formed contact-biotite, thus ending up where it began,—combined in dark mica. Such contact-biotite has been shown, by Lang and Jannasch, to contain as much as 3.40 per cent. of titanate, and Beck records (*op. cit.*) cases of *decayed* contact-biotite in which rutile has once more been separated out.

of muscovite, to be converted into biotite by direct combinations with those minerals we cannot say. Probably some geologists would not doubt the power of intense pressure to bring this about. We may question whether pressure alone could do this, or anything like it, but when high temperature and water are also considered, acting at the same time, we must at least look on this method of formation as not outside the limits of consideration.

(3) *Newly-formed quartz and felspar.*—We cannot well consider the occurrence of these “regenerated” minerals separately, because though they may not occur together, yet when they do so occur their modes of formation appear to be intimately connected.

The quartz is the more frequent, and, even when felspar occurs, is the more “sensitive,” occurring at earlier stages and being often largely developed before any felspar is seen.

It seems obvious that the quartz-grain of the “contact-mosaic” bears no direct relationship to the clastic grain of the original slates, etc. The latter, with its more or less angular outlines, its fluid-cavities, and its practical freedom from enclosures, has disappeared, and in its place we have the round, or in the best-developed mosaics the polygonal, grain, free from fluid cavities, and containing usually numerous microlites, biotite crystals and grains, and other enclosures, these grains of quartz fitting to one another and to the other constituents of the mosaic quite closely and exactly.

It is here still less possible to see how this change can be due to any simple re-formation of each individual grain of quartz,—to its mere recrystallization *in situ*. Neither is it due to any simple addition to its outer portions of any freshly-deposited silica. Looking at these mosaics carefully, it does not seem easy to come to any other conclusion than that the quartz in them, as a whole, has recrystallized out of some sort of solution in which were, forming or already formed, the microlites and other bodies which are now enclosed within the quartz grains.

Many fine-grained slates do not contain any distinguishable grains of quartz at all, and in these, as also in slates where such grains are plentiful, there is a good deal of exceedingly finely-divided quartz or other forms of free silica. It is equally difficult to see how this could be formed up into the larger grains of the mosaic, and charged with enclosures, without a previous re-solution and recrystallization.

Taking now the felspar, where such occurs, all the same considerations apply. The grains in the mosaic have the same characteristics as regards general outlines, enclosures, etc., and it is clear that the quartz and felspar were crystallized *together*, though there is a distinctly greater tendency in the felspar to develop idiomorphic forms.¹

¹ It should be noted that the “tessellated” or “mosaic” structure is by no means peculiar to the alteration of *sedimentary* rocks. At Shap, for instance, it is very beautifully developed in some of the altered rhyolitic and andesitic ashes in the contact-zone; sometimes it is quartz alone, sometimes quartz and felspar, and sometimes felspar alone which forms these mosaics.

A tessellated structure has been looked upon by some observers as quite specially a result of *dynamic* metamorphism, but this idea can, of course, no longer be enter-

It is to be noted that during the stage of metamorphism which develops the mosaic of quartz, or quartz and felspar, the biotite formed at an earlier, less intense, stage undergoes further alteration. It may be seen (as at Shap) that the earlier biotite is more ragged and indefinite in form, is less dichroic and less bi-refractive than that which exists in the mosaics nearer contact. Whether it is redissolved and recrystallized in part it does not seem possible to decide; but the biotite which is *enclosed* in the quartz and felspar is of much interest, and may give us some indications as to the degree of temperature which prevailed during the processes. In addition to small flakes and more or less definite *crystals*, we see numerous oval grains and little *balls* of biotite, whose forms seem to indicate that they were quite plastic at the time of inclosion, and we may well assume that in these zones there was a condition of aqueous fusion of sufficient intensity to melt, or soften, portions of the biotite.

The interesting question arises as to the mode of origin of this new felspar: What is it made from?

It may, I consider, be stated as a fact that *new* felspar does not exist in any normal slates, no matter how highly developed they are. I have given much time and patience in studying this special point, and have convinced myself that no regeneration of felspar takes place. In the Cornish slates no trace of felspar of any kind is seen, though these are so very highly developed. Rénard records the same absence of it in the very similar Ardennes rocks.

In Welsh slates a few grains are sometimes seen of *clastic* felspar more or less altered, and this is seen in other slates also, but such grains are rare and unevenly disseminated. At Shap, as before stated, no trace of it is seen in the less altered slates, though it is so abundant in the well developed mosaics. We have, therefore, in most cases not to deal with a recrystallization of an already existing mineral, but with the formation of this mineral from other materials. The average and normal slate is composed of the materials which make up the clays with which I dealt in the first part of this paper. The simplest explanation, and indeed perhaps the only one open to us on the evidence in our possession, is that the materials of the mica and the quartz of the slates combine, under the conditions we are supposing, to form felspar. In its decay felspar yields mica and free silica, and it is perfectly simple and easy to comprehend how, under the conditions of temperature pressure, etc., we are supposing, with the intervention of such a dense mineral solution as I have shown cause for believing to exist, the reverse process may take place, and we may have a *partial*

tained. It may be well seen in igneous rocks. Thus, General McMahon shows it to occur in the marginal portions of Dartmoor granite (Q.J.G.S. vol. 49, p. 388), and has previously shown its occurrence in granite of the Himalayas. It is also, as is well known, seen perfectly developed in the groundmass of some quartz-porphyrries, not to be distinguished, as to its structure and general appearance, or in anything except its inclusions, from some of the finer-grained mosaics of altered slates. In considering this structure in the contact-metamorphism of slates it is a point of some importance that exactly similar structures can be seen to result during the consolidation of purely igneous magmas.

re-formation of the felspar which originally formed part of the deposits of the slates, but which no longer existed in them as such.

(4) *White mica*.—In some, probably in most, contact-areas a good deal of white mica is newly-formed. It may not be always easy to trace the exact stages of the process. The original slates are usually rich in white mica to begin with, and the recrystallization of the mineral in the earlier stages of contact-metamorphism is not readily distinguishable. But in the more advanced stages we see it more plainly, and find a good deal of mica which from its general appearance and relationship to the other minerals is clearly re-formed, this being also demonstrated frequently, as to larger individuals, by their being more or less charged with quartz-grains, etc., in the same manner as are the biotite-flakes and crystals, and the other newly-formed minerals.

In addition to this mica which appears plainly to have recrystallized in a manner exactly similar to that of the quartz and felspar, we often see also the mica of which I have spoken as appearing by various stages in the newly-formed substance to which so much reference has been made, and which in the fullest form of its development will remain as patches among the mosaic, or as fringes round its component minerals, and may not in all specimens be visibly connected with any residual portions of the substance out of which it originated.

(5) *Cordierite, Andalusite, Cyanite, Sillimanite*.—These are the minerals we may perhaps designate as more specially “contact-minerals,” and we may safely say of them that the conditions of their formation, and why sometimes one is formed and sometimes another, are the points as to which we probably know least of all.

As regards cordierite, it appears to stand in a somewhat different position from the rest. It is not a silicate of alumina only, and its formation appears to take place often at a much earlier stage of the metamorphism. It is also a much less stable mineral than the others.

Looking at the composition of cordierite and the mode of its occurrence in these contact-rocks, the most reasonable supposition as to its origin appears to be that when the recrystallizations and rearrangements of the minerals commence, the cordierite, when formed, largely represents the magnesia which is combined in the mica of the slates,—mica which we call sericite and which we have good reasons to regard as muscovite into whose composition magnesia has been taken. As recrystallization of this mica takes place, it will have a tendency to purify itself, and to approach more nearly to the normal muscovite, with more potash and less of other bases; and the magnesia will be available for the formation, more usually of biotite, but under conditions we cannot specify, more or less of cordierite forms also.¹

¹ It seems desirable to point out that there is risk of sometimes confusing cordierite and felspar in such rocks, where the cordierite occurs as smaller grains and is not a striking constituent. In the books we are warned against mistaking cordierite and quartz; but a similar warning does not seem to be given as regards felspar. As regards quartz the risk does not appear to be great. Where there are several grains

Staurolite may be appropriately mentioned at this point. It is also not a silicate of alumina only, and from its chemical composition may be reasonably supposed to have an origin of somewhat similar nature to that of cordierite.

It does not appear to occur as frequently as the other minerals now being considered; but when it occurs it seems to form in the less intense stages of metamorphism, as for instance in the example studied by Mr. Barrow, to which reference is made below.

As regards the other minerals, the aluminous silicates, it is usually assumed that they represent the "kaoline" of the slates, etc., and there is on the face of it much in favour of this view. If this be so we should expect rocks rich in alkali in proportion to alumina to give little or none of these aluminous silicates during metamorphism, and there are cases where this is borne out, as at Shap for instance. But I do not think that an examination of the analytical evidence in general will show that we can make a rule of this. There are analyses of rocks very rich in andalusite, for instance, which show high percentage of alkali; and copious development of andalusite and felspar may take place together, as is seen in some of the Andlau rocks. At Shap also, we have altered rhyolitic ashes in which there is a great deal of newly-formed felspar, together with andalusite, and an unusually large development of sillimanite. Therefore it would appear as if the formation of these minerals is a more complex question than to depend simply on certain ratios among the chemical constituents of the rocks metamorphosed.

For the rest, looking at their chemical composition and their mode of appearance in our sections, their relationships to other minerals, etc., it would be even more difficult than in other cases to imagine their origination in any other way than by a dissolving-up and complete re-combination of the original materials of the slates, etc.

Concerning the conditions determining *which* of the aluminous minerals shall be formed, we have as yet very little to guide us; but in a paper of exceptional interest, recently read before the Geological Society, Mr. Barrow produces evidence to show that probably difference of temperature is the principal controlling element. In his work on a contact-region in Scotland, Mr. Barrow has observed that sillimanite, cyanite, and staurolite occur in the order named as we recede from the actual contact, and he suggests that the occurrence of each mineral coincides with a zone of temperature. He correlates this observation with the experimental of cordierite in a slide it will nearly always be possible to prove that we have a *biaxal* mineral before us, and therefore not quartz. But in a thin section it may often happen that we cannot say whether the biaxal grain is cordierite or felspar, simply by its optic examination. Felspar grains, without any cleavages or definite forms, occur in these rocks. The refraction and bi refraction are not sufficiently different to serve as guides when cut thin, and in many of the best occurrences of cordierite in contact-slates the dichroism of the mineral, and the "halos" round its enclosures, are not discernible at all in most grains. Enclosures, etc., may aid in discrimination, and usually do so; but I have satisfied myself that the determination is often a difficult one, and think that there is considerable chance of cordierite being overlooked in some cases in consequence.

work of Vernadsky, in which it was shown that when cyanite is heated to 1300°—1400° C. it is altered to sillimanite.

Mr. Barrow has made it appear very probable that in the hotter zones of contact sillimanite will be formed, that cyanite will take its place when a certain limit of lower temperature is reached, and staurolite will occur in the still cooler outer zone. No doubt these observations of Mr. Barrow in this particular district will cause the point to be carefully studied elsewhere.

Mr. Barrow further infers from Vernadsky's experiment that we may conclude that in the "sillimanite zone" of contact-rocks there has been a temperature of 1300°—1400° C. or over. This seems rather straining the matter; and certainly attempts in the past to draw conclusions as to rock-temperatures from experiments on minerals in the laboratory have not been so happy in their results as to encourage repetition, however tempting a system of "recording pyrometers" in the rocks may appear. It does not seem in the least necessarily to follow from Vernadsky's experiment, either that the sillimanite in the rocks was produced from cyanite, or that the formation of sillimanite *instead of* cyanite in the hotter zones indicates that the temperature was 1300°—1400° C. or that there need be any definite relationship whatever between the temperature at which it forms in these rocks and that at which it may be produced, from another mineral, in a laboratory-furnace.

If contact-metamorphism be due entirely to the thermal effects of the intrusive rock, as now seems usually to be believed, we should expect to see these effects always the same, in kind, on the same variety of rocks undergoing metamorphism, and the alterations due to a dolerite should not differ from those due to a granite. In the main this seems to be borne out, but there are some important points which still call for a good deal of attention.

I may state that so far as my own observations go, in which I am still engaged, there is, for instance, a formation of the same substance, due to the same causes, as I have described above; but it is present in less amount, and a larger proportion of it remains in the amorphous, or slightly developed, state. Also there is much less formation of "mosaics" and these again much less developed. These facts can be easily understood by having regard to the relative smallness of the masses of igneous rock acting in the observed occurrences, and the consequent less *time* during which recrystallization would be able to go on.

It does not appear that isotropic substance is much mentioned by observers of altered slates, etc., at *granite-contacts*; but it is frequently spoken of where the intrusive rock has been "diabase."

The most striking difference, however, between acid and basic contacts, so to speak, lies in the fact so often recorded, and backed up by so much evidence, microscopic and chemical, that there is a transfer of material from the intruding basic rock to the slates, etc.; a transfer which is large in proportion to the relatively small masses of igneous rock concerned in it. It is shown by the increase of *soda* in the altered rocks, chemically considered, this increase being

again represented mineralogically by the formation of albite and probably other soda-bearing felspars.

The transfer has been disputed by some observers, but the evidence is too strong to be denied, even if it may be proved that there are exceptions. It has also been suggested that the transfer has not taken place at the time of intrusion,—that it is not a contact-phenomenon at all,—but is due to later processes connected with the decay of the igneous rock. This is a point still calling for investigation.

I hope later on to deal with the subject of these basic contacts, as I am at present engaged in examining a large series of such rocks.

All that I need here remark is that I have fully satisfied myself of the transfer of soda, and of the presence in some cases of large amounts of isotropic material due to contact-action.

IV.—NOTE ON A TOOTH OF *OXYRHINA* FROM THE RED CRAG OF SUFFOLK.

By ARTHUR SMITH WOODWARD, F.L.S., F.G.S.

ALL the Selachian teeth hitherto obtained from the English Crags referable to the genus *Oxyrhina* are much compressed antero-posteriorly, and are thus slender in proportion to their size. They are now commonly regarded as representing a single extinct species, *Oxyrhina hastalis* of Agassiz,¹ and the same form of dentition

Tooth of *Oxyrhina crassa*, Ag., natural size, from the external (A) and lateral (B) aspects; Red Crag, Hemley Hall, near Woodbridge, Suffolk.
[Collection of T. W. Reader, Esq., F.G.S.]

is abundant in the Pliocene of Italy. besides occurring in other Tertiary deposits in various parts of the world. In Italy and Belgium, however, these comparatively slender teeth are accompanied by a few

¹ Smith Woodward, "Catal. Foss. Fishes, B. M.," pt. i. p. 385; E. T. Newton, "Verteb. Pliocene Dep. Britain" (Mem. Geol. Surv. 1891), p. 106, pl. ix. fig. 15.