Under these circumstances some at least of the skeletons of drowned animals would be buried in the sediment at the bottom of the flood; while any accumulation of bones lying on the plain would be rapidly entombed. That great accumulations of this kind often do occur near water, is indicated by Mr. Hesketh Prichard's observations recorded in his recent book on Patagonia.[1] On the bank of one small muddy lake he found a heap of at least 500 skeletons of guanaco, which had perished during the severities of the previous winter. "Their long necks were outstretched, the rime of weather upon their decaying hides, and their bone-joints glistening through the wounds made by the beaks of carrion-birds." A desolate plain adjoining Lake Viedma is also described as covered with the bones of guanaco and other mammals in great profusion. In fact, in winter the animals seem to congregate near drinking-places where the water is likely to be free from ice, and there they die of starvation in immense numbers.

According to an observation communicated to me by Professor McKenny Hughes, when bones are exposed to the vicissitudes of ordinary weathering they often disintegrate into sharp flakes. He has noticed this phenomenon especially in the case of bones of rabbits scattered on the ground. It is therefore quite likely that the sharp splinters found mingled with the complete bones in many of the bone-beds are not the result of any physical violence, but merely of prolonged exposure to the elements.

IV.—The Diffusion of Granite into Crystalline Schists.

By Edward Greenly, F.G.S.

(PLATE XIII.)

1. *Roberts-Austen's Experiments on the Diffusion of Metals.*

About a year ago my friend Professor Dobbie, of the University College of North Wales, drew my attention to the remarkable experiments of Sir W. Roberts-Austen (whose premature death we must lament as a very great loss to science) on the diffusion of solid metals, suggesting that they might have some geological application. The phenomena referred to in this paper, in which I have been very keenly interested ever since my work as a Geological Surveyor in Eastern Sutherland, occurred to my mind at the time as a probable case; but after some reflection certain difficulties began to appear, and I put the subject aside for a while. The very suggestive address of General McMahon to the Geological Section of the British Association at Belfast has reawakened my interest; and it seems to me worth while to put forward some considerations on the matter, somewhat speculative indeed, but which may perhaps be of service in stimulating research on a fascinating though difficult subject.

Sir W. Roberts-Austen[2] showed that certain selected substances, especially gold and lead, were able to diffuse into each other in the solid state, and at temperatures far below the fusion-point of either.

[1] H. H. Prichard: "Through the Heart of Patagonia" (1902), pp. 189, 203, 254.

[2] Roberts-Austen: Bakerian Lecture, Phil. Trans., 1896, vol. clxxxvii; Proc. Roy. Soc., Oct. 1900, p. 436.

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Rods of the metals were placed end to end, and in each case the gold diffused upwards into the lead.

At the end of four years, at only 18° C. (ordinary Summer temperature), gold could be detected 9·95 mm. from the contact. At 251° C., which is still 75° C. below the fusion-point of lead, at the end of 31 days, ·002 per cent. of gold was found 7 cm. from the contact. If the column of lead was kept liquid, the diffusion was much faster. But as much gold would pass up into liquid lead in a day as into solid lead at 18° C. in 1000 years.

It is clear, therefore, that in solids, as General McMahon remarks, as well as in liquids and gases, there is a good deal of molecular movement; and as we cannot suppose this to be confined to a few cases only, we may expect diffusion to take place between many solids under favourable conditions. Any pair of solids cannot, of course, be expected to diffuse, any more than any pair of liquids—mercury and water, for example. But solids with as much in common as most silicate-bearing rocks might reasonably be expected to do so.

2. *The Metamorphic Theory of Igneous Rocks.*

Before attempting to apply Roberts-Austen's results, it will be desirable to refer to the relation of granites to crystalline schists in highly metamorphic regions; and, first of all, to review a theory which at one time had much influence upon geological opinion, and even now continues to recur from time to time to the mind of the worker in regions of this description.

The eruptive nature of granite has, ever since the classic demonstration of Hutton, been rightly regarded as one of the established truths of geology, and this has been confirmed by numberless examples since discovered in all parts of the world, and in rocks of all ages.

But the phenomena to be seen at the margins of granites do not always show clear evidence of intrusion, and the study of some of these led to a modification, about the middle of the nineteenth century, of Hutton's original view. That granites are often, perhaps generally, intrusive, was never, I believe, denied. But it was asserted that in many cases the margins showed a gradual transition into the material of the surrounding rocks; and it was inferred that these rocks had been in such cases, not merely altered in mineral character, but actually melted down, and had recrystallized in cooling as granitoid material; that, in fact, the granite was, in part at any rate, of metamorphic origin. From this view it was an easy transition to that according to which such granites were regarded as of metamorphic origin throughout their whole body; the heat to which such fusion was due being then ascribed, not to intrusion of heated foreign matter, but to local intensification of the internal heat of the earth. A comprehensive resumé of the theory is given, with his usual admirable lucidity, by the late Professor A. H. Green in his "Physical Geology" (ed. 1882, pp. 399–455).

Unfortunately, however, the theory was not always applied in this moderate and scientific spirit. The chemical composition of

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even the acid igneous rocks always presented difficulties, but basic rocks and, I believe, even peridotites and serpentines were sometimes supposed to have originated in this way, as well as felsites, which could not have consolidated under plutonic conditions.

It is easy to be wise after the event, and for one period of human thought to point the finger of scorn at the aberrations of its predecessors; and we must not forget that at that time hardly anything was known of the microscopic structure of rocks, and very little more of their chemical composition.[1] When, therefore, in the light of microscopic and chemical research the field evidence for many of the alleged cases of transition broke down, it is not surprising that the whole theory was cast aside, often with no little scorn, and relegated to the limbo of exploded hypotheses.

The remark has been made by Mr. Herbert Spencer that as there is "a soul of goodness in things evil," so often is there, and that very generally, "a soul of truth in things erroneous." And in this old theory there *was* a soul of truth.

It is noteworthy that most of the cases in which the evidence so hopelessly broke down were those where the igneous rocks were surrounded by tracts of ordinary sedimentary rocks that were only locally, not regionally, metamorphosed. Regionally metamorphosed rocks had been, indeed, examined, and speculation aroused concerning them; but the time for systematic research into their phenomena had not yet come.

The past twenty years or so, however, have seen much energetic and enthusiastic research into the crystalline schists, and really scientific methods applied to their problems. Now, during that period descriptions have been given, from time to time, of a good many cases where granitoid rocks which occur in districts of regional metamorphism have been really observed to pass into the surrounding gneissose rocks by perfectly gradual transitions. North America, Scandinavia, Saxony, the Alps, more than one part of the Scottish Highlands, Ireland, and even Anglesey, have furnished examples.[2]

[1] The time and labour demanded by analyses of silicates have always stood in the way of a really thorough knowledge of the chemistry of rocks, and at the present time hardly any work is so much needed in geology, if intelligently directed in conjunction with microscopic and especially with field work.

[2] Lawson: Geol. Rainy Lake Region, 1888, pp. 118, 130, 137.
Van Hise: Pre-Camb. Rocks N. America, Corr. Papers, 1892, p. 488; and, quoting Jukes, Hitchcock, and others, p. 479.
Reusch: The Bömmel and Karm Islands, 1888.
Lehmann: Enst. Altkryst. Sch., pp. 64, 67, etc.
Lory: Etudes Sch. Cryst.: Congrès Inter. Geol., 1888.
Bonney: Pres. Address Geol. Soc., 1886, p. 51, etc.; Two Traverses Cryst. Sch. Alps, Q.J.G.S., 1889, pp. 95, etc.
Barrow: An Intrusion Muscovite-Biotite Gneiss, etc.: Q.J.G.S., 1893, pp. 341, 343, 353.
Horne & Greenly: Fol. Granites and Cryst. Sch. East Sutherland: Q.J.G.S., 1896.
Cole: Metam. Rocks Tyrone and Donegal: Roy. Irish Ac., xxxi (1900).
Greenly: Sillim. Gneiss, Anglesey: Geol. Mag., 1896, p. 495.
Teall: Pres. Address Geol. Soc., 1902, p. lxxiv.
(These references are of course not exhaustive.)

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3. *Gneisses and Granites of Eastern Sutherland.*

The phenomena of Eastern Sutherland were described some years ago in a joint paper by Dr. John Horne and myself.[1] The granites, which are generally foliated, lie as sills in a region entirely composed of gneissose rocks in which no original structures whatever have been detected. On parts of the northern coastline, where granulitic, somewhat siliceous rocks prevail, the granitoid matter is injected "lit par lit," producing complex synthetic banded gneisses. But on other parts of the coast, and inland about Kinbrace, where a flaky or wavy biotite gneiss is the dominant type, we find the permeation phenomena.

In "lit par lit" injection the injecting and the injected rock retain their separate individualities, however thin and frequent may be the sills; whereas at the permeation junctions "the margins of a sill fade off into the gneiss through a series of thinner and thinner lenticles" (Pl. XIII, Fig. 1), "the ends of a sill also fading off into the gneiss by a dovetailing of biotitic folia into the granite" (Pl. XIII, Fig. 2). "Finally, large masses occur in which these relations are carried to such a degree of intimacy as to render it very difficult to decide whether to regard them as granite or as gneiss (Pl. XIII, Fig. 3), difficult even to produce a consistent map, all lines being wholly arbitrary" (op. cit., p. 644).[2]

In the same paper (pp. 642–3) evidence is adduced to show that much of the gneissose rock so permeated must be of sedimentary origin. That it does not consist merely of the material of the adjacent granites altered by marginal shearing is shown by the existence of uninjured intrusive junctions at other parts of the same sill (ibid., figs. 2, 3). In conclusion (ibid., pp. 647–8) it was suggested, though with the caution due to the chemical difficulties to be encountered, that the granites might not be wholly foreign matter; and this was alluded to in the discussion by several speakers, who pointed out that the suggestion was really a revival of the older theory which I have described above.

4. *Application of Roberts-Austen's Results.*

Now, in the interpretation of phenomena of this kind the results of Sir W. Roberts-Austen's experiments seem to open up a prospect of considerable help. In the permeation zones of these granites, whatever may be their cause, we see, at any rate, an unquestionable case of the diffusion of one rock into another.

Roberts-Austen has shown (1) that diffusion takes place between closely adpressed solids at ordinary temperature, (2) that with rise

[1] "On Foliated Granites and their relations to the Crystalline Schists in Eastern Sutherland": Q.J.G.S., 1896. The views of our colleague, the late Hugh Miller, jun., are also given in this paper.

[2] I had not at the time this was written read this passage from Lehmann (Enst. Altkryst. Sch., p. 64): "Die Abgrenzung zwischen dem, was als Granit oder Granitgneiss und dem, was als Gneissglimmerschiefer zu bezeichnen wäre, wird oft unmöglich *und kommt ganz auf subjectives Ermessen hinaus*, so schnell wechselt der Gesammtcharakter," but cannot refrain from quoting it now. I put one phrase in italics.

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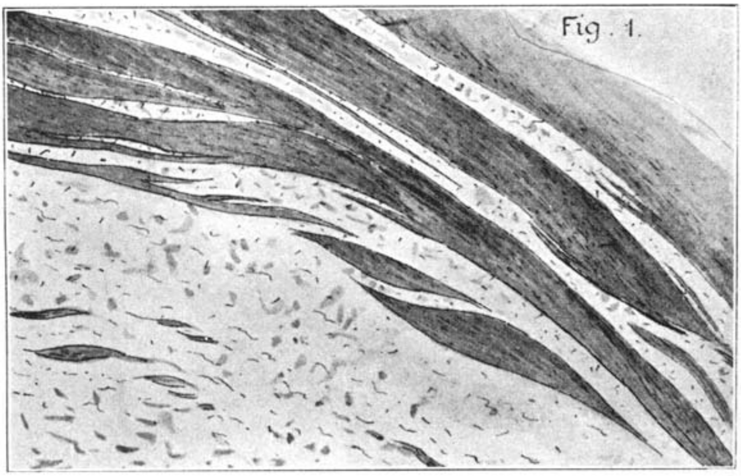
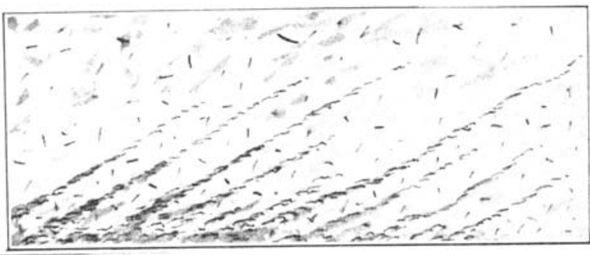


Fig. 1.—Side of Granite Sill, Strathy Point.

Note.—The junctions here shown are much sharper than those of the true permeation phenomena, the softness of which it is very difficult to represent.

Fig. 2.—End of Granite Sill, Kinbrace.



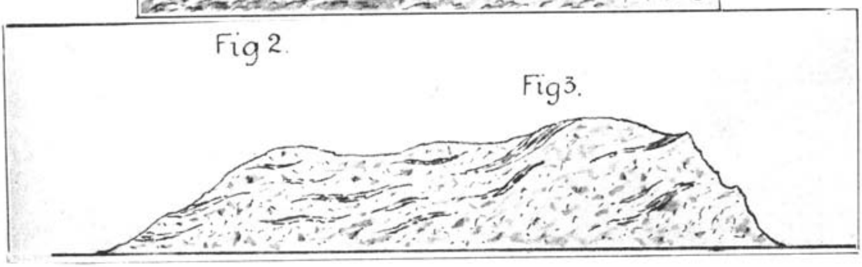


Fig. 3.—Cliff about 200 feet high, Glas Eilean, Strathy Point.

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of temperature diffusion is greatly accelerated, (3) that if the temperature is kept permanently above the fusion-point of one of the substances, diffusion is still further accelerated.

It may be asked why there is any necessity to appeal to these results, seeing that we have long known that igneous magmas are intruded in a liquid state, and into remarkably narrow veins. But if ordinary igneous intrusion can account for all the phenomena under consideration, why do we not find permeation zones surrounding all intrusive rocks, even ordinary basalt dykes, for there is reason to believe that basic magmas have a high degree of liquidity?

On the other hand, it is clear that solid diffusion does not take place between rocks by mere close adpression, even after long periods of time, for junctions of normal igneous, even plutonic masses with sedimentary rocks of all ages, as well as junctions of igneous and sedimentary rocks with one another, can be seen at which no permeation whatever has taken place. Rocks do not, it is evident, diffuse with the ease that gold and lead do.

It is clear that another factor must be necessary, and this can be found, I believe, in the existence of *an already high temperature in the surrounding rocks.*

Ordinary igneous intrusions, as is shown by their chilled edges, found the rocks into which they were injected relatively cold, i.e., not appreciably above the temperature proper to a zone of the earth-crust far outside that from which the magma came. They cooled, therefore, at the margin soon after injection, and did not remain in contact at a high temperature for any length of time.

But there is abundant evidence in permeation regions that the granite at the time of injection found the surrounding rocks already at a high temperature. The junctions in Eastern Sutherland are clearly exposed in a great many places; and yet no sign of a chilled margin has been detected anywhere. The same is the case in other regions. (Indeed, "lit par lit" injection itself would appear, *a priori*, to be possible only among hot rocks, as seams so thin would soon consolidate among cold rocks, and so fail to make their way for any distance.)

If, now, we suppose a granitic magma introduced among rocks with a pre-existing temperature scarcely lower than its own (it might be even higher if the rocks were less fusible), not only would much more intimate intrusion be possible, but even when actual intrusion ceased the granite sills and the adjacent rocks would retain a high temperature at the junction for a very long time. The conditions, in fact, would be related to those of ordinary intrusion somewhat as those of a column of liquid lead poured into a hot cylinder on to hot gold would be to those of lead poured into a cold cylinder on to cold gold. Solidification would be long delayed, and all this time the magma might reasonably be expected to diffuse into the surrounding rocks, following their natural divisional planes, and giving rise to all the phenomena of a permeation zone.

The singular fact that the inclusions of gneiss in granite, even down to the thinnest films, are so very seldom, in these zones, disturbed in

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position (ibid., fig. 4), affords some support for the view that the extension of the magma proceeded by quiet diffusion rather than by forcible injection.

An explanation would also be found for the occurrence of lenticles of granite in complete isolation from the parent mass.

The experiments quoted lead us to expect that diffusion might go on even after solidification, seeing that in such a complex a high temperature would be maintained for a long time, thus extending the permeation zone yet further, and in perhaps an even subtler manner. Indeed, what we know of the changes that have certainly gone on in solid rocks shows that the solid state is no obstacle to extensive molecular change.[1]

The principal difficulty would be this. The solids of the experiments were homogeneous, being pure metals, so that diffusion took place between molecules of only one kind on either side. Whether the liquid magma of a granite was a completely homogeneous liquid we do not know, but certainly after solidification no granite is a homogeneous solid. Molecules of at least three kinds would therefore, it would appear, have to diffuse in order to convey granitoid matter from place to place, and that in due proportion. This is certainly a difficulty, though not an impossibility.

Moreover, are we quite sure that solid diffusion would be obliged to proceed in this way? At the close of the paper by Mr. Horne and myself to which reference has been made, it was pointed out (ibid., pp. 647–8) how little is known of the chemistry of the compounds of silicon, and how very much may be hoped for from an extension of that knowledge, when we consider the chemical analogies presented by that element and the part which it plays in Nature.[2]

V. — Vein-Quartz and Sands.

By A. R. Hunt, M.A., F.G.S.

SOME time ago my friend Mr. Jukes-Browne asked me to examine some sand, with a view to ascertaining whether it was derived from Dartmoor. Dartmoor quartzes have so many specific characters that it is often easy to say that a quartz is not derived from that region; but owing to the fact that quartz-veins have not been studied, it is usually impossible to say whence various sands have in fact come.

In the course of conversation, Mr. Jukes-Browne suggested my submitting a short letter to the Geological Magazine, as the subject might interest students; but the more I looked at the matter the more abstruse and cumbrous did it appear; and, from past experience, I doubted whether the inquiry would not be more attractive to chemists than to geologists.[3]

[1] Hitherto we have been under the necessity of invoking the agency of percolating water.

[2] My friend Dr. Horne very kindly read the MS. of this paper, and he gives me leave to say that he agrees with the views expressed in it. Indeed, I believe it would be nearer the truth to say that he had come to similar conclusions *before* he saw my MS., and had discussed them with my former colleagues of the Scottish Geological Survey.

[3] I was unaware at the time that quartz-veins were under discussion in the Magazine.

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