difficulty which it is only fair to admit: the "semicrystalline" division includes, at present, two marked varieties—rocks where the matrix is cryptocrystalline, and those where it is microcrystalline. The former, no doubt, is sometimes the result of devitrification, *i.e.*, of a secondary change. Is it always so, and if not, can we distinguish those where it is a secondary change from those where it is an original condition? and ought we not to separate the cryptocrystalline from the microcrystalline group? To these questions we cannot at present give an answer, but I see no reason why, with patience and labour, we should not ultimately be able so to do. At any rate, this method of division seems to me to be more manageable and open to fewer objections than any other.

Each rock, then, of a given chemical constitution may occur, and probably does occur, in each of these three conditions, glassy, semicrystalline, and crystalline; but we shall notice that, as might be expected, the glassy and semicrystalline varieties occur more commonly and in larger masses among the acid than among the basic rocks. Some readjustment of names will be required by this, notably in the case of dolerite and basalt—the latter often being under the microscope, as truly crystalline as the former. The phonolites must be subdivided, and all the nepheline and leucite rocks revised. The peridotite group also, as materials accumulate, must be more carefully studied. Still, though there are at present not a few difficulties, and there always will be some, because nature herself refuses to be fettered, I believe that the system of classifying primarily by composition, and secondarily by structure, is far better than any other which has yet been devised.

ON A NEW THEORY OF THE FORMATION OF BASALT.

BY PROF. T. G. BONNEY, M.A., F.R.S., F.G.S., &c.

In this paper I propose to examine a theory advanced by Mr. C. T. Clough in a communication to the "Geological Magazine" for October last (Dec. II, Vol. vii, p. 433), entitled "The Whin Sill of Teesdale as an Assimilator of the Surrounding Beds." This theory seems to me so improbable, and so opposed to what I regard as established facts in petrology, that I cannot refrain from criticising it, though always an unwelcome task. I know, however,

from past experience that upon some geologists any theory of the metamorphic origin of rocks commonly called igneous exercises a species of fascination, and thus, if unchallenged, it is certain to find its way into text books and to add to the mental confusion of unhappy students of a subject already difficult enough. I have not, indeed, had the opportunity of examining the Whin Sill in the field, though specimens from it and the adjoining rocks have passed under my notice, but I have paid especial attention to the contact phenomena of igneous rocks, and particularly of dolerite and basalt, so that on this occasion I venture to depart from my usual practice of visiting a district before I write upon its petrology. I trust, then, to be able to show that the direct arguments advanced by the author in favour of his theory are inconclusive, and that it is so contrary to all we can learn from other quarters as to require the most indisputable proof before we are justified in accepting it.

For a brief enunciation of this theory I will quote the author's own words: "It seems to me that we shall be forced to conclude that the Whin consists in part of altered sedimentary beds; that it partly represents beds which were once in the position it now occupies; that it did not make room for itself simply by thrusting aside these beds, but also by incorporating them into itself" (p. 484).

The author's arguments may be classified as follows:—

- (1) Field evidence in favour of his view.
- (2) Corroborative evidence from other districts.
- (3) Consideration of objections.

These I propose to deal with separately, before enumerating the facts which appear to me diametrically opposed to it.

(1) The cases instanced by Mr. Clough, though, no doubt, curious, do not seem to me at all conclusive in favour of his view. There is, he says, "a general absence of mechanical disturbance" of the surrounding beds; that is, the beds end abruptly against the Whin without any evidence of disturbance, such as faulting, bending down towards the intrusive mass, &c. He admits, however, that commonly the "Sill" is not seen included between the beds through which it has broken; and I cannot see that those cases which perplex him differ materially from many that I have observed, where it is hardly possible to assume a theory of assimilation.

For example, let us take the case of the vertical dykes of Strathairn* (Skye). Any one who has studied these in the field will, I think, admit that any assimilation theory is inadmissible. The basalt has broken through only slightly altered sedimentary beds in an almost countless number of dykes, generally a very few feet wide. The explanation seems to me simple—a great mass of basalt making its way to the surface has uplifted the overlying beds into a very low arch. These, unable to undergo much lateral tension, have cracked into a number of fragments like the separate blocks in a paved floor, and the basalt has welled up through the intervals. Now, it does not follow that these cracks should in all cases be vertical—even here they are not always so; but still in any case the cracks would gape in the same way, and there need not be any great mechanical disturbance of the ruptured strata. Basalt is seen in other parts of the coast to occur as intrusive sheets between the jurassic strata without much disturbance; yet in these comparatively modern deposits we can hardly expect assimilation, as there is no evidence of the adjacent beds having been long kept at a high temperature. The annexed diagram (Fig. 1) from a quarry

FIG. 1. BASALT INTRUSIVE IN THE CALCIFEROUS SANDSTONE SERIES W. OF BURNTISLAND HARBOUR.

near Burntisland shows how a mass of basalt can inject itself without materially disturbing the adjacent beds. Perhaps Mr. Clough will say that in this case also assimilation has taken place. I can only reply that the boundary of the basalt (now in the condition of "white trap") is perfectly sharp, even where the sheet is no thicker than the hand; that the adjacent rock is but little altered,

* Figured by Macculloch. Western Isles, Pl. xvi.

so that the basalt cannot have been at a very high temperature ; and that there is no reason to suppose the conversion of the basalt into white trap is due to any assimilation. The shale into which the basalt intrudes is a little squeezed and indurated—that is all. But when we consider that very probably all these rocks were more plastic and moist when the basalt was injected than they now are, it is easy to understand that the basalt might be compressed—as paper is in a bound book or textile goods packed by a Bramah press—without any conspicuous indication of the squeezing. The second diagram (Fig. 2) gives another instance, where compression is more

FIG. 2. BASALT INTRUSIVE IN CALCIFEROUS SANDSTONE SERIES, WHINNYHILL, NEAR BURNTISLAND (FIFE).

distinctly marked. I grant that I have sometimes seen instances where it was difficult to understand exactly how the rocks had been displaced, but we must remember that generally we have to explain the disturbances in a solid mass from a single plane section, and, therefore, need not be surprised at occasional difficulties, so long as we meet with numerous cases where the process is perfectly obvious.

Mr. Clough's strongest piece of evidence seems to me inconclusive. "Fortunately," he says, "we have in a few places much more distinct evidence than mere absence of disturbance to show that certain beds have been absorbed by the Whin. Here are a few sections which show at once the bottom and top of the Whin and the beds above and below, and so clearly that we may with certainty recognise these beds, and we can see in such cases that certain beds are missing where the Whin now is" (p. 437). The "best sections" are Cockle Syke, Rowantree Syke, and Lodge Gill Syke. The first two of these (400 feet apart) Mr. Clough

gives in detail,* pointing out that in the first two there is about 80 feet interval between two marked beds, in the one without any Whin, in the other, the same, including 21 feet of Whin. The coincidence is curious, but then to complete the proof we ought to find that the intervening beds also should be in part identifiable, which they are not. Is it not more probable that there is a thinning out? This, at the rate of 1 in 20, might easily be overlooked in the field where the intervening section is not well exposed. Mr. Clough says the evidence in Lodge Gill Syke is less conclusive, but I may remark that it at any rate proves the variability of the beds.

(2) As regards the corroborative evidence, this, on Mr. Clough's own showing, is not very strong where it does not rest upon insufficient authority. The basaltic rock will be found to lie amongst beds which are readily compressible, as mentioned above, or are easily reduced in volume, as coal. One case "of assimilating power derived from dykes," which is subsequently quoted, I have myself seen, and cannot admit the accuracy of the statement.

(3) I pass now to Mr. Clough's consideration of objections. To the obvious one that the Whin Sill is a normal dolerite or basalt, and thus shows no sign of that variation which we might expect in a rock which had digested at one place a sandstone, at another a shale, at a third a limestone, he replies: "There may have been a very general circulation, both on a large scale and molecule by molecule, reducing all the parts of the mixture to a uniformity of composition. The very possibility of forming alloys, and of modifying the properties of metals by adding to them small portions of other substances, depends on this principle of circulation or diffusion." The last remark is doubtless true, and the argument might be urged if we were dealing with a great mass of igneous rock which had for a long time remained at a very high temperature, but I think the analogy wholly fails here.

* This is Mr. Clough's section in ascending order:—

<i>Cockle Syke.</i>	<i>Rowantree Syke.</i>
<p>a. Limestone. 30ft. shale, with occasional girdle beds; 4in. coal at top. 3ft. shale, with girdle beds. 14ft. sandstone. 30ft. strong sandstone seen near the top; signs of a little limestone coming in about 12ft. from the top.</p>	<p>a. Limestone. 3ft. calcareous shale. 14ft. altered shale. 21ft. Whin. 25ft. strong sandstone rests apparently directly on the Whin, then strong girdle beds and sandstone; no shale. 5ft. limestone. 12ft. sandstone, thin flags in places.</p>
b. Limestone.	b. Limestone.

The rock in contact with the sill has evidently had its temperature locally raised by heat derived from the intruder. The latter, then, must soon have assumed a viscous condition, as is usual in flowing lava, and this would be unfavourable to diffusion. We must also remember that the volume of the basalt is supposed to be about equal to that of the missing rock, so that the basalt-making fluid must either have borne only a small proportion to the digested matter or have been little better than impregnated water. In the former case it would soon have lost its heat; in the latter we may fairly ask the author to give us some parallel of the existence of this "universal solvent" among known natural substances.

Mr. Clough attempts to strengthen his argument by estimating from analyses the mean composition of the various rocks which the basalt seems to replace. I confess that to myself the result, when compared with those given of the Sill itself, only increases the difficulty.* The solvent fluid must have contained alumina,

* The following are the analyses of a specimen of Whin (I.) from Tinkler's Syke, (II.) from Twards Bridge:—

I.				II.			
Silica	...	...	51.47	...	...	...	50.35
Alumina	...	...	16.48	...	...	...	16.80
Protoxide of Iron	...	...	8.49	...	...	...	8.36
Peroxide of Iron	..	...	3.61	...	...	...	3.51
Protoxide of Manganese	...	...	0.46	...	...	...	0.41
Lime	...	...	8.22	...	...	...	9.01
Magnesia	...	...	5.10	...	...	...	5.73
Potash	...	...	3.28	...	...	...	2.87
Soda	...	...	1.18	...	...	...	1.07
Iron	0.08	} Iron	0.17	...	{ 0.037	} Iron Pyrites	0.08
Sulphur	0.09			...	{ 0.043		
Water	{ Hygroscopic	} 0.50	1.70	...	{ 1.20	}	2.00
	{ Combined...			1.20	...		
			100.16				100.19
Specific Gravity...			2.82				2.84

The following is the estimated analysis of the average sedimentary rock:—

Silica	...	...	...	48.206
Alumina	...	...	...	9.835
Iron Protoxide	...	...	...	1.832
Iron Peroxide	...	...	...	.612
Protoxide of Manganese	...	...	...	.042
Lime	...	...	...	19.332
Magnesia	...	...	...	.957
Potash	...	...	...	1.188
Soda	...	...	...	.117
Water	...	...	...	2.883
Phosphoric Acid	...	...	...	0.36
Organic Matter	...	...	...	.116
Carbonic Acid	...	...	...	15.477
Sulphuric Acid	...	...	...	0.66

And should be of specific gravity 2.62.

iron, and magnesia, as all these are notably deficient in the mean rock, and it must have had the power of disposing of a considerable quantity of lime, as this is in excess. Perhaps Mr. Clough will instance the cases of greisen, zwitter-rock, and tourmaline-rock, but in none of these is there anything like a complete melting down, only a metamorphism (commonly very local) of a portion of the constituents by hydro-thermal agency; the contact phenomena also are very different.*

I proceed next to consider the evidence adverse to Mr. Clough's theory, by propounding the question, "Do igneous rocks, as a rule, exhibit any indication of possessing this assimilating power?" I do not, indeed, deny the possibility of such a thing happening at **very great depths; there, no doubt, by the action of heat, water,** and pressure, sedimentary beds *may* be melted down and mingled so as to be indistinguishable from igneous rocks. But still I assert that such evidence as we have obtained is opposed to it where the rock is making its way upwards through sedimentary strata. Now, we should expect to find the best indications in the case of the coarsely crystalline rocks, because it is probable that they and the adjacent rocks have been kept for a long time at a high temperature. It is, I think, needless to describe in detail the cases with which my note-books are filled; a simple enumeration will suffice. I have, then, repeatedly seen fragments of schists and quartzites in granites (Scotland, Cornwall, Auvergne, and many parts of the Alps), and in gabbro (Cornwall), and have examined the junction surfaces of these rocks with these granites and with syenites and diorites.† Of these I have seen scores of instances where the junction-surface, as a rule, is perfectly clear and well defined, even under the microscope. A later granite or gabbro does not appear to melt down an earlier one through which it breaks, nor do gabbros nor granites appear to assimilate serpentine.

* I cannot believe it possible that angular fragments with perfectly sharp boundaries (even under the microscope) can be undigested residues. Mr. Clough quotes my lamented friend, Mr. Clifton Ward, as favourable to the view that an altered rock may have a sharply-defined margin. After a fairly large experience, I can only say that I have seen nothing to favour and much to oppose this idea, and, after examining the country, which he so admirably described, was unable to admit that there was any valid evidence for his view, and felt that in this one case even he was unable to free himself from that confluent metamorphism which seems so infectious among the members of the English Geological Survey.

† I have omitted serpentine because, perhaps, some geologists would still dispute its being an altered peridotite.

In short, among the coarsely crystalline rocks I have never had the least reason to suspect anything more than the slightest possible amount of surface fusion or welding. Regional metamorphism there may have been, but I speak now of contact effects.

Proceeding, then, to the finer-grained dolerites and basalts : I have examined intrusions of these into rocks of like nature, and into granite, syenite, gabbro, serpentine and felstone, often without the slightest indication of melting or of more than at most a mere surface fusion, if, indeed, it deserves that name. For instance, the dolerite of Salisbury Craig does not show the slightest sign of having eaten up the beds through which it breaks. Dolerites and basalts may be seen in numerous cases on the Fifeshire coast at various places, breaking through shales, sandstones, and limestones with perfectly clear, sharp, well-defined junctions, frequently including fragments of these rocks, sometimes in abundance. Near St. Monance a basalt is full of fragments of a rather coarser basalt, very much as a diorite (?) mass near Christiania is crowded with pieces of gneiss.* The columnar basalt of the Gross Weilberg (Siebengebirge) abounds in fragments (often about the size of a walnut) of the trachytic tuff into which it has intruded, and has been unable to assimilate. In all cases the included fragments, though more or less altered, like the surfaces of the same rock at a junction, retain much of their original character, and are rather "baked" or "stewed" than fused. But it is needless to continue to multiply instances.† I have studied the demeanour of igneous rocks near their junctions in hundreds of instances, and in many districts—as, for example, in Cornwall, Wales, the Lake District, Fifeshire, and other parts of Scotland, in our own island : in Auvergne, the Pyrenees, the Bernina Group, the Fassa Valley, and elsewhere in the Alps, and in various parts of Germany and Italy, without seeing anything to confirm, and much to disprove, this idea of a possible assimilation of sedimentary beds by a solvent mass, which has subsequently crystallised so as to be indistinguishable, even in its microscopic structure and chemical composition, from ordinary dolerite or basalt.

* Lyell's "Elements of Geology," p. 683. I have seen the place myself.

† I may venture to predict that when Mr. Clough has worked for a few years with the microscope at rock structure generally, he will be convinced that even a moderate-sized grain of quartz requires nature to use a very strong "gastric juice" in order to digest it.