DISCUSSION

This department has been established by the editors in order to afford to those interested in questions relating to economic geology an opportunity for informal discussion. Contributions are cordially invited either in the form of discussion of more formal papers appearing in earlier numbers or bearing upon matters not previously treated. Letters should be directed to the Editor, Sheffield Scientific School of Yale University, New Haven, Conn. The full name of the author should be attached to all communications.

THE ORIGIN OF SECONDARY SILICATE ZONES.

DISCUSSION OF PAPER BY W. L. UGLOW.

Sir: In his original paper on the origin of secondary silicate zones at the contacts of intrusives with limestones,¹ Mr. Uglow divides the minerals of the contact zones into two groups. Group 1 contains garnet, epidote, diopside, tremolite and other silicates most abundant in these zones; group 2 contains in addition to quartz, muscovite, albite and the metallics only those minerals that contain "mineralizers." He then limits the phrase "secondary silicate zone" to the phase characterized by minerals of the first group (p. 26), and proceeds to argue that the minerals of this "secondary silicate" zone are the result of recrystallization of the impurities of the original limestone (p. 222 and p. 233). He admits that minerals containing such elements as boron, fluorine and beryllium involve contributions from igneous sources, but from a careful re-reading of his paper I am unable to find any indication that he attributes the formation of garnet, diopside, etc., to igneous emanations, and it was upon that point that I took issue with him.

In a recent reply to criticisms by Professor Kemp and myself, he says:

"The list of minerals in group 2 should of course be enlarged to include some garnets, pyroxenes, amphiboles, etc., which are sometimes

¹ ECON. GEOL., VIII., pp. 19-50 and 215-234.

the products of the second phase of contact action. Undoubtedly in the great number of cases of contact action that have been described, the two processes have varied greatly in their relative strength and influence. Indeed, when the second, or infiltration process, was exceedingly potent, the materials emitted may have largely replaced and so destroyed the results of the first process."

These two statements involve a great change from the contention of his first paper as I interpreted it. With the re-stated views that garnets, amphiboles, and similar minerals may be the result of both processes, and that the relative importance of the two processes may vary in different cases, I am in entire accord. In fact this is exactly the conclusion of my original discussion.

The specific point of difference now remaining between us is the interpretation of the analyses of contact zone material from the San Jose, Bisbee, White Knob, and other localities discussed in the original paper. In that article Mr. Uglow states that the "ratio of $\text{SiO}_2:\text{Al}_2\text{O}_3:\text{Fe}$ in the unaltered rock is very nearly identical with their ratio in the unaltered type" (p. 220). In reply to my criticism of that statement, he writes:

"With respect to the differences shown by potash, soda, magnesia, lime, water, and carbon dioxide ratios, there certainly is an approximate constancy in the silica, alumina, and iron ratios."

From the standpoint of the recrystallization hypothesis the bearing of this statement upon the disputed question of the origin of the secondary silicate zone is as follows: Lime, magnesia, alkalis, etc., are removed in the process of metamorphism in amounts depending upon their varying solubilities under varying conditions of combination, change in composition of leaching waters, etc.; therefore their ratios show great variation in the altered rock. Silica, iron and alumina in great part remain behind and are recrystallized; therefore their ratios remain more nearly the same.

"It would indeed be a remarkable coincidence if the silica, iron, and alumina were each time added in almost exactly the same ratio as that in which they were present in the unaltered rock."

In view of the preceding, it seems evident that the essential point is not whether the silica, iron, and alumina ratios vary

more or less than other sets of ratios, but *whether or not they vary at all*. In my first discussion I showed that the ratios in question did exhibit a variation of from 20 to 700 per cent. I still believe that such a variation is appreciable, and that it does not accord with the hypothesis that the iron, alumina, and silica represent the original impurities of the limestone left in practically unchanged proportions. The fact that lime, magnesia, water, and carbon dioxide show greater differences means to me that the removal of these substances in the process of replacement or volatilization involved greater changes in the ratios than did the magmatic additions; it does not prove that these latter changes were not considerable.

We must remember also that the changes in the ratios are in *different directions*, in some cases the silica having increased relatively to the alumina and iron, in other instances the reverse being true. At Iron Springs the ratio of silica to alumina has *increased* nearly a third; in Hastings County the same ratio has *decreased* the same amount; at Iron Springs the ratio of alumina to iron has *decreased* over 50 per cent., while in Hastings County the same ratio shows a slight *increase*. The ratio $\text{SiO}_2/(\text{Al}_2\text{O}_3 + \text{Fe}_2\text{O}_3)$ varies as follows: Bisbee, an *increase* of about twenty five per cent.; White Knob, a *decrease* of over 60 per cent.; China cello, a *decrease* of 20 per cent.; San Jose, a slight *increase*; Morenci, a *decrease* of over 700 per cent. Therefore iron, silica and alumina in the metamorphosed rocks show not only a different ratio, but they show in some cases an increase of one constituent, and again an increase of another. I believe such a condition more easily explained on the hypothesis of magmatic additions in varying proportions than upon the re-crystallization hypothesis.

Mr. Uglow makes much of the fact that the analyses quoted are incomplete, or were not taken with the view of distinguishing between the first and second stages of metamorphism. What analyses of different samples *might* show is beside the point. The original paper maintained that as they stand these analyses indicate the re-crystallization hypothesis as the most favorable

explanation. It may also be urged that under the revised views stated by Mr. Uglow some of the discrepancies can be explained as due to two processes being important. That of course involves no progress, for it is admitted by all advocates of magmatic additions that whatever impurities were present in the limestone have gone to make up the contact silicates in part.

I disclaim all intention to be unduly controversial, particularly since Mr. Uglow has so courteously replied to my rather blunt criticisms of a very painstaking piece of work, but I cannot forego a reply to the implied accusation that I misinterpreted the original paper in assuming that it did not allow magmatic waters a part in the formation of garnet zones. In this connection Mr. Uglow quotes from his original paper as follows:

"This view (recrystallization hypothesis) does not by any means involve a total absence of infiltrated material from the intrusive, but its advocates maintain that the so-called contact zone can be lithologically divided into two parts, one of which is formed by recrystallization in the early stages of metamorphism, and the other part by later addition of material in the way of emanations from the magma."

If he had continued this quotation to include the next sentence, he would have added:

"The minerals which belong to the first class are those mentioned in the left hand column on pages 7 and 8, while those of the second class include, among others, those listed in the right hand column."

In view of the fact that garnet, epidote, pyroxene, and other common contact minerals were definitely included in the minerals of the first class, and nowhere mentioned as minerals of the second class, I believe that I was entirely justified in interpreting Mr. Uglow to mean that garnet, epidote and similar minerals are the result of recrystallization alone, and that only the minerals containing mineralizers or associated with minerals containing mineralizers belong to the second class. This belief was further strengthened by Mr. Uglow's treatment of the various districts. Localities that have become classic as instances of magmatic additions—if magmatic additions be conceded at all—were treated as most in accord with the recrystallization hypothesis.

C. A. STEWART.

SECONDARY SILICATE ZONE.

DISCUSSION OF PAPER BY W. L. UGLOW.

Sir: Mr. Uglow's letter in continuing the discussion of the interesting question of the formation of the lime-silicate contact zones, and their associated deposits of minerals, is expressed in the best of spirit and will be unreservedly welcomed by all to whom this subject has appealed. It is unfortunate that the controversial portion of his first paper was so prominent as to render too inconspicuous the very important points now more clearly brought out. Sympathizers with views which were pretty severely attacked, and, as it seemed to them, sometimes on rather far-fetched grounds, could hardly be blamed for coming to their defence.

All closely observant students of these phenomena have noted the succession of the minerals in time of formation. The line is not absolutely drawn between the silicates and the ores, but the contrast is clearly observable and the distinctions have been recorded by almost all who have written of the phenomena. The explanation of this succession as due to stages of different influence on the part of the intrusive mass, first of heat effects without pronounced emissions; and later of emissions with contributions, about whose relative importance and character we may still differ,—the explanation, one may repeat, is of great interest and is worthy of careful reflection and of trial application. Nevertheless, the objections on the chemical side, advanced by Dr. Stewart and the writer, seem still so difficult to meet, that one can hardly admit that they have been successfully overcome. Without repeating them here, they may be commended to those studying and interpreting the phenomena of contact metamorphism, as considerations not to be lightly overlooked.

J. F. KEMP.