THE MAKING OF A MAGMATIST

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Abstract

Professor Larsen's studies of lavas, minor intrusions, and batholiths in the San Juan region of Colorado and in Southern California are presented as the basis of his development of magmatist views. Anti-magmatist views on the formation of batholiths are discussed, and also the magmatist view that batholiths are formed by refusion in a tectogene.

He probably scorns a label, preferring to be regarded as one who keeps an open mind, as indeed he is (Larsen, 1950), but it is no great distortion of the truth to say that Professor Larsen is a magmatist. He would not, at least, regard this designation as a term of contempt, nor should he, for he acquired magmatist views as a result of long years of arduous field studies in difficult but rewarding terranes and of equally arduous investigation of materials and collation of facts gleaned in the field.

The San Juan

In 1909 Larsen joined Whitman Cross in a comprehensive study of the geology of the San Juan region of Colorado where Cross had already been working for fifteen years. During eighteen additional years, with some interruptions occasioned by the first World War, field investigations were continued by Larsen, partly in association with Cross, but with many other associates as well, the most notable of whom was C. S. Ross. Many publications, including seven folios, had been written by Cross, and in 1935 appeared a "brief review" of the geology of the region written by Larsen (Cross and Larsen, 1935).

The San Juan region was found to be one in which volcanic activity, dominantly of Tertiary age, was very prominent, indeed most of the mountains of the region have been carved in great accumulations of lavas and ejectamenta, extruded at intervals throughout a long period of time. The lavas range from basalts through several varieties of andesites, latites and quartz latites to typical rhyolites. There is no simple order of eruption. Several volcanic series are distinguished, each of which includes a wide range of compositions and in some instances they were separated from each other by intervals of erosion.

It would seem impossible that anyone would deny that these lavas represent igneous magmas, and it is to be hoped that anti-magmatists are merely being facetious when they state that the existence of granitic magma is based entirely on assumption. Larsen, at least, and all of his associates, without exception or reservation, concluded that the field evidence was decisively in favor of the formation of these rocks from molten magmas.

Magnificent studies of phase petrology by Professor Larsen and associates (1936, 1937, 1938) have led them to some very definite conclusions as to the courses of crystallization in the various types of lava and in the series of lavas as a whole. These indicate that a prominent control over the derivation of the series is fractional crystallization of liquid magma, but assimilation of foreign material and actual mixing of magmas are also suggested by the observations.

In addition to the lavas of the region there are bodies of rock showing the same range of chemical composition as the effusive lavas but having forms and relations to their surroundings which lead to their description under the accepted terms dikes, sills, sheets, laccoliths, and stocks. These yield varying degrees of evidence of having been insinuated into existing rock masses. Larsen and associates have thus been led to regard them as intrusive rocks of magmatic origin. In the smallest of bodies, as for example in thin sheets or sills, these rocks may differ scarcely at all, in type of crystallization exhibited, from their chemical equivalents among the lavas. Thus some of the sheets are, like the similar lavas, referred to as andesites, but in larger masses the degrees of crystallinity and of granularity may depart notably from those of the lavas and typical granular rocks (diorite among others) are found in some larger stocks and laccoliths. The passage from the one extreme of partly glassy rocks to holocrystalline granular types is so well exhibited that the crystallization of all of the masses from igneous magmas analogous to the lava flows has been accepted by Larsen.

The conclusion seems reasonable, but as soon as we move down more than a foot or two from the surface anti-magmatists begin to balk at the suggestion that rocks in these situations were formed from magmas. It must be supposed therefore that some of them, at least, would regard these hypabyssal minor intrusions as formed by some variety of metasomatic replacement. What evidence they might be able to adduce in favor of a replacement origin of the masses is a question that must remain unanswered here. It is to be noted, however, that at the borders of the masses replacement effects are to be found locally and some valuable ore deposits have been formed in connection with these effects, but that these relations can be taken as an indication of the replacement origin of the "intrusive" itself is a view which Larsen would regard as unconfirmed by the field evidence, as would a great many others who have investigated the ore deposits and their relations and who were intensely interested in problems of metasomatic replacement.

Rather they regard the evidence as favoring the concept that igneous magmas have come from still greater depths in the earth and have formed the hypabyssal intrusives and the lava flows. Speaking of the stocks, Cross and Larsen (1935, p. 126) say that "they have a closer connection with the deeply buried parent magma chamber."

Do we have any clue as to nature of such materials at greater depths?

Southern California

Even before he began his work in the San Juan region Professor Larsen, as an assistant to W. C. Mendenhall, had in 1906 studied a part of the batholith of Southern California, and in 1930, the same year in which he made his final corrections to the field studies in the San Juans, he began a systematic study of this batholith. As formerly, he had a number of collaborators, the field investigations extended over several years, and he was able to incorporate in his report the observations of others not directly associated with him in his investigations. If his conclusions are correct, the batholith, though most remote from the San Juan intrusives in both space and time, may nevertheless be regarded as taking up the story of igneous action about where the San Juan intrusives leave off, but perhaps with significant overlap, for there are minor intrusions satellitic to the main batholith, itself a complex of intrusives. These minor intrusions may be likened to the stocks and laccoliths of the San Juans. The main batholithic complex may then be something of the nature of a magma chamber analogous to that which supplied the magmas of the San Juans.

The batholith of Southern California is itself presumed to be derived from a still deeper magma chamber. Larsen (1948, p. 171) says "A magma of relatively uniform gabbro was formed at depth in the core of the ranges. This gabbro was slowly differentiating, having at any time in its history an upper part essentially the same throughout the length of the batholith as to chemical composition and kind and amount of suspended crystals. From time to time diastrophism moved the magma toward the surface. . . . After emplacement of the gabbro, little further movement took place until the upper part of the magma had the composition of a tonalite. Then many local and several widespread movements emplaced the several tonalites. . . . After intrusion of the tonalites no widespread earth movement took place until the upper part of the remaining magma reached the composition of a granodiorite. The Woodson Mountain granodiorite was then injected over much of the length of the mountain range. The small volume of granite in the batholith may result from a scarcity of residual granite liquid or from the fact that during the granitic stage diastrophism was not favorable to movement of the magma toward the surface."

The picture he paints of an order of intrusion related to and controlled by an order of crystallization is supported by a wealth of data on phase petrology. He thus finds that, in determining the relations between the various rock types, the same factor, crystal$\rightleftharpoons$liquid equilibrium, has been in control.

Comment

The above is in skeleton outline the story of the making of a magmatist. Though it spans a period of forty-five years it by no means presents or even suggests all the geological phenomena and regions studied by Larsen. Wide and varied has been his experience, yet it appears to have served, on the whole, only to yield more evidence in favor of magmatism.

It is earnestly to be hoped that no one will make the insulting suggestion that Larsen heard about magmas from his teacher but has never given the subject any real thought himself, a suggestion that has been made of another investigator not less experienced and distinguished than Larsen. Daly (1949) has recounted, in his inimitable manner, the story of how he himself, as a young man, diligently exposed himself to all schools of thought on eruptive rocks, yet turned out a magmatist. To be sure antimagmatists tell a similar story of how they have reached their convictions, but it would be most desirable if they would refrain from adding the suggestion that the magmatist had no field experience or failed to think about what he did see in the field.

The question might be raised as to whether Larsen would have been a magmatist if he had studied the California batholith before he studied the San Juan lavas. Actually he did have some prior experience in the California field, but it was relatively limited, and the question is, of course, one to which there can be no reply, nor can it be profitably discussed.

A more useful question is one that asks whether his conclusions regarding the California batholith will withstand the test of time. To this question only time can give a final answer, but it can be profitably discussed, for it is such discussion, and investigation arising from it, that will enable an answer.

In his conclusions, set forth above, Larsen has shown himself to be not only a magmatist but, as far as the California batholith is concerned at least, a special brand of magmatist, one who regards the evidence as pointing to the parental nature of basaltic magma, a view of which Daly has long been the leading proponent.

We shall discuss the various possibilities by turning first to the view at the other extreme. The batholith of Southern California was, let us say, emplaced by solid diffusion. No liquid or fluid of any kind was involved in its formation. Atoms or ions diffused through crystal lattices and along intergranular boundaries under the influence of some assumed potential or driving force, and built up the mass as we find it. Apart from the convenient assumptions already made that this process would be effective at all, on what basis are the additional assumptions made that it would give associations of crystalline phases (rock types) that so closely imitate the results that would be obtained if the control over these associations were crystal-liquid equilibrium?

There is also the objection based on the existence of zoned crystals in the Californian batholith. A chemical potential exists in such crystals tending to make them of uniform composition, but it has frequently been unable even to blur boundaries between zones which would have required diffusion only through hundredths of a millimeter for their complete merging. Yet the ionic diffusion must take place through distances measured in kilometers in order to feed the assumed growth of the crystals by this method. The attempt to explain the persistence of zoning by assuming a lesser facility of ionic diffusion within the crystal lattice than in the intergranular film must be regarded as unsuccessful. No less an authority on crystal structure than Buerger (1948, p. 114) claims that it requires much more energy to cause an ion to escape from a lattice than to wander in the lattice. *Per contra*, then, an ion assumed to be already in an intergranular film will have a much smaller tendency to wander foot-loose in that film than to get back into the lattice, where it can then wander more freely. Even if this reasoning should prove erroneous, and there really is a greater freedom of wandering in the supposed film, it should be realized that this excess of mobility requires to be enormous, considering the enormous discrepancy between the distances involved, kilometers versus hundredths of a millimeter. This becomes increasingly apparent when it is realized that in diffusion effects it is the square of the distance that counts. No, solid diffusion is not a satisfactory way of getting batholiths with the mineral associations and the mineral textures they exhibit, and it is probably too slow a process to yield a mass of batholithic dimensions in the time that can reasonably be allotted to the formation of these masses, long though it may be.

Nor can it be said that the case for formation of a batholith through the agency of an ichor or mineralizers is significantly stronger. The question of time is probably not so critical here, for the bodily movement of material through the minute openings that may develop in a rock mass undergoing deformation, while undoubtedly slow, must be of an order of magnitude greatly in excess of that characteristic of ion diffusion. However, the same objections that have been raised against ion replacement apply here as well, in the matter of the actual petrographic character of the materials produced. A replacement process of any kind should be completely lacking in any tendency to produce an association of minerals in definite or approximately definite proportions. The feldspathization of a quartzite, for example, should go to completion, except in so far as it was accidentally interrupted. It would not characteristically cease while some quartz still remained, and as for coming to a close when there is established that nice balance between quartz, potash feldspar, and albitic plagioclase characteristic of the usual granite, that could be regarded only as a miracle of replacement (Chayes, 1950). The relative proportions of solid phases have no effect on chemical equilibria, the mass of a solid phase being constant whether it be present as a trace or in dominant amount. On the other hand, the relative proportions of the components of a liquid in equilibrium with solids is very definitely controlled, and granites tend to have a constant mineral composition because they are formed by the consolidation of liquids whose composition has been subject to this crystal-liquid equilibrium control. The liquid so prepared can drench surrounding rocks and thus granitize them, but this is a wholly subsidiary process.

The whole batholithic picture points to formation as a result of the intrusion and consolidation of liquid magma. Granitic magma has been shown with reasonable certainty by laboratory studies to be the normal residual liquid from the fractional crystallization of the polycomponent system of rock-forming oxides, a relation that is confirmed by observations upon rocks. But the very facts that indicate this relation for granitic magma offer equal support to the possibility that it would be the first liquid formed in the remelting of a heterogeneous massif. There are some magmatists who favor this view of the formation of batholiths. There is no doubt that Waters (1948, p. 107) had these considerations in mind when, emphatically rejecting replacement of any kind as the controlling process in the formation of batholiths, he proposes that they are formed by selective fusion of granitic gneisses and sialic sediments forced down to levels of high temperature in a tectogene. A process of this general character has been suggested by others as well. Let us consider the probable results of such a process.

If the temperature of the down-warped sial were raised to a value such that quartz, potash feldspar, and sodic plagioclase, together with such water as might be present, could form a liquid, that liquid would, of course, have these components in granitic proportions, for the control of crystal-liquid equilibrium over the composition of the liquid is just as effective in selective fusion as in fractional crystallization. The liquid so formed would have in considerable degree the characteristic of a constant temperature mixture, such as the mixture of ice and water used to obtain a constant reference temperature. So long as any crystalline material containing quartz and the feldspars remained the temperature could not rise. Heat might flow freely into the mass from hotter surroundings but it would be consumed entirely in producing more granitic liquid, without significant rise of temperature. There would therefore be a long period during which only granitic magma existed and all the earliest intrusive material would be granitic. In many batholiths it would probably be the only intrusive material. In others, as time passed, all the more fusible materials would be melted and the temperature would rise as less fusible materials were taken into solution. The liquid would pass to granodioritic composition and perhaps on to dacitic. That it might attain dioritic composition can be entertained, but that it could ever reach the gabbroic seems quite beyond the bounds of credence.

Waters probably had the Sierra Nevada batholith in mind when he made his suggestion, and that batholith is almost continuous with the batholith of Southern California. Larsen's findings in the latter batholith show very poor agreement with the deduced picture of a batholith whose members had their origin in a process of refusion. The earliest intrusions were gabbroic; they were followed by tonalitic and later granodioritic intrusives. Finally there was some granite, but only a little.

There is no escape from agreement with Larsen that basaltic magma was the parental material and that the later intrusives were produced from it by fractional crystallization, with some modification by assimilation of foreign materials.

It is not necessary to go so far as to assert that the action pictured by Waters never occurs. It probably does. A general survey of batholiths might reveal how great importance it may have. Knopf's (1948) survey does seem to indicate that there may be batholiths entirely of granite. How often do composite batholiths have the order of intrusion expected of the selective fusion process? Larsen says that the order he finds is the normal order. In making this statement he probably has most prominently in mind the Mesozoic and Tertiary batholiths of the western cordillera. Some have suggested that refusion was a more prominent feature in batholith formation in early geologic times, especially in the pre-Cambrian. It may be that support for this concept could be found by considering them with the questions just asked in mind. Nevertheless one would require to exercise caution. Any indication that might be found of absence of all rock types except granite in ancient batholiths might easily be more apparent than real. Old terranes have ordinarily suffered much retrograde metamorphism. Of all the possible batholithic rocks the higher temperature varieties are the more susceptible to such changes and might easily be changed sufficiently to escape recognition as facies of the intrusive. Granite, on the other hand, already a comparatively low-grade rock, suffers less under these conditions and remains recognizable as such. It would require a very careful survey of all the facts to answer the question whether ancient batholiths are really significantly different from those of more recent times.

While we are examining these questions that grow out of the larger question of the parental nature of basaltic magma, it seems desirable to discuss one fact that has been regarded as fatal to the concept that granitic magma can be derived from basaltic by differentiation. This fact is the absence of granite or rhyolite in the Pacific basin, and, according to recent indications, in much of the Atlantic floor as well. The answer is inherent in Larsen's description, already quoted, of the development of the batholith of Southern California. As the liquid changed slowly by crystallization from basaltic to more salic composition it was from time to time pushed up from below by mountain-building forces. If we add to this concept the probability that gravitative settling of crystals is not sufficient to give separation of crystals and liquid, but that filter pressing comes into play during the periods of diastrophism, it is possible to suggest why there are no granites or rhyolites in ocean basins. The mechanics of true ocean floors are probably not such as to permit the formation of intrusive masses of batholithic dimensions in the first place. Only the dike feeders of lava flows may be developed. In addition the mechanics characteristic of continental diastrophism may never prevail in true ocean floors. Consequently the filter-pressing effect is not freely available for the production of granitic differentiates, but when island-arc mechanics develop rhyolites appear to be developed also.

The story told by the San Juan and the Southern California phenomena and the light it throws on other regions suggest that igneous action is a unity. The fashionable cleavage between plutonism and volcanism probably has no objective existence.

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