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GRANITIZATION, METAMORPHISM AND VOLCANISM

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ABSTRACT. In Europe at least, the existence of granitization by replacement is now generally conceded, and present discussion concerns the existence of true magmatic granites, which appears to me actually to be an unnecessary hypothesis. The obvious removal of Mg and Fe in the granitization process, coupled with the absence of well-established large-scale basic fronts, strongly favors two-way diffusion in a stationary medium over circulation in a moving medium as the mechanism introducing and removing material. Following Ramberg, two-way diffusion may be attributed to minimization of the free energy of silicate structures by migration of alkalis into tectosilicates in the sial and of Mg and Fe into metasilicates in the sima. Such a diffusion theory holds promise of explaining not only granitization but also metamorphism, orogeny, and volcanism.

PRESENT STATUS OF THE GRANITE PROBLEM IN EUROPE

The conceptions of petrographers, in particular on the genesis of granites, have been altered greatly during the last twenty years; such evolution is general, but very uneven according to authors, and it has given rise to a rather too simple classification: the magmatists and the metamorphists.

What is the present state of these concepts in Europe? As a metamorphist from the onset (1934-1935) I am afraid I might be partial. I shall try, nevertheless, to give an objective outline, although this is no easy task. Works in Europe on the subject of granites are rather few and often essentially descriptive, without any reference to the genetic process; quite a few authors still seem to adopt the "wait and see" attitude, not knowing to which side the scale will tip.

I recently enquired from a colleague: "How would you sum up the opinion of European petrologists on the genesis of granites?" His reply was "I believe that, for many of them, the answer to this question depends more upon pyschoanalysis than upon logic."

I think he is right. Petrographers have long believed in magmas and magmatic differentiation alone, theories which were, in their opinion, entirely supported by experience and field study. New ideas resulting from precise observation have caused the whole question to be taken up again. The majority of authors have progressively recognized that there was some truth in those new ideas, but their boldness was somewhat disturbing. In particular, the intrusive appearance of certain granites or of dikes, which was the basis of Hutton's concepts (1785), has for a long time seemed to be an unquestionable proof of intrusion, therefore, of the liquid state of the granites.

In 1938 and 1939 our[1] statement, substantiated by evidence, that the intrusive appearance was not in itself a proof of intrusion and that replacement by metasomatism could lead to such appearances, met, we must confess, with a good deal of incredulity.

[1] To make reading easier, I shall use the word "we" every time articles written together with Roubault are mentioned.

Since then, we have cited other facts relating to granites. Along the same line, the works of Goodspeed (1940), King (1948), Phemister (1945), Higazy (1949), Misch (1949), Ramberg (1952a), and ourselves (1949c, 1952) on pegmatite and aplite dikes, have demonstrated their formation by replacement in a great many cases; as a result, the intrusive appearance is not put forward any more by certain leaders as evidence in favor of magmas; however, it would be contrary to the truth to say that some petrologists are not still impressed by this eruptive appearance. Such was still the case a few years ago when authors of metamorphist tendency, such as Read (1943, 1944), still seemed to think that the intrusive appearance was consistent with a liquid intrusion only. However, Read's metamorphism was largely based on the phenomenon of feldspathization which I used as a basis (1935) on which to reach the conclusion that the genesis of granite took place by replacement in the solid, and this commonly in the case of intrusive-looking granites.

Reynolds (1947a) and Backlund (1938), who were thorough transformists, also considered at that time, as an explanation for the intrusive appearance, a certain partial remelting after metasomatism, allowing relative displacements: this is Backlund's "rheomorphism."

The facts most in favor of a rheomorphism, at least local, seem to have been cited by Goodspeed (1952, 1953) and by Misch (1952); they are based on the presence, in some points of the rocks certainly formed by metasomatism, of euhedral crystals and of a flow structure. I do not deny the possibility of such a rheomorphism, but the facts cited do not seem to be quite conclusive evidence: 1) Feldspathization of the country rock generates "uncompleted" porphyroblasts, but also completed crystals and euhedrals, although no rheomorphism could take place in this case. 2) The flow structure resulting from an orientation of the mafic minerals often originates from a directed pressure; in the process of progressive granitization the non-existing or low changes in volume that ensue may cause directed or undirected pressures in adjacent places. There does not seem to be any other possible explanation for facts observed by us or for the inextricable mixtures granite-gneiss, the rapid succession in one granite body from non-oriented porphyroblast feldspars to oriented feldspars, etc.

In the many cases I have personally observed, the intrusive appearance of the granites seemed to find an explanation in metasomatism alone without plastic flow.

Some differences in concepts, at least partial, still seem to exist between metamorphists as regards the eruptive appearance itself.

Let us now try to proceed with psychoanalysis: for some European petrologists, the batholiths of homogeneous appearance seem to be accounted for by the liquid state phase only, replacement in the solid not being considered by them as likely to reach such a result, but this liquid state phase does not always imply in their minds differentiation and magmatic stoping. In France at any rate there is a decided tendency to come back to the old tradition of the French school: granite is the end product of metamorphism.

With respect to granites known as anatectic, containing many relicts of initial rocks, including sometimes important septa of those rocks that have retained their initial orientation, replacement in the solid is more and more admitted; the word granitization is now quite familiar to the mind and the language. As to the difficulty of considering now a liquid state, now a solid state, according to the homogeneous or inhomogeneous nature, in cases where a batholith is homogeneous and eruptive on part of its edge and has an anatetical appearance on another part, nobody seems to have raised the question.

I shall also point out the tendency of some authors in France to consider in some cases for the granite a state that is neither solid nor liquid but semi-fluid, pasty, for instance. This is sometimes associated with the argument that under very heavy pressures there may not be any more essential differences between the liquid and the solid state. I confess that here the word solid results in confusion with the properties of glass and that it would be better to substitute the word crystalline for the word solid, as, even under very heavy pressures, there can be no confusion between the crystalline and the liquid state. This pasty state has been assumed to explain the presence in some granites of so-called displaced xenoliths because the schistosity differs sometimes from one xenolith to another—facts cited by Jung and Roques (1952) and Cogné (1950), etc. We have studied such cases, including the one observed by Cogné, and have made very peculiar observations that have caused us to find there was, in such case, no opposition between the existence of xenoliths with differing orientation of schistosity and replacement in the solid. We have published a brief note (1952) and intend to develop this problem further in the near future.

Generally, with the exception perhaps of P. Eskola (according to Erdmannsdörffer, 1950), there seem to be no more petrologists in Europe who deny the existence of granites or parts of granitic bodies formed by replacement in the solid without remelting. Even the much regretted Paul Niggli (1950), who was a great theorist and an earnest supporter of the magmatic theories, has recently classified granites in three categories: magmagranites, metagranites proceeding from metasomatism without remelting, even partial, and ultrametagranites, formed by metasomatism with at least partial remelting.

The position that there are granites and granites, stated by Read (1944), seems to correspond to the psychoanalysis of a rather large number of petrologists, a position which allows at least the "wait and see" attitude, as well as adaption to all cases. Among the leaders, only Wegmann, Reynolds, Holmes, Backlund (1943) and ourselves seem to have denied the existence of granitic magmas capable of generating granites by direct crystallization from the liquid.

In the minds of the supporters of the concept that there are granites and granites, the magmagranites are chiefly admitted for the large batholiths. Has not in fact Grout (1941) reproached the metamorphists with reaching their conclusions from studies made on the rim and over small spaces without having studied large batholiths? The studies on the large Singo batholith made by King (1947), whose conclusion was definitely in favor of metamorphism, have created a strong impression and given rise to passionate discussions in the United Kingdom. The same applied to the metamorphist conclusions of van Biljon (1949) on the Bushveld complex which, up to that time, had been regarded as a typical demonstration of differentiation; Raguin (1949) has called attention to that study. The very interesting monograph by Misch (1949) on metasomatic granitization of Nanga Parbat and of the Sheku area does not seem to have been much studied yet on the Continent.

Generally, the supporters of "granite and granite" in Europe do not seem to take into much consideration the determination of the criteria that would allow them to distinguish the magmagranites and the metagranites, to use Niggli's terminology. This question has been raised, without any precise answer, at the Ottawa symposium. Drescher-Kaden (1948) has, however, distinguished the fabrics that would correspond, according to him, to a theoretical granitic fabric, which is distinct from the metamorphic crystalloblastic fabrics, the common occurrence of which he has found in the gneisses and granites. O. E. Erdmannsdörffer (1950) in one of the last European articles of magmatist authors on the whole granite problem, has stated objectively that this theoretical fabric was exceptional: "Die reine theorische Form allein ist kaum ja vorhanden." He has found, just as we have, in many cases, in intrusive granites, the presence of metasomatic fabrics, involving what we have called (1939) the corrosion between crystals, often called metasomatic crystalloblastic fabric. In his opinion, intrusive is evidence of liquid intrusion; therefore, to remove the objection resulting from the presence of these fabrics, he has produced a new hypothesis that he opposes to our personal concept: "Wenn echte metasomatisch Strukturen in einen geologisch als intrusiv nachgewiesenen Granit auftreten, so ist dies kein Beweis, dass diesen in seiner Gesamtheit rein metasomatisch entstanden sei." The hypothesis may be summed up as follows: after solidification and crystallization of a granite, the latter is subjected to a kind of autometamorphism in a stage called "endomagmatisch," corresponding to the end of the magmatic process; during that stage, under the influence of the supposed residual solutions, a metamorphic transformation of the granite takes place, resulting in the metamorphic crystalloblastic fabric.[2]

Observations of these crystalloblastic fabrics on granites, granodiorites, gneiss, etc., have been multiplied throughout the world. We have said that the granite fabric seemed to us characteristic of crystallization in the solid. Recently, Ramberg (1952a, p. 262) has written: "It seems apparent, on the ground of the fabric of granitic plutons, that either the large majority have recrystallized after their birth out of a melt or they originated entirely along a non-igneous path."

Very close studies of the porphyroblasts of feldspar of the gneiss, known as orthogneiss, of the Hohen Tauern in Austria, and of the relation between the porphyroblasts and the deepseated rock, have also led Exner (1948) to conclude that genesis was in the solid. He has also concluded that formation of aplite and pegmatite dikes was by replacement. The idea of orthogneiss,

[2] In fact, the metasomatic crystalloblastic fabric is more particularly pronounced along the edge as well as in the feldspathic areas isolated in the enclosing rocks. Often, more deeply inside, the feldspar crystals, for instance, seem to have cleared themselves of their inclusions, according to the illustrative expression of Misch; see also King (Singo batholith), etc. This structure corresponds, to my mind, to an uncompleted granitization and not to an autometamorphism of granite. It sets forth in evidence the progressive mechanism of granitization.

i.e. gneiss deriving from a previous magmatic rock by dynamometamorphism alone, if it has not been quite given up, tends nonetheless to become much rarer. Niggli has admitted that many gneisses, formerly regarded as ortho-, are in fact paragneisses or mischgneisses, the latter word being rather indefinite. Also augengneiss or eyed gneisses are less and less regarded as resulting from the deformation of a previous rock but as having originated directly in their present fabric.

As a matter of fact, the number of authors in Europe studying crystallization and the relationship of crystals between themselves, in order to gain some light on the genesis of rocks, seems rather limited.

I also want to call attention to a paper not yet published, from the mineralogist J. Wyart, presented at the Algiers Congress (1952); Wyart, who is not opposed to the genesis of granites by replacement but who, as a specialist in pneumatolytic mineral syntheses, believes in the predominant role of water as mineralizer, has expressed a new hypothesis: the granites would proceed from magmas consolidated in the glassy form and would then have been subjected to recrystallization in the solid under the influence of mineralizers. We have already admitted the possibility of such a process, recrystallization of rhyolites, for instance, but, in my opinion, in rare cases only; as, apart from the problem of emplacement that remains unsolved in such an hypothesis, this theory fails to explain feldspathization or the birth of isolated granitic spots in the enclosing rocks.

On the whole, the thinking of many authors on the genesis of granites results from a mixture of new and old concepts, the whole being accompanied by subjective impressions. There is at the same time fear because of the boldness of the new theories, and doubt regarding the old ones.

A question that has raised controversy, even among metamorphists, is that of the basic front of Wegmann (1935), Reynolds, Backlund (1943), etc. Erdmannsdörffer does not believe in its existence and draws an argument therefrom against the metamorphist theories, just as if such theories implied unavoidably the existence of the basic front. I do not believe in the basic front, except in very particular cases, because in all the many batholiths I have studied, particularly intrusive, there is no basic front. Such is also the opinion of Misch and of Ramberg, and the descriptions of many authors do not bring it to view.[3]

MECHANISM OF METAMORPHISM AND GRANITIZATION

If there has been a convergency of opinion, at least partial, on the genesis of granites, divergences remain very great as regards the mechanism leading to metamorphism and chiefly to migmatization and to granitization. Everyone admits the evidence of the alkaline introductions, but there is a fight between the "wet" and the "dry," to use Read's illustrative expression.

I think that the words wet and dry are in fact inaccurate. The wet are actually wet, but although I am considered as an extremist dry, I cannot say that I am unavoidably, exclusively dry.

[3] I shall base my hypotheses in connection with the mechanism of granitization on the non-existence in general of the basic front.

We have always admitted and even written that water or the OH or H ion might have played a role as catalyst in the real chemical reactions in the crystalline rocks attending granitization, reactions that are unquestionable under the microscope: there is no substitution of a crystal for another without a chemical reaction.

The essential question, to my mind, is not in fact wet or dry, but is actually the following: Is there any transport of substances by circulation of solutions or of gases, or only diffusions in a stationary medium, long distance diffusions through rocks, whether the medium in which such diffusions occur does or does not contain water in very small quantity. This is the main question for petrography, and I shall sum it up in the alternative: "circulations" or "diffusions." I am speaking, of course, of deep zones of metamorphism and granitization and not of surface zones.

Magmatists believe, of course, in "circulations," but so also do quite a number of metamorphists, in particular Read, van Biljon, (and in the U. S. A., Misch); these consider the introduction of substances as resulting from a real circulation of hot solutions or pore solutions.

My attitude as regards long distance diffusion through rocks was set forth in the booklet in which I expressed the theory of metamorphism by diffusion in the solid and I have written (1934b): "The petrographic facies presently observed on the Earth, apart from the sedimentary terrains not yet transformed, results, in its extreme variety, mainly from chemical diffusions that have occurred between the sedimentary terrains resulting from detrital or biologic actions on the elements of the first crust or on the subsequent terrains, and the deep layers, every time the temperature has been high enough for reaction speeds to be appreciable and has remained so sufficiently long enough." Along the same lines, in his fine article "Zur Deutung der Migmatite," Wegmann (1935) developed the theory of intergranular films. The two modes of diffusion have been described by us (1938c)—intergranular or lattice diffusion, with which my metallurgist training made me acquainted long ago. Since then, we have been fighting, despite the opposition we met with, in favor of long distance diffusion. Reynolds (1946) has decided in favor of diffusion of ions. Recently, in a work of very great interest and of high scientific value, Ramberg (1952a) has written: "One is forced to accept long distance diffusion through rocks as an important petrogenetic process," thus supporting my deep and persistent belief.

Ramberg is in favor of intergranular diffusion and thinks that we are "perhaps going much too far with the belief in large scale lattice diffusion" and he states in particular as argument against the importance of lattice diffusion the experiments of Jagitsch (1949). It is true that we wrote (1949a) that, while we admitted the two modes, intergranular diffusion did not seem to have played a very important part, this for reasons of geological observation. I do not believe that our knowledge in the matter of diffusion in the solid allows us presently to settle this discussion, the importance of which is indeed only theoretical; the reader whom this question might interest could refer to my paper presented at the Algiers Congress which has not yet been published, in which I answer the arguments raised against long distance diffusion by Jagitsch which, to my mind, have no real bearing. It also cites particularly instructive experimental facts. Recently, Frederickson (1952) has expressed another hypothesis: the diffusion paths would be "channels" observed by him in micrographic examinations of a most precise nature.

But, I repeat, the discussion—lattice diffusion or intergranular diffusion —appears to me of a purely theoretical order and with no great geological bearing. If the discussion must actually be settled, it should be by geological observations. The main question, because it is full of consequences, is: circulation of hot solutions or long distance diffusion in a stationary medium.

CIRCULATION OR DIFFUSION

Ramberg (1952a) has given excellent arguments against circulation and concludes for diffusion. We have already said that the many facts of replacement known to us appear to have been initiated essentially by chemical phenomena because they have no connection with the very uneven respective solubilities of the various bodies that circulation of solutions should normally involve (unless the latter be undersaturated for all the chemical bodies in question, which hypothesis does not seem quite natural and would imply circulation of considerable volumes).

I would like to bring new arguments based again on geological observations, which remain the clue for me, in view of our still too imperfect theoretical knowledge in the field under consideration. I shall say that:

(1) Diffusion is demonstrated (in this case lattice diffusion) on the crystal scale by the phenomena of corrosion between crystals and the fact that, according to the illustrative expression of Misch, one sees, when one follows the progress of granitization, that the crystals have cleared themselves of their inclusions.

(2) On the average scale, we have already cited observations of chemical exchanges, where diffusion of components in opposite directions between different strata (our paper of 1946; Ramberg, 1952a; and other authors) or between a layer and the pebbles it contains (du Toit, J. Jung, cited by us, 1939) are evident. The diffusions in opposite directions, in "double directions," as we have called them, imply diffusion in a stationary medium and are inconsistent with a movement of hot solutions.

(3) Let us now consider migmatization and granitization. The movement of bodies by circulating hot solutions can only be envisaged if (a) either the introduction of elements such as alkalis is not accompanied by the removal of other elements such as Fe, Mg, (b) or a removal of ferromagnesians balances the introduction of alkalis, the ferromagnesians being driven in the same direction as the alkalis, i.e. outward, which involves therefore the existence of the basic front. On the contrary, if the diffusions take place in the opposite direction, alkalis outward, ferromagnesians downward, the only mechanism that may be considered is a diffusion in a stationary medium, by lattice or intergranular diffusions.

This simple reasoning shows that field observation should bring the clue to the process of granitization.

The first question is: Is there only introduction or, simultaneously, removal? Without recalling here the numerous conclusions of authors in favor of a simultaneous removal, I shall follow the same deductive method I have followed to reach my conclusions on the genesis of granite—I shall start from detailed observations. It would be contrary to the scientific spirit to think that a granitic body originates from a process different from the one that is actually evidenced for the formation of a portion of that body, though small it may be, identical in composition and structure.

Let us consider feldspathization: Plate 1-A and B show two stages of evolution of xenoliths of the granite of Lanildut (Brittany) right in the heart of the batholith; the first shows a xenolith of microdiorite, rich in hornblende; in the second photograph, granitization is almost completed and the enclave scarcely differs from the granite (all the intermediate stages are to be found). It is clear from the appearance that the enrichment in K has been accompanied by a removal of Fe and Mg. Very fine examples of corrosion of hornblende and biotite by orthoclase may be seen under the microscope.[4]

The fact is still more evident in the case of the development of granitic areas isolated inside enclosing rocks, shown on plate 2-A taken at Argenton and B at Illiec Island, Brittany. The latter is particularly characteristic and conveys the impression of seeing the film of the granitization process—the edge of the granitization front reproduces the outside shape of the patches which it has joined. The chemical exchanges are written on the rocks: there has been introduction of Si and K, while at least Fe and Mg have been carried away.

I have already stated above, like other authors, that the basic front is an exceptional case. It is quite frequent to see the front of intrusive granites in gneiss or common micaschists or quartzites, etc., without any enrichment in ferromagnesians ahead of the front, whereas the study of the detail of the granitization process shows that it has been accompanied, as in the above case, by removal of Fe and Mg.

The conclusion seems quite definite; the general phenomenon of granitization or granodioritization, or migmatization, corresponds to an introduction of alkalis coming up from the depth and the return to the depth of Fe and Mg, which implies diffusions in a stationary medium.

WHAT IS THE MOTOR THAT CAUSES ALKALIS TO MOVE UPWARDS AND FERRO-MAGNESIANS DOWNWARDS?[5]

Ramberg has expressed the hypothesis of the action of the gravitation field. To explain the upward movement of the alkalis, Wegmann (1935) assumes that deepseated rocks contained alkalis and set them free. Misch (1949) has stated that it might thus be admitted that the upper part of the

[4] For simplification I have mentioned only the alkalis and the ferromagnesians, which is a general phenomenon quite easily observed, but there are actually other exchanges, in which Si, Al and Ca in particular are involved. Nor have I mentioned the question of H_2O (or OH or H), whose migration is geologically evidenced, in order not to lengthen this article. I shall only say that what may be observed seems to be accounted for if we consider water on the same basis as Na_2O, which basis gives rise to similar phenomena of diffusion.

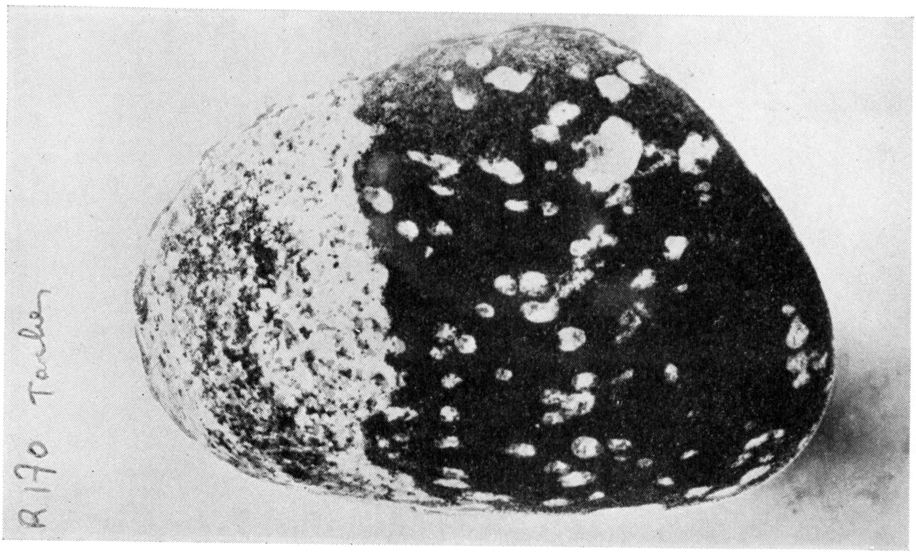
[5] This idea of motor seems to be too much neglected in the studies on diffusion speeds. There is no reason why the latter should be the same in the case of autodiffusion, of homogenization of a solid solution or of diffusion leading to decidedly exothermic re-

PLATE 1

A. Progressive feldspathization and granitization of granite enclaves of Lanildut.

B. Granitic patches isolated in a gneiss ahead of a front of granitization, Argenton (Brittany).

PLATE 2



Front of granitization, Illiec Island (Brittany). Granitic patches isolated ahead of the front: the outer form of the latter reproduces the outlines of patches.

sima is alkalinobasaltic, but he has made at the same time an objection which at first sight seems quite pertinent: the lavas that are poured out into the geosynclines prior to orogenesis, therefore prior to granitization, cannot have lost their alkalis since granitization has not yet taken place, and yet the major part thereof are not alkalic.

This objection supposes that those lavas are representative of an extrusion of a deep liquid initial sima that has remained unaltered; I shall explain below that to my mind such is not the case, which removes the objection. Ramberg is also of the opinion that metamorphism and granitization result from a tendency toward a state of equilibrium in the geosynclines. The latter have become out of equilibrium as a result of the accumulation of strata originating from the disintegration by erosion of the initial layers and of transport of materials accompanied by chemical (pH of water) and mechanical grading. These detrital strata are not in equilibrium between themselves nor with the deep strata, nor the rest of the initial crust that has not been submitted to erosion. When a rise of temperature, resulting from deep subsidence (eventually a concentration of radioactive elements) allows it, chemical diffusions begin and there is a tendency towards a new state of equilibrium by diffusion through solid rocks, in an aim toward the lowest level of free energy.

What may be the constitution of the initial crust? Geophysics teaches us that under the relatively thin layer called sial, more basic rocks called sima are to be found, but the data supplied tend more and more to show that this sima is a crystalline solid and not a liquid.

I expressed the hypothesis (1934a, b, 1948) that the constitution of the crust had been determined by a gigantic equilibrium: metal-slag-atmosphere, comparable to equilibriums that may be observed in iron metallurgy. Recent theories tend to explain the formation of the Earth by agglomeration of dust floating in the space. A verbal objection made to my theory is that the establishment of an equilibrium would have required considerable periods of time; in fact, there may not be such a wide difference between the two theories as might appear offhand to be the case. As a matter of fact, the knowledge of physical constants of bodies shows that the slag must have been liquid at a temperature when ferrous metals were still in the vapor state. Under those conditions, there would have been at first an agglomeration of the liquid particles before condensation of the metals. As their condensation proceeded, the latter would have gone through the already formed liquid slags, which is quite favorable to the quick attainment, or at least to an approach to equilibrium. In the present state of our knowledge, the most likely hypotheses are those which, being scientifically plausible, explain the maximum of known facts. I abide by my hypothesis, not because it is mine but because it explains.

actions. As an example, if we extrapolate to geologic periods of time the results of the experiments carried out by Vogel and Gibson (1950), who have measured the diffusion speed of Na ions through a plate of quartz under the influence of an electric field at 240°C., calculations made by my collaborator, J. Lamberton, show that the weight of Na carried in 10^5 years through a surface of one square meter would be of the order of approximately 10,000 tons. A real current of ions takes place in such case, and the figures computed have no longer any relation to the conclusions, very uncertain indeed, of Jagitsch.

without any added artifice, both the whole constitution of the crust, as compared with the density of the Earth, and the lack of atmosphere on the moon, and the comparison between the composition of our crust and that of the meteorites; the latter are, on the whole, decidedly richer in nickel, phosphorus and sulphur, which is quite in line with the theory of equilibrium. Finally, the consequence of such theory is that uranium, a very heavy metal, but with a strong affinity for oxygen, must have concentrated in the lithosphere.

A consequence of this theory is that the composition of the initial slag must have been very near that of a total of definite compounds,[6] and its silica content must have been of the order of 45 to 50 percent if we admit the presence of iron in the core. It is therefore not unlikely that the first layer may have contained as main constituents after cooling mixed crystals of the aluminous pyroxene and alkali pyroxene types.

Further, the external zones of metamorphism are chiefly characterized by micas such as chlorite and sericite; biotite and amphiboles or pyroxenes are to be found deeper, and last of all, the zones of gneissification, migmatization, and granitization are characterized by the progressively increasing development of feldspars, accompanied by resorption of the major part of the micas and amphiboles (I limit this enumeration to a very incomplete outline, leaving out many particular cases and many kinds of crystals, but this description is quite representative of the whole set-up as regards alkalis and ferromagnesians). In the external zones, there has been no or very low introduction of material brought from the depth; exchanges between surrounding strata have however taken place. The strong introduction of alkalis and the corresponding removal of Fe and Mg are quite apparent, principally in gneissification and migmatization.

Ramberg has shown recently (1952b) that in striving toward the lowest level of free energy, alkalis must move in the line orthosilicates → metasilicates → phyllosilicates → tectosilicates (feldspars), Fe and Mg moving in the opposite direction. The movement of alkalis indicated by Ramberg corresponds to observation.

It therefore seems to me quite natural to take advantage of Ramberg's remarks to express the hypothesis that, conversely, Fe and Mg, that are evidenced by observation to have diffused downwards, have been substituted for alkalis that might have existed at the beginning in the deep metasilicates, thus forming new metasilicates and even orthosilicates and simultaneously setting free Si. Therefore Ramberg's line of reasoning seems likely to give a general thermodynamic account for those long distance diffusions through solid rocks, provided that the original sima contained mixed silicates of Fe, Mg and alkalis, which hypothesis does not seem utterly impossible. To such action might perhaps be added that of the gravitational field of Ramberg.

When the geosynclinal condition allows diffusion, such diffusion tends to reach a new solid sima-sial equilibrium, which calls for concentration of the alkalis in the feldspars, expelling Fe and Mg from the larger part of the crystals existing in the old metamorphosed detrital strata; Fe and Mg con-

[6] The limits of this article do not allow me to state here the reasons for such assertion, which is in line with siderurgical experience.

centrate in the ortho- and metasilicates of the depth, being substituted for alkalis. On the whole, the only attempt to be made is to try and perceive the dominating features of a kind of life of the external layers of the Earth in their aim at an equilibrio-petal process; the equilibrium is never reached, were it only as a result of the resistances opposed to diffusions, of which I shall give an example below, and of the volcanic eruptions that bring back ferromagnesians into the surface zones.

In this concept, the initial external crust must have been richer in Fe and Mg than the whole of the strata (other than the lavas) that may be observed on the continents, since the latter would have become poorer in Fe and Mg. The common occurrence of large iron deposits and dolomites in the old strata does not seem to be in contradiction with this deduction. However, it is natural that the substratum of the oceans should have remained mainly basic.

VOLCANISM, METAMORPHISM, GRANITIZATION

The lavas are mainly basic, the plutonic rocks mainly acid. Numerous authors, among them Read (1943, 1944), have drawn attention to this point. However analogies in chemical composition between lavas and plutonic rocks account a great deal for the common magmatic origin which has been claimed for them. Many years ago I expressed the hypothesis that volcanoes represented magmas without roots.

Geophysical data show more and more that the crust is solid down to very great depths; hence a tendency to regard lavas as originating from the partial melting of solid rocks, following a sudden decompression brought about by fissuration of the external layers, which hypothesis is experimentally founded (Yoder, 1952).

It has often been noticed that volcanism precedes orogeny and also that a new volcanism succeeds a period of great orogeny.

Comparison of these facts leads me to outline a new theory, according to which metamorphism, granitization, and volcanism would result from one cause, namely, chemical exchanges between internal and external layers by long distance diffusion through solid rocks, only the effects of this cause being different.[7]

The tendency toward attainment of an equilibrium corresponding to the lowest level of free energy conflicts with the fact that this attainment would have as a consequence important local changes in volume. If I base myself upon the hypothesis expressed by Barth (1948a, b) that oxygen remains statically stationary in the course of diffusions owing to its extreme superabundance, I have calculated (1950) in a few cases, the analyses and densities of which I knew, the increases in volume resulting from granitization; I have found low values of the order of 2 percent. That it should be so is natural as, following Le Chatelier's principle, the difficulty of deformation of the strata of the country existing above opposes the occurrence of reactions that would involve too important changes in volume, as stated by us (1949b). Nevertheless, in relatively superficial regions, exothermic chemical reactions

[7] Ramberg (1952a, p. 273-274) also considers an acid secondary volcanism, through melting, as a consequence of orogeny, following the rise of temperature resulting from exothermy of the reactions.

resulting from diffusions may bring about changes in volume and are a cause of oregeny. Simultaneously, impoverishment in alkalis of deep metasilicates of the sial should have as a normal consequence a decrease in volume of the latter. Do we find there, apart from isostasy, a possible explanation for the sima depression and the Si-Al roots under mountain chains? (May I suggest to the readers who would question the possibility for diffusions in the solid accompanied by chemical reactions to bring about deformations that they refer to the photos published in my paper to the Algiers Congress, with the authorization of Dr. Ehlers of the University of Chicago following a very interesting test carried out by him).

If diffusions begin in depth between solid sima and subjacent detrital strata in a geosyncline under a very great thickness of the strata, deformations are not possible; exchanges tend therefore to take place without any change in volume or deformation, absorbing energy, and the exothermy of reactions considered by Ramberg brings necessarily a rise of temperature. Plastic deformations being impossible, tensions that are originated can only cause fissurations by shearing. Should such a fissuration occur, there follows a sudden decompression and, as a result, melting, at least partial, together with an increase of volume and volcanic extrusion. This extrusion stops when the excess in volume has been resorbed, and the fissure may be stopped until the cycle begins over again, unless exothermy evolved by the continuation of the reactions be sufficient to maintain the liquid column. The nature of the lavas outpoured depends upon the depth of the fissure; if it corresponds to a level of de-alkalized sima, for instance, extrusions will be basic; if, on the contrary, the fissure reaches higher layers in process of alkalization, outpourings could be rhyolitic. All compositions are possible for intermediate levels, according to the nature of the strata involved; and it is natural that compositions should undergo change, as time proceeds, for one volcano.

In the light of this hypothesis, it therefore seems normal that generally the same chemical composition should be found in the lavas and plutonic rocks since they are created by the same process of diffusion. There is however a difference, namely, that the genesis of granular rocks may be accompanied by slight increases in volume, while this does not apply to lavas, which has a bearing on possible compositions. This fact should play a part particularly in the case of acid lavas; alkali feldspars have a low concentration of oxygen (number of atoms of O per unit of volume of crystal), and their formation at the cost of other minerals tends to increase the volume, whereas quartz, on the contrary, has a relatively high concentration of O. Following this theory, during the process of formation of rocks generating rhyolites, the proportion of quartz (or tridymite) should be more important as compared with feldspars than in the granites. Is it a mere coincidence that rhyolites are actually richer in silica than granites?

According to this theory the real phenocrysts should correspond to crystals existing in solid rocks prior to remelting that have not entirely dissolved during the remelting process and removal. This would explain how, among many examples, phenocrysts of quartz known as of the first period are to be found in a mush containing quartz and feldspars known as of the second period, which is contrary to the hypotheses of differentiation and sequences of crystallization of magmatic theories. (I have no knowledge of a magmatist explanation of these numerous aberrant facts.)[8]

Thus, outpourings of lava prior to orogeny would correspond to the beginning of diffusion in relatively deep strata. If the zone of diffusion reaches higher strata, possibilities of deformation exist, and we come to the cycle of metamorphism, granitization and orogeny. The two phenomena may, in fact, coexist during part of the passage of time. In Brittany, for instance, dark dikes of diabase may be seen cutting the granites with sharp borders, while others, on part of their course, are partly granitized with formation of eruptive breccia; others, finally, remain only as xenoliths more or less transformed or as discontinuous trails of amphibolite.

At the end of the intense period of orogenesis, the new external forms taken by the strata increase their resistance to deformation, and continuation of diffusion in depth can hardly generate anything but volcanism; the lavas will be emitted through fissures opened in the less resisting zones, therefore preferably on the edges of the Cordilleras: post-orogenic volcanism. Never in fact is the equilibrium thoroughly attained in any compartment of the crust, and volcanism may occur again by way of fissures, even in regions of old metamorphism and granitization.

Under such conceptions, volcanoes would correspond to deep chemical reaction centers, to real subjacent abscesses, unable to deform the Earth's crust, that would break through to the surface by way of fissures developed by the tensions.

Pursuing the line of reasoning I have followed for nearly twenty years I have here attempted a synthesis explaining at once metamorphism, granitization and volcanism and, partly at least, orogeny by diffusion migrations in the solid state, a consequence of the "unstable state of affairs of the geosynclinal stage," to use Ramberg's own words (1952a). He also entertains the idea of a connection between geosynclines, orogeny, granitization (Misch also), and volcanism, at least acid. I even go further and include volcanism in its entirety. Moreover, I incline to consider diffusion as one of the causes of orogenesis (other causes remain, to me, entirely mysterious), provided of course, and this seems to me likely, that such diffusions involve whole and deep compartments of the crust. Under such conception, the convection currents mentioned by Vening-Menesz and other authors in their theories on orogenesis are restricted to convections of ions.

I am under the impression that by so doing, I very much run the risk of being accused of "perhaps going much too far" and the "perhaps" may probably even be suppressed. But the thought that granitization in the solid state and long distance diffusions through solid rocks are now admitted by eminent authors is a comfort to me.

I believe, on the other hand, that when a theory leads to deductions that correspond to a non-negligible number of still unexplained geological facts and that no facts seem to be in opposition to them, one should not hesitate to

[8] The possibility of generation of phenocrysts by subsequent metamorphism of a lava should not be forgotten either. We have brought this fact to light in a precise case (1938b).

express it. Even if it is not perfect, as is certainly the case at present, or even if it is erroneous, it may cause ideas to occur to some other authors, and from these exchanges ensue advances in our knowledge.

In closing, I would like to tell of the great influence that the reading of the remarkable articles published in American publications has had upon the evolution of my own conceptions. Without excluding others, I shall mention in particular in this respect Professors T. W. Barth, L. C. Graton, G. C. Goodspeed, Peter Misch, and H. Ramberg. I wish to express to them my very deep gratitude.

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