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GRANITE AND METASOMATISM

REGINALD A. DALY

ABSTRACT. All geologists interested in the genetic problem of granite have long agreed that some granites crystallized from their own melts (magmas), while other rocks of closely similar composition and texture were formed by molecular replacement (metasomatism) of older, non-granitic rocks by absorption of foreign material. According to the cumulative opinion of most experienced geologists during the past hundred years, the ratio of total volume of "magmatic granite" to total volume of "metasomatic granite" in the earth's crust is large. Within the last twenty-five years a smaller but increasing number of workers, here called for convenience "metasomatists," have come to prefer a reversed ratio. The following paper is intended to show that the second group has not thought "to scale," have neglected many vital facts bearing on the problem, and have in no wise weakened the arguments of the "magmatists," which have led toward what appears to be a sound theory of the evolution of the earth's crust.

INTRODUCTION

POSED more than a century and a half ago, the granite problem is one of peculiar dignity and meaning. If it were solved, the origin of basalt, peridotite, anorthosite, gneiss, and other principal stuffs of the earth's crust would be more intelligently discussed, and theories relating to continental genesis, geophysics, ore genesis, geodesy, physiography, oceanography, and paleontology would be better controlled. If it were solved, man would know with more assurance why there is dry land, a perch for air-breathers like himself; how he can thrive in a metal-hungry age; and how to account for the sedimentary rocks which bear the medals of creation and declare so much of the history of our planet. But there are two general and portentous difficulties in the way when the attempt is made to round out a good theory of granite itself.

First, the solution cannot be complete until the cosmogonists declare the mode of birth, the initial temperature, the initial composition including gas-content, and the youthful organization of the nearest of the heavenly bodies; here geologists lack some of the guiding data they need.

The second lion in the way toward the desired goal is an ineluctable necessity. No solution of the granite problem is possible until there be assembled and digested the relevant facts recorded by polyglot field-workers in seven continents and the wide oceans. But even this world-wide, encyclopedic product of field work with hobnails, hammer, compass, and topographic map is manifestly insufficient. Other data, also widely scattered in the great libraries, must come: (1) from properly designed experiments on melting, molten, and freezing silicates and mixtures of silicates, even if only in "little crucibles"; (2) from analogies secured by experiments with materials other than silicates; (3) from handbooks of the skilled petrographers; (4) from information supplied by seismograph and gravity pendulum; (5) from any appropriate hints by astrophysicists and cosmologists; and (6) from the results of those writers who used well regulated imagination when they delved into the depths of both space and time.

Thus the granite problem demands scholarship of dismaying breadth and thoroughness as well as vision regulated by common sense. To make the best interpretation of what is both figuratively and literally a world of facts and suggestions is not science; it is rather an art, a way to science, and subject to the personal handicaps of the artist. Anyone offering a solution should be wise enough to know that his own building is in part merely a scaffolding around the temple of truth. Life is short; art is long!

Hutton showed how wrong was Werner's guess that granite was a precipitate from a primeval ocean, a world-circling "chaotic fluid." Ever since, most geologists have favored the idea that nearly all massive granite represents former magma, a melt, which during its crystallization lost some of its initially small content of gas and vapor. No widely experienced petrologist doubts that some granite-like rocks are products of metasomatism of sediments and schists, that is, the products of mineralogical change wrought by the introduction of thin, exotic, highly mobile fluids ("emanations") containing the elements of quartz and feldspar and imbibed by the invaded rocks. For a century the ratio of volume for such metasomatic granite to magmatic granite in the earth was steadily regarded as small. But during the last twenty years geologists and petrologists of increasing number have come to prefer a reversed ratio. Their doctrine of molecule-by-molecule or atom-by-atom deposition of matter essential to granite by "emanations" originating at depth, and in quality sufficient to have made most of the quartz-rich massive rocks of the world, is a kind of inverted Wernerism.

However, upholders of the new doctrine believe it possible that the energized "thin" fluids have here and there fluxed originally solid rock, with the development of true magma which has been capable of eruption in the form of bodies of "young" granite as well as bodies of rhyolite and obsidian. These exponents also recognize that partial fluxing of the kind may have made the invaded formations mobile and thus able to intrude higher levels of the earth's crust; and they also assume that during orogeny the lower part of the planet's Sial or "granitic layer" may, because of its forming part of the depressed mountain-root, have been thoroughly melted and eruptible.

Thus, while they differ about the ratio of volume of metasomatic granite to volume of magmatic granite, the two schools of thought have some common ground. This fact suggests the possible value of a convention adopted for brevity of reference and perhaps for clearer understanding of the text to follow. The convention: those who stress solidification of melts will be called "magmatists," and those who stress molecular replacement and addition of material will be called "metasomatists." The quotation marks will indicate the arbitrary character of the naming. Most writers belonging to each of the two schools emphasize the rise of matter from the depths. However, some "magmatists" prefer a two-way street, the sinking as well as the rising of material being essential in the development of granitic bodies. With one exception the "metasomatists" prefer a one-way street and the uprise of material. The "magmatists" demand temperature of 1000°C. or higher in the development, while the "metasomatists" accept much lower temperature for their emanations when still effective in the making of granite.

Another expression, also to be distinguished by quotation marks, will be convenient for present use. It is "granite" (with adjective "granitic"), intended to cover the massive quartz-rich types of rock—granodiorite, quartz monzonite, quartz syenite, tonalite, and quartz diorite, which each of the two schools explains in a manner similar to that preferred for granite proper.

The object of this paper is to list some principal reasons for the writer's belief that the "metasomatists" have overstated their case, and this because as individuals and as a group they have neglected vital facts. The grounds for his belief will appear as the discussion deals with: (1) the assumed emanations or granitizing agents; (2) the phenomena of the vast display of "granite" in the Pre-Cambrian "shields," which seem to represent fairly well the greater part of the Sial or "granitic layer" with minimum average thickness of ten kilometers; (3) the nature of a basaltic earth-shell, still deeper, which must be in close genetic connection with the formation of the Sial itself; (4) the associated questions arising from the facts known about the composition and volume of the ocean; and (5) alternative method or methods by which any large body of "granite" won space for itself in the earth's crust—a question which the "metasomatist" Read (1948, p. 7) has described as "at the heart of the granite problem," and a question particularly intriguing to any geologist who studies post-Cambrian batholiths and stocks in the field.

MODE OF ATTACK ON THE GRANITE PROBLEM

To estimate the volume-ratio of magmatic "granite" to metasomatic "granite" is necessarily a matter of theory. A good theory must rest on facts derived from observation at the earth's surface and just as obviously on assumptions about conditions in depth when "granites" were made.

At the outset it is expedient to weigh some fundamental considerations. The first of these is dry land of continental dimensions, dry land dominantly surfaced by "granite" or by the chemically equivalent acid gneisses or by the debris of "granite" and gneisses. A second fact is proof that the velocities of the earthquake waves are those expected if closely similar acid rock underlies the continental surfaces to average depth of at least ten kilometers. Geophysicists have established a third fact, that this interrupted earth-shell, the Sial, is in quasi-flotation on the earth's body because of the relatively low density of the patchy shell. The geologists have proved a fourth fact—that long before the Paleozoic era extensive dry-land masses of Sialic or dominantly "granitic" composition were eroded in some regions and veneered with quartzose sediments in other regions. The evidence for this comes from all of the existing continents. It seems therefore clear that more than 95 percent of the world's "granite" was formed far back in Archean time. And there is a fifth fact of the first rank: "granite" made in post-Cambrian time is chiefly batholithic and almost entirely confined to belts of post-Cambrian mountain-building, which themselves are located in the Sial, whether emerged or covered by the ocean.

The discussion of origins for both Sial and the comparatively young batholithic masses is laden with difficulties. We do not know the initial temperature and gas-content of the earth. We do not know whether the Moon is wholly or in part of terrestrial origin, though, if it was, the forceful ejection from one hemisphere might have produced a strong contrast of both material and temperature of the robbed hemisphere with the material and temperature of the opposite hemisphere. We do not know how effective was the Moon, supposed to be a daughter of the earth and initially close to the earth, in slowing down the planet's rotation and thus threatening the stability of the original crust of the planet by fracturing. We do not know with absolute certainty the heating power of radioactivity in the earth-shells.

Thus a speculator has to cut a number of Gordian knots before he can make any real progress toward explaining the Sial, the real "heart of the granite problem." With full consciousness of arbitrary procedure the writer does some cutting. Specifically he does not assume the temperature of the earth's outer shell in pre-Archean time to have been high enough for unmixing of a granitic layer from a basaltic layer —a conception founded on Fenner's (1948) idea of such immiscibility even in post-Cambrian time. Encouraged by the opinion of petrologists in general, the present writer assumes that important superheat does not now characterize, and perhaps has never characterized, the superficial earth-shell. He assumes that the heat of radioactivity is, and has been, steadily developed in the globe. Further, he is quite prepared to entertain the hypothesis that from the depths of the young earth steam and other gases emanated toward the surface at a rate higher than that ruling since Archean time. Therewith he finds one reason why the Archean terranes exhibit so much local metasomatism. On the present occasion he does not assume any particular thermal gradients for the earth-shells beneath the basaltic shell whence have come the basaltic lava-floods at intervals since early Archean time. He assumes that the crystallized Sialic material, if in average no more basic than granodiorite, is gravitationally stable on an underlying layer of liquid basalt. On the other hand, he assumes that, because of density difference, crystallized Sial is liable to founder in granite magma and in Sialic magma generally; also that crystallized basalt and even other somewhat less basic but crystallized rocks are liable to founder in liquid basalt. He assumes the sinking of early-formed crystals in and from either liquid. Finally, he assumes that sunken blocks are partially remelted, with the result that the more salic, lower-melting part of each rises toward the earth's surface, there being a tendency to form ultimately within the composite magma a top layer of granitic composition.

Although the "metasomatists" recognize the five great facts above listed, no member of the group has given the five all the attention they deserve in our problem. Largely because of this limited outlook the arguments for the wholesale molecular replacements have been based on widely different assumptions. The wide range of differing and competing premises essential to their scheme, is of itself ground for doubting the logic of any or all of their school of thought. Their ideas about source, chemical nature, and mode of travel of the emanations will first be examined. Then the origins of the Sial and of the post-Cambrian batholiths will be considered, when it will be found that the "metasomatists" are vague also about their second necessary premise, namely, the original character of the rock matter supposed to be molecularly replaced. Further evidence that those writers have not thought through our problem appears when the history of the basaltic earth-shell and also the composition and volume of the ocean are incidentally discussed. Thus again and again we shall see the "granite" problem is planetary and not to be solved by ordinary geological field-work.

THE "EMANATIONS"

Before taking a closer look at the arguments of the "metasomatists" it is well to note that Sederholm and his followers as well as Michel Lévy and Lacroix, all of whom by their combined researches demonstrated granitization on a comparatively large scale, are not to be classed among the "metasomatists" as above defined. For all of those Fennoscandian and French masters the source of the granitizing solutions was granitic magma. All of them were quite clear about the relative volumes of granite magma and corresponding granitized rock—clear as to which was the hen and which the egg. Equally obvious to them was the illogicality of seeking the origin of "granite" in the molecular transformation of shale, phyllite, mica schist, and quartzite, since these are chiefly detrital from "granite." All of these experts in petrogenesis were incapable of reasoning in a circle. The "metasomatists" of the present day are, therefore, not entitled to claim from these Fennoscandian and French studies any support for the idea that metasomatic "granite" dominates in the earth's crust. We may now return to the logical difficulties in the way of accepting the extreme ideas of the new school.

(1) Divergence of views about the source of the supposedly potent emanations is clear. Backlund (1936, p. 343) and MacGregor and Wilson (1939, p. 201) find a source in the solid rocks adjacent to the transformed body. Reynolds (1947, p. 222) finds there the principal source. MacGregor and Wilson (1939, p. 201), like Barth (1936, p. 830), find another source in the earth's granitic layer, the Sial. Barth (1939, p. 111) finds a source in the liquid residual from the advanced crystallization of a basaltic layer beneath the granitic Sial. Rittman (1939, p. 603) specified as source an olivine-basalt layer of great thickness and resting on a much denser planetary core. Holmes (1948, p. 414), Wegmann (1935, p. 345), van Bemmelen (1940, p. 33), and King (1947, p. 37) find the principal source at "great depth," but do not declare the nature of the parent earth-shell or earth-shells. Termier (1912, p. 592), who had recommended that the deep interior of the globe be left "aux poètes," has since written his eloquent prose to advocate colossal metasomatism and granitization by his "colonnes filtrantes" coming from great depth.

(2) Sederholm and Michel Lévy used field evidence rather than psychological necessity when they derived their granitizing solutions that bore silica and the oxides of both plagioclase and orthoclase dissolved in enough water to make the solutions "thin." Many of the more thorough-going "meta-

somatists" of the last twenty years describe the solutions as containing the oxides "needed" or "wanted" to give solid "granite" by molecular transformation of solid rocks. Since the latter include such differing types as pelites, mica schists, amphibolite, greenstone, diabase, gabbro, and limestone, the named oxides in the assumed solutions are those "needed" or "wanted" to make the "granites" whose development is being sought. Soda, silica, and lime are the oxides most commonly named, but seldom potash, which is nevertheless so important, so "needed," in the supposed granitization of limestone, greenstone, amphibolite, and basaltic rocks. In general the potash should be at least as abundant in the solutions as soda, and yet it is still to be shown why any emanation, especially one from great depth, would carry comparable proportions of the two alkalies.

Thus it is not easy to picture what the "metasomatists" mean when they write of the chemistry of the emanating solutions. Until there is clearer statement of the composition of these in terms of all the oxides and elements present, their argument for the extreme metasomatism must be regarded as weak in its second fundamental premise. Meantime specific mention of the oxides "needed" or "wanted" for granite-making might be readily taken as the product of reasoning in a circle so far as the chemistry of the postulated emanations is concerned.

One's mental trouble is not assuaged by Rittmann's (1947, p. 11 and 14) description of the granitizing agent as "gas," without his giving the composition of this gas. Nor is there any clear statement of the total composition of the rising "clouds" of ions or atoms, which, according to some "metasomatists" writing in the last dozen years, are to be preferred as the transformers. The nearest approach to such a statement appears to be that of Barth (1947, p. 181), who, though not to be classed as a full-fledged "metasomatist" as above defined, has discounted Sederholm's theory of transformism and now explains large bodies of Pre-Cambrian granite in Fennoscandia by the atomic-cloud hypothesis. For Barth each rising "cloud" is an "ichor," this term here meaning something quite different from that assigned by Sederholm, its inventor, Barth holds that his own "ichor" has produced two kinds of granite, one represented by molecularly replaced older rocks and the other represented by direct crystallizations of the atomic-cloud itself. This "ichor" is supposed by him to come from great depth. But we note again that the geophysicists are unanimously insistent on the comparatively minute proportion of potash in the deep earth-shells, and that no process of increasing the proportion of the oxide in rising emanations has ever been announced by the "metasomatists." Their dilemma is all the more apparant when we take into account a principle stated by Lapadu-Hargues (1945, p. 299), one of them, namely that the mobility, migratory power, of the atoms is proportionate to the diameter of the ions concerned, so that Na and Ca, already at depth in decided excess over K, should outstrip K in the rise toward the earth's surface. In striking contrast is the view of King (1947, p. 56) that potash travels farther than soda for example and is consequently "fixed at greater distances from source than the soda emanations."

(3) To transform a heterogeneous mass of quartzose sediments, limestone, and basic rocks into homogeneous "granite" by molecular replacement demands the expenditure of much energy, particularly thermal energy. Since, according to the "metasomatists," the visible parts of the young batholiths of the orogenic belts were so developed close to the earth's rocky surface, the granitizing agent must have brought up most of the heat required for the postulated chemical reactions. Few estimates of the agent's temperature when rising to the appropriate levels have been published. That inferred by Ramberg (1946, p. 29), when in his view Pre-Cambrian limestone was granitized, was no higher than 350°C. From other Pre-Cambrian cases Backlund (1936, p. 343) assigned "moderate" temperature, "far below the melting temperature" of ordinary rock. From Norwegian cases Barth (1929, p. 122) deduced a temperature under 500°C. for the granitizing solutions. Holmes and Reynolds (1947, p. 61), preferring an agent composed of "clouds" of ions, inferred quite moderate temperature for the reason that "ionization attains a maximum at a little above 200°C." and "at higher temperatures it begins to decrease again."

None of the four estimates is accompanied by a statement as to the depth at which the molecular transformation was effected. Presumably the emanations were much hotter before they began to rise from "great depth." On that assumption and because the rocks invaded at "great depths" were also hotter, some "metasomatists" like to think of the granitizing reactions as having taken place after the invaded formations had been orogenically depressed to depths much greater than those ruling in the uppermost part of the earth's crust.

Clearly, the "metasomatists" are not agreed about the third premise basal to their logic—that of securing sufficient energy for the making of large masses of "granite" according to their recipe.

(4) Nor are they of one mind about the mode of upward diffusion of the emanations. One of their groups favors upward migration along the mutual contacts of the minerals composing the invaded rocks. A second group favors solid diffusion, the emanational matter being ionic and rising through the space lattices of the crystals in the invaded terrane.

That the first group has been on the wrong track in its thinking is shown by Holmes and Reynolds (1947, p. 50ff), who themselves are "metasomatists" and belong to the second group. They point out that "the essential functions of gaseous or liquid solutions at any particular spot are to bring up the incoming ions and maintain their concentration, and to carry away the displaced ions. As between liquid or hydrothermal solutions and gaseous or pneumatolytic solutions, it seems probable that these functions could be carried out more effectively by the former."

Continuing, their summary statement reads: "A gas phase would have the advantage of easy penetration along submicroscopic cracks through otherwise impermeable rock, but against this it suffers from several disadvantages. Considerations of relative volatility suggest that a gas phase is most likely to be acid. In the replacement phenomena under discussion, however, the evidence is overwhelmingly suggestive of the dominance of alkaline materials. Moreover, components of widely different volatility would need to be combined in the migrating stream . . . Ingerson and Morey cite experimental observations indicating that diffusion through gas is quantitatively a more effective process than transport by gas . . . . There is the third difficulty that the degree of ionisation of a concentrated gaseous solution under high pressure and therefore of relatively high density is likely to be extremely low.

A gas phase is therefore thought to be a feeble carrier of the ions necessary to initiate solid diffusion through the crystals with which it comes into contact. Crystal linings along the surfaces of rocks and drusy cavities (rather than metasomatic replacements) are the characteristic results to be expected from gaseous transport and deposition.

"On the other hand, hydrothermal solutions, if of magmatic ancestry, are likely to be strongly alkaline from the start, as a result of hydrolysis of silicate minerals. They are also likely to have a higher degree of ionisation . . . . In respect of both composition and degree of ionisation hydrothermal solutions appear to have a definite advantage over gaseous solutions, especially in their capacity for initiating and maintaining metasomatic replacements of appropriate parageneses. But except where adequate passage-ways are available, e.g. by way of open cracks and fissures, their capacity for penetrating rocks is severely limited. Silica-bearing solutions in particular seem to have a very small power of soaking through rocks, like quartzite, in which the only passage-ways are the minute interstices between closely fitting mineral grains."

These authors therefore conclude that, since the interstices are isolated and discontinuous, "molar transport becomes impossible, and replacement can then be effected by means of solid diffusion.

"Applying the above and other relevant considerations . . . . it can be said that the few criteria available for discrimination suggest that solid diffusion was probably the dominant process concerned."

"It is impossible in a limited space to discuss the matter systematically, but to us it seems manifest that the whole process is more elegantly explained by progressive solid diffusion due to migration of ions through the crystal lattices and along the crystal boundaries."

Other writers regarding solid diffusion as the principal mechanism in granitization include Backlund (1936, p. 343), Ramberg (1948), Perrin and Roubault (1939, p. 64), Bugge (1945, p. 46), and Barth (1947, p. 181). All have rejected "ichors" as solutions. All are in duty bound to show how "clouds" of ions can make nearly or quite homogeneous "granite" in masses kilometers in thickness by diffusions into and through the crystals constituting a highly heterogeneous terrane. To discharge that duty they have assumed an enormously energized condition of the "clouds" as they start up their long upward journey, but they are far from having explained how enough of this initial energy could be retained to permit the transformism at such high levels as those near the roofs of batholiths. Neither have the diffusionists paid adequate attention to the loss of chemical potential as each "cloud" works its arduous way upward. With each centimeter of advance the "cloud" must rapidly lose its chemical potential also. Moreover, these writers have overlooked the fact that the time during which a Cordilleran batholith was formed was finite; one might well ask: which would win the race, transformism or the inevitable cooling with loss of transforming power? Is there a clear answer to that question?

A more authoritative, though in principle similar, criticism of reliance on solid diffusion in the "granite" problem was voiced at the Ottawa symposium by Bowen (1948, p. 81).

Since the day-long discussion at Ottawa saw its way into print, Ramberg (1948), a solid diffusionist, has tried to account for the upward migration of the granite-making ions. He assumes the active force to be gravity, which has caused the light, granite-making ions to rise and the "unwanted" heavier ions to sink—a case of separation and double migration. To the present writer this reliance on tractions due to the differing weights of almost infinitesmal particles inside each of a multitude of crystals is a reductio ad absurdum in the theory of solid diffusion.

To conclude: while every experienced geologist cannot doubt the rise of emanations and also the fact that rocks and minerals including ores have been modified as well as replaced by solid diffusion, neither of these actions can with good reason be thought responsible for the emplacement of the huge batholiths. In this whole matter of emanations the "metasomatists" should try to reconcile premises and lines of reasoning and, above all, to think to scale! That they have overshot the mark is still more evident as we leave the field of physical chemistry and proceed to review facts which geologists have brought home in their note-books: facts concerning the Sial as well as the "granites" of the post-Cambrian chains of mountains.

THE SIAL OR "GRANITIC LAYER"

The following brief discussion of the outer earth-shells will be preceded by a few words devoted to some questions about the interior of our planet, particularly with regard to the possibility of a superheated, vitreous state for a large part of the silicate mantle on the iron core.

Some cosmogonists are inclined to look on meteorites as giving a clue to the composition of the earth, to regard this a daughter of the Sun, and to assume an initial temperature for the planet to have been so high as to compel a molten state from surface to center. These authorities recognize immiscibility of metallic, nickel-bearing iron with the silicate material and an early gravitational separation of an iron core from its thick silicate mantle. The rate of such settling-out of the iron must have been progressively lowered as the depth increased. The enormous increase of viscosity with increasing pressure may conceivably have prevented complete separation of small droplets of iron at great depth, even after the lapse of three billion years. Moreover, there is no certainty that such high pressure did not affect the degree of immiscibility. Hence, for those reasons alone, it seems possible that the mantle never reached chemical homogeneity, thus permitting convection currents from top to bottom of the mantle. If this should have been the case, a further possibility would have to be faced: some superheat at depth. On the other hand, at and near the surface, where viscosity was low, the silicate mantile could have been thoroughly cleansed of metallic nickel-iron. There, to the depth of perhaps one or two hundred kilometers, the material can be thought to have speedily attained the composition of the iron-free meteorites, that is, a peridotite. On this assumption may be based a speculative origin for a world-circling Sial.

Jeffreys (1929, p. 138) has pictured for the infant earth a complete settling-out of the metallic iron, leaving a peridotitic mantle about 3000 kilometers thick and so homogeneous as to permit complete convective overturn of its matter, with rapid chilling, and consequent freezing of the whole thick shell. By postulating such crystallinity he considered that he could best explain the transmissibility of the shear (rigidity-controlled) seismic waves throughout the mantle. But Bridgman's (1931,

p. 340 and 1949, p. 117) experiments on the remarkable effect of high pressure on the viscosity of both liquids and gases suggest the possibility that much, if not most, of the earth's mantle may be so viscous as to behave like a crystalline solid with respect to the deforming forces connected with either earthquakes or the variation of latitude.

In his own thinking about the early differentiation of the material of the infant, star-born earth, the present writer finds it easier to assume an initially molten state for the superficial peridotite. At the temperature of about 1400°C. radiative chilling would have been extremely rapid, so that before crystal-fractionation could get under way the surface material might have been crusted to small depth—tens of meters. Following Thomson's (1883, p. 483) speculation of long ago, one should expect instability, breakup and foundering of the new crust in the thick, less dense magma beneath; some remelting of the fragments in the hotter liquid; the rise of the lower-melting, less femic material; renewed crusting and foundering, with a tendency to develop a temporarily-liquid basalt at the surface. The same result would be expected if the high-melting olivine and bronzite or hypersthene, as free individualizations, sank because of their relatively high density. By integration of both effects one can conceive that a world-circling layer of molten basalt, several tens of kilometers thick, was developed.

The molten basalt would, however, be unstable chemically. It, too, would be affected by crystal-fractionation. With a composition possibly a little more salic than the plateau-basalt of post-Cambrian eruptivity, a five-kilometer or six-kilometer, world-circling, superficial layer of material chemically close to granodiorite might result. Such differentiation can be most readily understood if it too was a step-by-step process, involving repeated crustings, repeated founderings, and repeated remelting of sunken crust-blocks. The ultimate product would thus be an unsinkable "granitic" crust resting on a layer of slowly cooling and crystallizing basalt in which the intrinsic density increased with depth so as to ensure the inner stability of this sub-layer also.

At the Ottawa symposium Bowen (1948, p. 82), leading "magmatist," did not mention the Sial or the earth's "granitic layer" and stated that the batholiths "represent the great bulk of our granites." Read (1948, p. 4), leading "metasomatist," spoke of an original granitic layer and again of "a light granitic sialic layer" overlying a denser, basaltic, Simatic layer in the earth, both layers having been formed when the planet was new. He gives no opinion as to how much of the less dense shell is true granite and how much consists of other kinds of "granite." Both of the leaders approve of the conclusion of the geophysicists who have found the upper Sialic part of the earth's crust in continental areas, to an average depth of 10 or 12 kilometers, to be "granitic." Read (1948, p. 5) seems to agree that this top sublayer was, in pre-Archean time, magmatic though even then of granitic composition. Is he, then, logical when he claims that most of the world's granite is metasomatic and that the problem of its development is a matter to be dealt with by field geology? Can the unaided field geologist deal adequately with an earth-shell so largely invisible, inaccessible?

Perhaps some "metasomatists" prefer to think of the young earth's crust as having been made of crystallized basalt or even more basic material and in pre-Archean time granitized by emanations from depth in volume to match the volume of the existing Sial. No one of their school, however, has developed the hypothesis so far as to imagine the fate of the enormous volume of material corresponding to the "basic fronts," or has deduced the effect on the volume and composition of the ocean. Any "thin" solution, representing a postulated emanation would presumably have been made "thin" by abundant water. In that case the extra silica and potash required for the granitization would be, by weight, only a small fraction of the hydrous solution, which itself was a small fraction of any earth-shell where the emanation originated. To develop a world-circling "granitic" layer only five or six kilometers thick would doubtless give an ocean several times as voluminous as that now existing. Also in all likelihood the salinity of this watery envelope would not be readily reconcilable with Clarke's (1916) careful accounting for the salinity of the existing ocean.

If the emanating solutions came from depth greater than the top of the peridotitic earth-shell, now apparently less than 80 kilometers below the surface, the basalt extruded during the formation of the Sial would, by the metasomatic hypothesis, differ chemically and decidedly from the basalt extruded in post-Archean time. Yet there is a high degree of uniformity for the basaltic lavas emitted ever since an early Pre-Cambrian epoch.

If the solutions came from such depth the transition from the Sialic layer to the layer just below it should be gradual, but some seismologists, like Gutenberg, believe the mutual contact to be relatively sharp.

The same failures to account for relevant and more or less assured facts mean as many grounds for rejecting the hypothesis of upward-migrating "clouds" of ions. Ramberg's speculation does avoid the observational difficulties by sinking the heavier ions, but, as already observed, gives an incredible picture of the way in which diffusion can change the chemical composition of any rock.

Other objections to the "metasomatic" doctrine are indicated by the proof of both seismologists and geodesists that the Sial is now largely restricted to one hemisphere. The cause of the restriction is one of the most horrendous mysteries of earth science, one not to be discussed in the present paper. But the fact is beyond cavil and of critical importance. Since the "metasomatic" hypothesis, expressed in any of its chameleon forms, recognizes no need for the emanations to choose some routes upward rather than others, the hypothesis is quite unable to account for the utter lack of granite or rhyolite within the Sial-free earth-sector capped by the deep Pacific ocean, which covers nearly one quarter of the globe. No fragment of either type of acid rock has been found in the countless basaltic flows, and no Sialic mass has been indubitably found in this sector by the seismologists.

A related fact: If the postulated emanations have been so potent as to develop the batholiths of the post-Cambrian orogenic belts, one would expect that in some form or other three million square kilometers or more of the Canadian Archean Shield should exhibit on its anciently-peneplained surface some signs of similar upward invasion; yet no tell-tale sign of new granitization since the late Archean has been found in this vast area.

Read (1948, p. 7) regards "the problem of room" as lying at the heart of the granite problem." He doubtless had in mind the batholiths and stocks of the post-Cambrian orogenic belts. How space for these bodies was secured is truly a vital question, to be later considered. It does not seem to be anything like so difficult to answer in the case of the granite of the Sial, if, as prompted by a reasonable guess, this layer was originally world-circling. Whatever the processes responsible for that vast granite-making, they had "all outdoors" in which to carry on their constructive work. If, on the other hand, the "granitic layer" was formed almost wholly in one hemisphere the colloquialism would not be strictly true. In either case the emplacement of the Sialic granite would not have involved the same kind of structural problem as that so insistent at post-Cambrian batholith or stock. Yet this conclusion does not conflict with the idea that molar movements were essential for the development of both Sial and batholithic bodies.

The writer also holds the opinion that the areal restriction of the Sial is not easy to reconcile with the view of a "magmatist" who believes each body of "granite" to be a direct, single-step differentiate of basaltic magma. During the long lives of the Hawaiian volcanoes the basaltic magma below their vents was chilled countless times to temperatures below which crystal-fractionation should, by theory, have produced "granite" or its effusive equivalent. Yet neither of these rock types has been discovered in the thousand-mile chain of islands. The same is true for some other volcanic piles of the central Pacific. Again, if the basaltic magma under the Canadian "shield" has been slowly freezing since late Pre-Cambrian time, with the direct formation of a corresponding amount of "granitic" differentiate, some of this relatively light material should have risen to the surface or to crust-levels reached by erosion. But there is no record of such upsqueezings since the beginning of the Paleozoic era.

Nor is it likely that the material of the Sial could represent the residual liquid resulting from the crystal-fractionation of a peridotitic earth-shell in a single step. The writer's doubt on this point is based on an application of the Clausius-Clapeyron equation connecting pressure with the temperature of freezing. From this relation it appears that the granitic residuum of crystallizing peridotite would be itself pressure-crystallized before it could be squeezed out of a peridotitic layer with top 40 kilometers or more below the earth's surface.

In view of the geological facts and of theoretical considerations the writer favors the conclusion that the Sial, containing the bulk of the world's granite, was evolved during pre-Archean or early Archean time, and that this layer as well as bodies of post-Archean "granite" were formed by successive founderings, successive remeltings in depth, and successive increases in the acidity of the upsqueezed, relatively light differentiate.

Considering the complexity of the whole process, it is reasonable to suppose that the young "granitic" crust would include rock types ranging from quartz diorite to granite proper as well as bodies of basaltic material which invaded the evolving Sialic crust from time to time. Thus speculatively the primitive Sial covered all the earth. How it could have been driven or pulled into the existing "land hemisphere" is a profound puzzle for "magmatist" and "metasomatist" alike. In whatever way the areal restriction was accomplished the variety of rock types might well have been increased by the action but without changing the essentially "granitic" nature of the layer, a character persisting to the present day.

METASOMATISM IN THE BELTS OF POST-CAMBRIAN OROGENY

We shall now consider the big cross-cutting masses of "granite" with no visible floors which center in the post-Cambrian mountain chains. Whatever their origin, the larger bodies may be conveniently called batholiths and the smaller ones stocks. They have specially significant meaning for the "granite" problem in general, because of the high reliefs of the belts, permitting observation to depths greater than those commonly possible in the peneplained Archean "granitic" shields. For these younger bodies field geology, as Read (1948, p. 2) emphasized, is of crucial importance, and his statement (p. 7) that accounting for them is "at the heart of the granite problem" is, with the qualifications already made, manifestly true. To illustrate the case a personal note is thought to be appropriate.

The present writer met the batholith-stock question squarely in his first serious undertaking in field geology, when, in 1892, he attacked the composite stock at Mount Ascutney, Vermont. There he found: (1) proof of significant feldspathization (incipient granitization) of the phyllites and other schists surrounding the quartz syenite and granite of the mountain—this to a maximum distance of about 100 meters from the sharp contact with the syenite-granite; (2) also plain evidence that the syenite-granite replaced many cubic kilometers of the schists. The "room problem" was at once seen to be serious. He had already studied the writings of the French school, especially those of Michel Lévy, stressing metasomatism with resulting "granite." In 1897 he went to Paris to work under Lacroix, who has done so much to prove the reality of granitization, and the next year saw Sederholm demonstrating the same principle in the field. He also went to Heidelberg to improve his petrographical technique by study with Rosenbusch, who, while showing mastery of such technique, had exhibited little experience in field geology. Rosenbusch confessed to finding the intimate association of gabbro and alkali-rich syenite at Mount Ascutney to be a fact inexplicable according to his general philosophy of "eruptive" or "igneous" rocks. So that then and later, in discussions of alkaline rocks in general, the present writer showed that his student was not entirely docile under the "baleful" influence of the master. The student left Heidelberg, Paris, and Finland with no hint as to a practical answer to the "room problem" at Mount Ascutney. Its difficulty became still more impressive when he mapped in some detail nearly a score of batholiths and stocks distributed along the 600-kilometer Cordilleran section of the Canada-United States Forty-ninth Parallel boundary (Daly, 1912); this during six field sessions, from 1901 to 1906. It was in 1902, about ten years from his first bafflement at Mount Ascutney, that the student found the only promising solution—that of magmatic stoping, a principle which was independently deduced by Barrell (1907) at the Marysville batholith of Montana. After forty-six more years of steady concern over the room problem the writer has found, neither in the geological literature nor in his own additional field studies of big post-Cambrian "granite" bodies in Montana, California, New England, and the Pyrenees, any other reasonable solution. For these post-Cambrian masses replacement of the older rocks is obvious. For these masses all the arguments against the metasomatic origin of the Sial apply with special cogency. For them the emanational-one-way-street hypothesis is, as a matter of pure physics, incomparably harder to accept than the hypothesis of replacement by "granitic" magma, along a two-way street, where stoping, remelting at some depth, and renewed differentiation are assumed.

Incidentally it may be recalled that not only all "metasomatists" but also nearly all the leading "magmatists," from Rosenbusch, Osann, Zirkel, and Teall, to Lacroix, Shand, and Bowen, have disregarded magmatic stoping as an essential process in the generation of the world's "granite," whether Archean (Sialic) or post-Cambrian.

Holmes (1945, pp. 91, 409), a convinced "metasomatist," has raised several objections to the stoping hypothesis. First he notes that with increase of depth xenoliths in "granite" "become gradually smaller and less numerous, and more and more like the granitic rocks enclosing them, until finally they disappear altogether. Evidently they have been incorporated in the granite and the space problem remains." But the "magmatist" has a different answer: the viscosity of his "granitic" magma at the upper levels of the cooling mass increases at maximum speed until the enclosing material attains the small strength sufficient to prevent further sinking, while lower down the liquidity persists much longer and thus permits complete cleansing of xenoliths.

Holmes's second objection reads: "Long 'islands' of country rocks like those of the metamorphic aureole can sometimes be recognized in the granite, their structures remaining in continuity with those of the bordering country rocks, though they may often be no more than vague shadowy outlines, seen through a veil of granitization. Black metamorphosed dolerite dykes, older than the batholith, can sometimes be traced into it as partly granitized 'ghosts.' Since the heavy dyke rocks did not sink, there is no reason to suppose that the associated metamorphosed sediments could have done so." A "magmatist" answer is that the "islands" are roof-pendants, stably supported in the "granitic" melt by their own strength, which with downward tapering of each pendant can be shown to ensure such stability until the whole batholith freezes solid.

The most serious objection is the third: "If granite magma ascends in great bulk towards the surface in this way, it should often have broken clean through the crust to form gigantic volcanoes erupting rhyolite and obsidian and the corresponding pyroclasts. But this has very rarely happened, as the retention of the original roof clearly proves." This difficulty for the stoping hypothesis was fully apparent when the hypothesis was first proposed but the present writer has steadily believed the difficulty can be surmounted. A considerable number of cases are recorded where large bodies of rhyolite, dacite, and obsidian have been erupted in areas where masses of apparently syngenetic "granites" were developed. A remarkable example, recently described by P. Lapadu-Hargues (1947, p. 950), is in the Eglab massif of the western Sahara, where at intervals for 150 to 200 kilometers a huge granite batholith passes continuously upward into broad cappings of rhyolite. It is, of course, conceivable that in many other mountain belts where erosion was prolonged similar rhyolitic cappings were completely destroyed. Nevertheless, it is probable that in perhaps most instances "granitic" batholiths did not break through to the earth's surface. A leading reason for this may well have been the freezing of the magma, chilled by the stoping itself as well as by conduction of the magmatic heat into rock which the stoping magma found to be cooler and cooler as it made its way upward. Perhaps, too, progressive loss of gas and corresponding rise of the freezing temperature is to be considered. On various grounds, therefore, the writer does not regard Holmes's third objection as compelling.

His fourth reads: "If granite magma rose from the depths in quantities corresponding to the enormous volumes of batholiths, it would be by far the most abundant of all magmas. In this case rhyolite should be the most abundant of all volcanic rocks. But it is not." For the "magmatist" "granite" is actually by far the most abundant of the massive rocks in the orogenic belts, the regions now under discussion. We have just found good grounds for doubting the added remark about rhyolite in these same belts.

Holmes (1945, p. 409) is willing to entertain the idea that convection to great depth in the earth may be competent to cause mountain-building of the Cordilleran type. This action, if real, must have resulted from the exceedingly small potential due to relatively small difference of temperature in the thick earth-shell involved. Yet he is averse to crediting the reality of solid-liquid convection to much smaller depth, though the potential is here incomparably greater and the overturn of material incomparably smaller in quantity. One must conclude, therefore, that Holmes in his general review of physical geology should not have had a priori objection to stoping by acid magma to the depth of ten or twenty kilometers. Since more thoroughly than any other geologist he has expressed doubts about the efficiency of magmatic stoping, analysis of arguments against the hypothesis seems not to invalidate it as easing explanation of post-Cambrian batholiths and stocks.

For those bodies the difficulties of the "metasomatists" include some of those already listed when the origin of the Sial was considered, but some of the arguments proffered by members of the school deserve special attention. But beforehand a statement by Bowen (1948, p. 88) at the Ottawa symposium may be recalled: "Though, locally, volume for volume replacement of individual beds may be demonstrated, the claim that wet granitization offers a solution of the 'room problem' is altogether spurious. Material pushed up chemically occupies just as much room as the same material pushed up mechanically. Even though repeated equivolume replacement is postulated, it cannot be repeated *ad infinitum*. There would necessarily be a large net addition of material in the upper crust. Wet granitization does not solve, it merely evades the room problem. The magmatist has, however, found not unreasonable real solutions of the problem."

In whatever way the "metasomatists" account for the earth's "granitic layer," they must admit that, according to their own views, the bulk of each post-Cambrian batholith of "granite" was itself metasomatized Archean (Sialic) "granite" and its own derived sedimentary products. Would "emanations" develop in the post-Cambrian orogenic belts "batholithic" rock-types like those of the primitive earth-shell, which, unless all signs fail, is best represented in visible presence by the oldest Archean terranes? The fact is that, apart from the problematical charnockite and the equally mysterious gneissic structure of these oldest "granites," dominant rock types of the Archean match well with the dominant types in the post-Cambrian batholiths and stocks.

Another trouble for the "metasomatists" in their explanation of these bodies is their comparative homogeneity in the vertical direction, a characteristic often displayed also over outcrop areas measurable in hundreds of square kilometers. An example is the batholith crossed by the Similkameen River of Washington State and British Columbia (Daly, 1912, p. 435). There the mass extends from the highest point in the valley wall down to the river nearly 2000 meters lower down. No floor is to be seen anywhere and the body has lost by erosion an unknown amount of substance above its highest outcrop. Many other batholiths of Alaska, British Columbia, and California afford proof of great thickness and total volume as well as comparative homogeneity, and there can be little doubt that these regions fairly represent the grandeur of the batholithic development along the cordilleran chain stretching from northern Alaska to Cape Horn—the world's most spectacular display of post-Cambrian "granite."

The almost perfect homogeneity of many batholiths to visible depths is by the "metasomatic" doctrine hard to understand not only because of the pronounced heterogeneity of the invaded rocks but also because of the strong probability that, if the emanations were fluids, made "thin" by the presence of much water, one would expect the upper part of each botholith to be specially rich in minerals stable in the presence of much water. Yet there is no field evidence of such differential hydration in the post-Cambrian bodies.

Further, no protagonist of the doctrine has shown why those bodies are confined to orogenic belts or, as already observed, why diffusion could have accomplished the enormous molecular replacement in the limited duration of an orogenic revolution.

Those who emphasize upward diffusion of emanations from great depth cannot doubt that magmatic basalt has risen along each belt both before and after the paroxysm of mountain-building and nevertheless retained the same composition. Yet by the metasomatic hypothesis the basalt itself should have been chemically changed while deep-sourced emanations were passing through it to the high batholithic levels.

Again, the observer exploring the high cliffs of the Cordillera sees that the lateral, steeply inclined contacts of the "granitic" giants as well as the contacts of enclosed xenoliths are commonly if not generally sharp. He notes there the relative slightness of metasomatism. He discovers the never-failing fringe of apophysal dikes from batholith and stock. He may think of the old masters. Will he find more comfort in the vaguely defined "plutonism" of Lyell than in Hutton's exciting demonstration of magmatic granite?

That some metasomatism has accompanied or immediately followed the emplacement of Cordilleran batholiths is clear, but their most experienced students have no doubt as to which is the horse and which the cart. They have found that such molecular replacement is much more conspicuous at the roof of a batholith than along its lateral contacts. The most remarkable example, quantitatively considered, is that reported by G. H. Anderson (1937). After prolonged exploration he explained the "Pellisier granite" of the California Inyo Range as the product of metasomatism of Cambrian and Pre-Cambrian schists by emanations from beneath. If he is right, he has found the largest Cordillera body proved to have been developed by metasomatism. He pictures it as a roof phenomenon. The Pellisier "granite" rests on the Boundary Peak granite, which in the molten, batholithic state gave forth the replacing fluids. From his cross-section one sees that Anderson assigned the remarkable thickness of 600 meters to 900 meters to the "Pellisier granite" while the parent Boundary Peak granite is two or three times thicker, even though no bottom for it appears in the draughted section. He too had no doubt as to which was the hen and which the egg. Like Sederholm and Michel Lévy he felt no need of seeking in mystical depths the immediate source of the working fluid.

CONCLUSION

To summarize: it seems evident that "metasomatists" have left gaps of critical importance in every published version of their hypothesis. They should make more specific: (1) their ideas concerning the nature of the Sial and its mode and time of development; (2) their premises with respect to the state, composition, source, energy, and mode of travel of the assumed emanations; (3) their estimates of the volumes of magmatic "granite" formed by melting of mountain roots, and their estimates of the volumes of "granite" made molten by anatexis; (4) their explanation of the fact that during at least half a billion years no discernible granitization of the earth's crust has been registered under the vast, deep Pacific or within nearly all of the extensive Canadian "shield"; (5) the rôle of crystal-fractionation plus remelt-fractionation in petrogenesis; (6) truly compelling grounds for doubting large-scale magmatic stoping, whether in relation to the earth's "granitic layer" as a whole or in relation to batholiths and stocks of the mountain chains; (7) explanation of the absence of material in the visible part of the earth's crust, corresponding to the "basic fronts" expected by the hypothesis; (8) the meaning of the almost perfect chemical uniformity of basaltic magma erupted since a comparatively early stage of the Archean; (9) the volume and composition of the ocean in relation to the hypothesis; (10) the commonly displayed sharpness of contact of post-Cambrian batholith or stock with the respective invaded formations; (11) the nearly perfect chemical and mineralogical uniformity of so many of these subjacent masses in the vertical direction; and (12) the restriction of typical post-Cambrian bodies of the sort to the mountain chains.

Until all the twelve kinds of argumentative trouble are overcome the "metasomatists" cannot be said to have succeeded in their effort to account for most of the world's "granite". As briefly indicated in the body of the foregoing text, there seems to be a more excellent way, though it too has to be speculative and reach back into the mists of cosmogonic theory.

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23 Hawthorn Street
Cambridge 38, Massachussetts