THE PRESENT STATUS OF DALY'S HYPOTHESIS OF THE ALKALINE ROCKS.

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ABSTRACT. Daly's hypothesis has been attacked by geologists and chemists. It is shown that neither the absence of visible limestone at certain localities nor the absence of visible reaction at other localities constitutes disproof of the hypothesis. The assumption of a "carbonate magma" is believed to be untenable. Experimental chemistry indicates a thermal barrier between oversaturated and undersaturated silicate melts, but the behavior of a hydrous magma is not completely determined by that of an anhydrous melt. The field evidence which alone can decide the question is increasingly favorable to Daly's hypothesis.

I.

A theory, it has been said, is a tool and not a creed. A hypothesis which has withstood criticism for thirty years and has gathered new strength during that period has certainly proved its value as a tool, even if it should eventually be replaced by a sharper instrument. There is no reason to suppose that Daly's hypothesis will soon be replaced, though it may receive a new cutting edge set at a different angle from the original one.

The hypothesis of the desilication of a feldspathic magma by reaction with limestone has faced severe criticism. It was attacked, first of all, by petrologists of the Rosenbusch school who did not believe in assimilation, and for whom "magmatic differentiation" was the key to every problem. It may be claimed as the first fruit of Daly's hypothesis that assimilation of country rock by magma, so long denied or discredited, has been recognized since 1910 to be an important factor in petrogenesis. It is not suggested that all the credit for this achievement belongs to Daly, but the investigations which were undertaken under the stimulus of his hypothesis yielded so much evidence of assimilation that the idea of the magma as a closed system collapsed beneath it. A second victory which followed from the first was the discrediting of the doctrine of "magmatic differentiation" in the original sense of a spontaneous splitting of the magma into contrasted liquid fractions.

These were important gains, but they did not prove Daly's
hypothesis. A high card in the hand of his opponents was that in certain regions where feldspathoidal rocks are present, no limestone is known. This is true, for instance, of Julianeaba (Greenland), Kuusamo (Finland), and Poohbah Lake (Ontario), and it was thought to be true of the Kola Peninsula, where the greatest known volume of nepheline rocks is found. The argument is a strong one, but it is not necessarily fatal to the hypothesis. The genesis of an eruptive rock is a three-dimensional problem and it is not to be expected that the evidence will always be available in two dimensions. How little we know of the rocks beneath our feet, even in a familiar sedimentary formation, is shown by the history of the East Texas oilfield, where a great lens of sandstone charged with the biggest accumulation of oil ever tapped was wholly unsuspected until it was penetrated by a wildcat well. Crystalline limestone is a common element of the Archaean in many parts of the world. When we do not see it at the surface are we entitled to deny its presence underground? If limestone seemed to be lacking in a large number of instances then the theory would indeed be discredited; but as the record now stands about 95 per cent of all known bodies of feldspathoidal rocks are in regions where limestone is present. This majority does not prove the hypothesis, but neither does the small minority of unfavorable cases disprove it.

Rather than admit the possibility of an unknown body of limestone underground, some petrologists have preferred to put their faith in hydrothermal solutions, in alkaline emanations, or even in that most strange thing, a carbonate magma. N. L. Bowen showed that alkali-feldspar has been replaced by hydrothermal carbonates in the Fen district of Norway; but Bowen realized what others have overlooked, that it is no easier to find a source of calcium carbonate solutions than to find an actual body of limestone. In a recent discussion of the katamorphism of igneous rocks, Bowen wrote(1)* "an abundant source of CO₂ is required, and the only possible one appears to lie in carbonate rocks suffering metamorphism." This admission, it seems to the writer, virtually awards the Fen occurrence to Daly with the endorsement of our leading petrological chemist.

Where indeed could the necessary amount of calcium carbonate come from, if not from an underlying body of limestone?

* Figures in parentheses refer to the bibliography.
At Iron Hill, Colorado, according to data furnished by E. S. Larsen(2), the visible part alone of the main carbonate body must amount to about one-fifth of a cubic mile, or more than 2,000,000,000 tons. If this was not derived from a sedimentary or metamorphic limestone then where did it come from? Not from the associated pyroxenite and feldspathoidal rocks, for these are younger than the carbonate.

Trying to solve a problem by means of hydrothermal solutions or alkaline emanations, without indicating a reasonable and adequate source of these things, is like issuing a check when there is no balance in the bank. It might be said that Daly himself wrote a check to cover the few occurrences of alkaline rocks in places where there is no known limestone, but it must be remembered that Daly’s checks have been honored in 95 cases out of 100.

The supporters of the “carbonate magma” hypothesis are in a position to issue checks upon either of two banks. If they draw upon the sedimentary bank, that is, if they derive their magma by mobilization of a pre-existing limestone, then their demands may be met; although in that case they have not produced a new hypothesis but just an interesting and valid extension of Daly’s hypothesis. That limestone may be mobilized and intruded along with a very fluid feldspathoidal magma has been convincingly demonstrated in the Bancroft district, especially by the recent work of F. Chayes(3). But if they offer us a carbonate magma which is not of sedimentary derivation, then they are operating on an imaginary balance and do not deserve our confidence.

A carbonate magma—that is, a body of calcium carbonate above its melting point of 1340°—would be a highly explosive liquid, requiring a confining pressure of more than 1000 atmospheres(4). Yet in the Chilwa vents of Nyassaland(5) and in the Kaiserstuhl (Baden), crystalline limestone is found in association with lavas and tuffs. It must have reached its present position under little more than atmospheric pressure, yet it did not explode. Of course by adding a sufficient quantity of water to the imaginary magma one might bring the temperature of fluidity down to a more convenient level; but it would still be necessary to find a reasonable source for the strange brew.

Is it legitimate to use the unknown interior of the earth as a conjurer’s hat to produce whatever solutions or emanations
our speculations may demand? Hypotheses of this character
do not weaken Daly’s case but by their very extravagance make
it seem simple and reasonable.

Another argument which is often advanced as if it were fatal
to Daly’s hypothesis is that there are many contacts between
granite and limestone which fail to show any sign of reaction.
Negative evidence of this nature is worthless in the face of pos-
itive evidence from other contacts. In Burma, for instance,
a body of syenite near the village of Oongain encloses bands of
limestone(6), yet the syenite in the neighborhood of these bands
shows no unusual feature. In the same district there are veins
of syenite cutting limestone, and about the contacts there
appears a coarsely crystalline rock composed of nepheline,
diopside, calcite, feldspar, and apatite. This conflict of evi-
dence makes it clear that Daly’s reaction, like any other reac-
tion involving a potential gas phase, depends on a delicate bal-
ance between temperature and pressure, to which we should add
as a third factor the concentration of water in the magma, since
water is the medium through which the reaction takes place.

Daly did not claim that desilication must take place at every
contact of magma with limestone; only that it did take place
in those instances where feldspathoids were formed.

II.

It is from the chemists rather than the petrologists that the
really serious opposition to Daly’s hypothesis has come.
About the year 1910 it did not seem absurd to suppose that a
magma rising into the sedimentary crust might be superheated
to the extent of one or two hundred degrees, and therefore be
capable of effecting considerable solution of its walls. But
“geological thermometer” studies soon demonstrated that gran-
ite and even diabase, at the time of intrusion into the crust,
are less highly heated than we had supposed and cannot have
much superheat. Bowen has added the further demonstration
that the silication of calcite and dolomite is an endothermic
process. How then can a granitic magma dissolve limestone?

The answer is perhaps that the portion of the magma which
enters into reaction with its limestone walls is always a very
small fraction of the whole, and that its temperature may be
maintained for a long time by the outflow of heat from the
main body of magma. For instance, the entire volume of felds-
pathoidal rocks in the Bushveld complex can hardly exceed 50 cubic miles; but the complex contains 5000 to 10,000 cubic miles of granite. W. C. Brögger computed that the nepheline rocks of the Christiania district make up only 0.3 per cent of the bulk of that complex. Another illustration has been furnished lately by A. Quinn (7) who showed that the feldspathoidal rocks of Red Hill and Pleasant Mountain make up less than one per cent of the White Mountain magma series of New Hampshire.

But we are not yet out of the wood. Bowen and Schairer (8) have now completed an experimental study of the system nepheline-kaliophilite-silica. They find that the composition point of albite forms a crest on the nepheline-silica curve, and from it a low ridge runs across the field of the soda-potash feldspars toward the composition point of orthoclase. The crest-line of this ridge forms a barrier between the region of quartz-bearing rocks and the region of feldspathoidal rocks. It follows that an oversaturated liquid, represented by a point in the former region, cannot be desilicated to the extent of forming nepheline or leucite unless it contains enough superheat to carry it over the top of this barrier, the lowest point on which corresponds to a temperature of 1076°. The average height of the barrier, compared to the quartz-feldspar eutectic line, seems to be between 60° and 70°, but exact figures are not yet available.

If to the above quantity of superheat we add the heat absorbed by the endothermic reaction with limestone, and a further amount to compensate for chilling of the magma in the act of intrusion, then the total demand for superheat becomes so formidable that it may well seem fatal to the hypothesis.

But the data presented above refer to an anhydrous system and must be modified by the presence of dissolved water and CO₂. R. W. Goranson (9) has shown that a melt of albite containing only 4.2 per cent of water does not begin to crystallize till the temperature falls to 960°, almost 160° below the freezing point of an anhydrous albite melt. Since all natural foyaitic magma seems to have a high content of water and other fugitive substances, it may be that the presence of these substances has such an effect on the configuration of the feldspar barrier that it is no longer an obstacle to desilication. Experimental evidence being incomplete in this respect, the field evidence deserves all the more attention.

Observe first of all that the existence of the feldspar barrier
does not in any way affect the desilication of granitic magma to form syenite (without feldspathoids). There is no theoretical objection to this process, which seems to be demonstrated at Bancroft, Loch Borolan, Ditró, Sviatoy Noss, and Palabora. Nor does the feldspar barrier present any obstacle to the desilication of foyaitic magma to form urtite and ijolite; this process is not only theoretically possible but is is beautifully demonstrated at Ice River, Magnet Cove, Sekukuniland, Alnö, Fen, and Jacupiranga. The barrier operates only to prevent the passage of an oversaturated (granitic) magma into an undersaturated (foyaitic) magma or rock, and conversely that of an undersaturated magma into an oversaturated magma or rock.

The situation is then that Daly’s hypothesis seems to split into two parts, each of which is theoretically possible and actually demonstrated in nature, but the junction between them remains in doubt. For this reason we must look very carefully at the evidence which has suggested a gradation between oversaturated, saturated, and undersaturated rocks in the field.

III.

A perfect gradation from oversaturated to undersaturated rock could only be realized if the magma remained stagnant during the entire period of desilication and subsequent crystallization, the transfer of silica being effected by diffusion alone. Such tranquil conditions are not to be expected in nature. Magmatic injection must generally be a turbulent, spasmodic process, involving fracturing of rocks, forceful injection of magma, stoping, convection currents, gas currents, and the squeezing out of residual liquids at various stages from the slowly crystallizing mass. That a perfect gradation of composition could be preserved in rocks formed under such conditions is almost unthinkable. Nevertheless a few examples of apparent gradation are well established.

The oldest illustration is at Loch Borolan. It was the opinion of J. J. H. Teall that in this locality “the quartz-syenites shade into the quartzless syenites and these again into the nepheline-syenites.” The present writer traversed this small area again and again and found no evidence of any discontinuity in the succession, at least down to the level at which quartz has disappeared and the first small quantity of nepheline or sodalite has made its appearance.
In the Ilimsausak batholith in western Greenland, N. V. Ussing found that arfvedsonite-granite, quartz-syenite, plaskite, and foyaite "lie as almost horizontal strata above one another without any sharply defined contact between them."

In the third volume of his great work on the Christiania district, W. C. Brögger stated that the nepheline-rich foyaites pass into nepheline-poor or nepheline-free hedrumite, defining the latter as a variety of plaskite with trachytic structure. Elsewhere he said that the plaskites grade into the quartz-bearing nordsmarkites. Unless there are two kinds of plaskite of different origin the evidence would seem to indicate a gradation from foyaite through plaskite into nordsmarkite.

In the Ditro complex, in Transylvania, A. Streckeisen (10) reported that granite, syenite, foyaite, alkali-diorite, and hornblendite are so intermingled as to make detailed mapping impossible. The older nepheline-syenite shows gradual transition into syenite and granite; the younger foyaite is intrusive.

At Mariupol, on the sea of Azov, A. S. Guinsberg (11) relates that "a small nepheline-syenite outcrop is associated by gradual transition with alkali-syenite and granite."

On the authority of T. T. Quirke (12) the syenite ("rut-terite") of French River, Ontario, "grades both into the quartz-bearing rocks and into those with nepheline. It occupies a truly intermediate position between the two groups."

A close study of the supposed transition has been made in the Haliburton-Bancroft district, where Adams and Barlow indicated a gradation from granite through syenite into foyaite at several points. Their opinion was upheld by W. G. Foye. The evidence in this region was lately re-examined by the writer in conjunction with F. Chayes. It appeared to us that the relation described by Adams, Barlow, and Foye is true in a broad sense; yet when studied in detail it is not a simple gradation but rather an oscillation of quartz-bearing, quartz-free, and nepheline-bearing facies without distinct boundaries between them. Such a condition might result either from the imperfect mixing of two liquids or from differential movement between a semi-solid mass and a residual liquid.

Sometimes evidence of transition is lacking, but instead there is a definite sequence of intrusion of oversaturated, saturated, and undersaturated magma. In the Christiania district, according to Brögger, the order of intrusion was one of increasing silica content. At Mariupol, on the other hand, Moroze-
wicz(13) found the order to be one of decreasing silica and increasing density.

The Berdiaush pluton, in the Urals, is intrusive in dolomite. It consists of granite, syenite, and foyaite which were intruded successively in that order, with a concentric arrangement (Zavaritzky).(14) A confocal arrangement resulting from the successive intrusion of quartz-syenite, syenite, and foyaite appears in the Leeuwfontein complex in the Transvaal.

Red Hill, New Hampshire, is another of these remarkable concentric stocks. The outermost facies is a coarse syenite with no more than 5 per cent of quartz. It is followed inwards by a foyaitic facies in which nepheline first increases and then decreases again; and the core of the structure is composed of quartz-syenite and granite. A. Quinn(7) claims that “the course of differentiation was from syenite to feldspatoid-syenite, to syenite, to quartz-syenite, to granite.”

The association of quartz-bearing, quartz-free, and nepheline-bearing rocks in a single outcrop only 150 yards long was recorded by A. Osann at Cevedaes, Portugal; and Daly found the same association of rocks under similar circumstances at Litchfield, Maine.

**IV.**

The evidence summarized above suggests rather strongly that the Bowen-Schairer barrier, separating the quartz region from the nepheline region, may be crossed in nature. The only alternative explanation of the phenomena would be hybridism between an oversaturated and an undersaturated magma; but an advocate of this view would be under the necessity of explaining how two wholly incompatible magmas came to exist in the same place at the same time.

A composition gradient such as we have described might result from the silication of an undersaturated magma as well as from the desilication of an oversaturated one. The marginal syenitic facies about the Alnö and Umptek intrusions are believed to be due to the attack of foyaitic magma on siliceous wall rocks. At Ahvenvaara, Finland, and also in the Fen district of Norway, an ijolitic magma has reacted with granitic country rocks to form a narrow zone of syenite; and at Korok, Greenland, an undersaturated magma has saturated itself by dissolving sandstone. In these instances we see the destruction of a feldspatoidal magma; its death, but not its birth.
They show us that the Bowen-Schairer barrier may be crossed in the direction of increasing silica, but they give us no clue to the origin of the feldspathoidal magma.

E. H. Kranck (15) assumed an ijolitic magma as the parent of the alkaline rocks at Cape Turja (Turij) on the Kola Peninsula, and supposed the various rocks to have been derived from it by the removal of olivine, pyroxene, and nepheline at successive stages, leaving a residual liquid rich in alkalis and carbonic acid. H. G. Backlund (16) adopted Kranck’s idea and extended it to other regions where alkaline rocks are associated with carbonates. Yet it is plainly impossible to reconcile Backlund’s hypothesis with the evidence at Iron Hill and Chilwa, where the limestone is older than the silicate rocks; at Bancroft, where the intrusive carbonate has been injected along with the foyaite; or at Ice River and Jacupiranga, where the limestone associated with the ijolitic rocks is clearly sedimentary. Backlund did not try to explain how his hypothetical magma acquired the enormous concentration of calcium carbonate which would be necessary to generate the great limestone bodies at Iron Hill, Fen, Chilwa, and Sekukuniland.

Any hypothesis which assumes a primary undersaturated magma simply begs the question. It does not explain how a magma of such unusual composition came into existence, or how a liquid so sensitive to silification could ever survive its intrusion into the predominantly acid rocks of the lithosphere.

In short, there is no hypothesis which fits the field evidence so well as Daly’s, or makes so few and so reasonable assumptions. Every one of the three stages in the process of desilification pictured by Daly, namely the reduction of granite to syenite, of syenite to foyaite, and of foyaite to ijolite, is supported by field evidence which hardly admits of any other interpretation than the one under discussion.

V.

This does not imply that all feldspathoidal rocks were formed in the same way. The nepheline-plagioclase rocks, for example, may have a different mode of origin from the foyaites. In his earliest statement of the hypothesis Daly favored basaltic rather than granitic magma as the parent of certain alkaline rocks. It is true that many feldspathoidal lavas are associated with olivine-basalt; but very few of the holocrystalline nepheline rocks show an intimate relation to gabbro.
The Salem gabbro, in Essex County, Massachusetts, was already solid when the Quincy granite came into place beside it; and the foyaite which followed the granite invaded and metamorphosed the gabbro, converting it into essexite. There is no suggestion of a genetic connection between gabbro and foyaite.

The Bushveld complex is equally unfavorable. Gabbro or norite is the most abundant rock in the complex; but it was solid before the red granite followed it, and the foyaite in turn cuts the red granite.

The alkaline complex of Ampasindava, Madagascar, contains certain rocks which have been described as amphibole-gabbro and nepheline-monzonite or essexite. The gabbro, which holds large poikilitic plates of hornblende (a sure indication of post-magmatic change), is cut by veins of nepheline-syenite; and both gabbro and essexite have been brecciated and the fragments enclosed in a syenitic matrix. A. Lacroix suggested on the basis of microscopic study that the foyaite of this locality grades into essexite, nepheline-gabbro, and gabbrodiorite; but field evidence of the supposed transition is lacking. The description of the rocks reminds one of Essex County and makes one suspect a similar history of metamorphism and hybridism of the gabbro by the foyaitic magma.

The derivation of foyaite from basaltic magma by reaction with limestone was favored by Streckeisen(10) at Ditró, Transylvania. Closely intermingled with the alkaline rocks in this complex are certain curious rocks which are termed alkali-diorite or orotvite. These are similar to the rocks which other petrologists call essexite, and they are intimately veined by syenite and foyaite, so a comparison with Essex County is again invited.

A. S. Guinsberg(11) believes that the alkaline rocks of Mariupol were generated “by the mixture of a basalt magma with limestone, followed by a differentiation into alkaline rocks and pyroxenite.” But this would seem to be a concession to Daly rather than a deduction from the field evidence, because no limestone and little gabbro is exposed anywhere in the district of Mariupol.

The discovery of veins of nepheline-pegmatite cutting a body of diopside-pyroxenite at Afrikanda, in the Kola peninsula, has suggested to B. M. Kupletsky(17) that the pyroxenite was formed “by an ultrabasic magma which assimilated carbonate
rocks, and that the nepheline rocks represent the residual magma." The only evidence suggestive of an ultrabasic magma is the presence of pyroxenite; yet is is well-known that diopside-pyroxenite may be formed by reaction between acid magma and limestone, so the assumption of an ultrabasic magma is unnecessary.

There are, however, two clear and indisputable examples of the formation of nepheline-bearing rocks by the action of basaltic magma on limestone. One of them is the Scawt Hill occurrence described by Tilley (18) in which a very small body of theralitic rock was formed at a contact between olivine-dolerite and chalk. Tilley maintains that this occurrence "is to be taken as an example of the restricted potentiality of igneous magma to generate alkali types by assimilation." In reality it merely shows that the rate of cooling was too rapid for extensive desilication to take place, and that little nepheline can be expected from a soda-poor and relatively anhydrous magma.

The other instance is on a much larger scale, but the details, unfortunately, are only accessible to those who can read Russian. G. M. Saranchina (19) has described an occurrence of nepheline-melilitite-garnet rocks in the contact zone between gabbro and limestone on Patin Mountain in Western Siberia. B. M. Kupletsky says (translation) "without doubt these rocks were formed during the assimilation of carbonate rocks by the basic magma."

CONCLUSION.

Unless a large number of observers are all at fault, we must believe that granitic magma does sometimes suffer desilication by reaction with limestone, to the extent of generating a relatively small amount of foyaite; that syenitic and foayitic magmas may be further desilicated to urtite and ijolite; and that basaltic magma may under similar conditions give rise to rocks such as theralite and nepheline-melilitite rocks. But there is no basis either in Daly’s hypothesis or in chemical theory for the assumption that these reactions must take place wherever magma and limestone come in contact.

The final test of any hypothesis is that it shall not only explain what is known but shall succeed in predicting the unknown. In 1910 Daly cited many occurrences of alkaline rocks in regions where no limestone was then known. Some of these occurrences are on islands, such as Cabo Frio (Brazil)
and the Los archipelago (West Africa), and in these instances the evidence may be hidden under the sea. Other localities are obscured by glacial deposits (Kola peninsula; Kuusamo and Kuolajärvi in Finland); others by lake, swamp and forest (Poohbah Lake); others by deep tropical weathering (Madagascar, Burma); and the great Greenland mass is partly covered by the continental ice sheet. Where such conditions prevail it seemed unlikely that the evidence required by Daly's hypothesis would ever be found. Yet in 1925 V. Hackman (20) succeeded in locating a considerable body of crystalline limestone at Kuolajärvi; and even in the Kola peninsula, which used to be cited by Daly's opponents as their crowning argument, recent studies have revealed bodies of pyroxene-carbonate rocks and carbonatite, as well as a sedimentary limestone horizon in the Karelian system (21).

Perhaps Daly's hypothesis has still to undergo a final metamorphosis before it takes a form that will satisfy all its critics, but already it has abundantly proved its value as an instrument for the increase of knowledge and the promotion of discovery.

POSTSCRIPT (December 1944)

In the short time that has passed since this paper was written, two new discoveries have been announced which are highly favorable to Daly's hypothesis. In the Records of the Geology Department of Mysore (vol. 41, 1943, p. 18) there is an account of a gabbro-limestone contact at which nepheline, leucite, and melilitic are all developed; the principal product of reaction is described as a "melilitic-nepheline-calciphylite." A brief account of a new occurrence of ijolite and other nepheline rocks, associated with intrusive carbonatite in Kavirondo, Kenya, appears in the Proceedings of the Geological Society of London for 1944, p. 101. Wollastonite and melanite are prominent contact products.

With the kind help of Miss Helen V. Elias as translator, the writer has learned of many alkaline intrusions which have been described in recent years in little-known Russian publications. Besides Berdiaush and Patin Mountain, to which brief reference was made above, there are occurrences of feldspathoidal rocks at Kaindy river, Erie-su river, Tagobi-Sobak river, Botogol hill, Kosogol lake, Tatarka river, Bulankul lake, Saibar mountain, and in northern Armenia. In each case the alkaline rocks
are intrusive in limestone; they contain xenoliths of marble and lime-silicate rocks; and such significant minerals as melanite, cancrinite, melilite, monticellite and wollastonite have been developed by reaction. Miss Elias and the writer plan to make these observations available in English.

References.

(Only the more recent and less familiar references are given here. Others may be found in Professor Daly's writings).


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