

5. Some notes on the differentiation problem.

By

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In 1892 IDDINGS first introduced the type of graphic representation now commonly known as variation diagrams. Their usefulness for petrological interpretations was especially emphasized by HARKER (1909), and in still later years BOWEN (1928) has discussed rather comprehensively their significance with special regard to their bearing upon the problem of rock genesis.

In the original variation diagrams the molecular percentages (IDDINGS) or the weight percentages (HARKER) were plotted against silica. A modification was introduced by NIGGLI (1920) who reverted to a molecular basis and, furthermore, substituted his »condensed» values *al*, *fm*, *c*, *alk* etc. for the oxide numbers. This no doubt added highly to the perspicuousness of the diagrams as far as silicate rocks are concerned but involved no change of principle. The following brief discussion, therefore, pertains as much to the NIGGLI curves as to the »classical» variation diagrams.

In IDDINGS' original paper the diagrams were used in a qualitative rather than a quantitative sense. Curves were constructed for six different American and European petrographic provinces, and the chief aim was to establish that within each of these provinces the rocks have certain distinct chemical features in common as contrasted to the rocks of the other provinces. Very little use was made of the curves, however, when it came to explaining the trend and the mechanics of the differentiation within each province. Indeed, the irregular shape of the curves was considered by IDDINGS to support his conception of a molecular rather than a mineralogical differentiation. In other words, the evidence of the diagrams was considered so negative, that one had to resort to an explanation that must reasonably be expected to yield highly irregular results.

HARKER (*loc. cit.*) ventured a step further. He vindicated that a rock series, the members of which are connected by differentiation of the same kind, will give a variation diagram of tolerably smooth curves. »More considerable discrepancies must be taken to indicate that the variation is not in fact wholly on one definite line but is complicated by the effects of a subsidiary

differentiation. In this way the variation-diagram becomes a criterion of the simplicity or complexity of the variation which it represents.» (p. 123). A number of actual examples appear to support this conclusion. It is quite evident, then, that HARKER was aware of a certain relationship between the form of the variation curves and the process of differentiation. It was not until much later, however, that BOWEN (*loc. cit.*) was able to demonstrate the genetical implication of this relationship. As the fundamental principles in BOWEN's genetical interpretation of the curves appear to be unknown to or are at least frequently overlooked by some petrologists (*cf.* for instance V. ECKERMANN 1937 p. 279 ff.) it seems desirable to call attention to the most important of them.

As is well known, BOWEN's deductions start from the fundamental assumption that the evolution of the igneous rocks is wholly governed by crystallization-differentiation. In view of many recent experiences the general validity of this assumption must be considered rather doubtful, but it seems to be quite evident that in a great many cases this process has been at work. As a matter of fact a magmatic differentiation that is not intimately connected with crystallization is very difficult to conceive and it seems quite justifiable, consequently, to consider »magmatic differentiation» as synonymous with »crystallization differentiation». For the moment, then, we will disregard the important question as to what extent the evolution of rocks is a result of magmatic differentiation and to what extent it may have been controlled by other processes. We will confine ourselves to the problem in what manner the variation curves depict the process of differentiation.

Starting from an initial magma the separation of any mineral or mineral assemblage whose composition does not exactly correspond to that of the melt will imply a change in the composition of the latter. Consequently, during the process of crystallization the composition of the residual melt will be changing through an infinite number of successive stages until complete solidification puts a stop to further changes. Now it is quite evident, however, that crystallization alone can in no conceivable way bring about a change of the bulk composition of a magma portion. To that effect movements of the crystals relative to the residual melt will have to take place. Such a crystal fractioning may be brought about in several ways (by gravitation, by squeezing etc.) but it is of utmost importance to realize that the resulting differentiation is controlled by two mutually independent factors, *viz.* firstly the stage of evolution of the melt and, secondly, the degree of fractioning.

The first of these factors is represented by the path along which, on crystallization, the successive changes of the residual melt have taken place. This path was designated by BOWEN as the liquid line of descent and may in fact be considered as a new and more useful conception of the old

variation curves. Its form and position is entirely controlled by physico-chemical laws and, knowing the $p-t-x$ -conditions in any given case, we should be able in simple systems to deduce it in a purely theoretical way. For the generally rather complex magmatic systems, however, we have to resort to empirical data and try to construct this line by the aid of chemical analyses. For this purpose only such rocks will do, whose bulk compositions correspond exactly or almost exactly to that of anterior melts, a condition which can as a rule be irrefutably established only for aphanitic or nearly aphanitic rocks without phenocrysts. In any given case, however, a microscopical investigation may decide whether or not even other rocks may be considered to fulfill this requirement. By such a critical selection of analyses the liquid line of descent may be constructed and it has been found that it comes out as a fairly smooth curve. It ought to be commonly known that by omitting all rocks that were obviously the result of crystal accumulation etc. BOWEN (*loc. cit.*) was able to reduce the very irregular variation diagrams of for instance the Katmai volcanic region to curves of excellent smoothness.

The liquid lines of descent may in fact be considered to represent differentiation curves at complete fractionation. If a series of eruptions from a cooling magma-chamber should happen to produce nothing but molten material that had at every successive stage been completely decanted from crystals of intratelluric solidification, then the differentiation displayed by the resulting rock-suite would be depicted by the liquid lines of descent.

The first of the above-mentioned factors governing differentiation may thus be considered to be an expression of rational physico-chemical laws and, accordingly, it is to some extent capable of being deduced. The second factor on the contrary — the degree of fractioning — is very complex and entirely irrational. It is controlled by so many independent variables — gravity, viscosity, orogenetic forces etc. — that we cannot reasonably hope to illustrate the possible variations by a curve that would be of any help in following the main trend of magmatic evolution.

So, for instance, during the cooling of a basaltic magma a gravitative settling of, say, olivine will produce in the bottom parts of the magma chamber a crystal porridge or perhaps, by refusion, a melt, the composition of which will depart conspicuously from the liquid lines of descent. The Fe- and Mg-values will rise above the curves, whereas the other oxides will be shifted downwards. A similar result would ensue if for instance plagioclase were gravitatively enriched, presumably in the upper parts of a magma. In this case the resulting rock would show an Al_2O_3 -value far above the corresponding curve, CaO and Na_2O would or would not show any conspicuous departure from their curves and the rest of the oxides would as before be shifted downwards. It is by no means impossible that

the mineral assemblage that is gravitatively accumulated may have about the same silica content as the melt into which it arrives. At any rate, by the cooperation of two gravitative processes — say, that the sinking of olivine is accompanied by the rising of plagioclase — it is perfectly possible that the total silica percentage may remain essentially unchanged. Consequently, a rock formed from the pure melt can very well come to have the same abscissa in the variation diagram as one formed by crystal accumulation in the same melt. The projective points of the other oxides will, however, show more or less conspicuous deviations.

These fundamental facts were brought out elaborately by BOWEN in the early twenties and were exhaustively treated in his excellent text-book on the evolution of the igneous rocks (*loc. cit.*). They have recently been rediscovered by V. ECKERMANN, who proposes for them a new term, *viz.* »vertical differentiation». The term seems extremely inappropriate as it implies a rather inconvenient mixing of metaphors — one part pertaining to a graphical representation, the other to a process in nature. This is of minor importance, however, for as I will show, there is really no need to use the term at all.

This »vertical differentiation» was first announced by V. ECKERMANN in connection with his studies on some alnöitic dikes of S. Norrland (1928) and was again treated in a more recent paper (1938). As a matter of fact the two phenomena explained by »vertical differentiation» have nothing in common. I will presently return to the earlier of the papers.

In his paper of 1938 V. ECKERMANN is led by his field observations to conclude that the anorthosite of the Nordingrå region has been derived from the Jotnian magma by gravitative magmatic differentiation. When entering its analytical data in the differentiation diagram of this series, as deduced by him, he finds, of course, that they fall far from the smooth curves. Nobody would indeed expect a labradorite analysis to coincide with that of an average basaltic rock of the same silica percentage. This deviation is, therefore, completely in accordance with all experience. If the mutual relations of the Jotnian igneous rocks are controlled by magmatic differentiation, then the analyses of those of them which have once been true residual melts, will fall along tolerably smooth curves as argued above. Such types, however, as are generated by crystal accumulation — as vindicated by V. ECKERMANN for the anorthosite — will depart more or less from these curves as was also at some length explained above. I cannot see, however, why this should induce us to introduce still another term, *viz.* differentiation axis (*loc. cit.* p. 281), which only serves to make a simple thing rather mysterious. The case is simply that the *sz*-values of basic plagioclase range from 100 (anorthite) to 145 (labradorite $Ab_{35}An_{65}$). Now anorthosite is by definition an all but monomineralic rock consisting of preferably basic plagioclase. Differentiation or no differentiation, therefore,

anorthosites tend to have si -values falling between these extremes, whereas their al , fm , c , and alk will depart considerably from the general curves of evolution within this interval. In this way every anorthosite analysis will necessarily give a »differentiation axis» located somewhere between $si = 100$ and $si = 145$. Now v. ECKERMANN attaches much weight to the fact that all his four analyses of anorthositic rocks fall at practically the same si -value, which he considers to prove the stability of the »differentiation axis»:

»In the case of the Nordingrå anorthosites such an axis is very much in evidence, too, at $si = 140$. It is located within a very narrow si -range, which, of course need not necessarily be expected to correspond to the si -axis of anorthositic differentiation of basic rocks belonging to different magma-provinces. Composition, temperature and pressure of the respective magmas may shift the si -value of the axis, but I suggest that in each different case it will be found to be practically stable. A necessary presumption is, of course, a differentiation under orogenetically undisturbed conditions. Recalculating, for instance the analyses of Bushveld published by HALL a similar differentiation-axis is found to lie at about $si = 120$.» (1938 p. 282).

I am quite unable to see why a highly mysterious »differentiation-axis» should have to be introduced only because four rocks of obvious petrological similarity come out to show the same si -values. As a matter of fact this »differentiation-axis» or, simpler, this departure from the curves only tends to show that the anorthosite and the kenningite do not belong to the main suite of evolution but are generated by some subsidiary process. It may very well be that the rocks on the smooth curves represent the results of differentiation at complete fractionation whereas the anorthositic rocks owe their formation to the subsidiary process of crystal accumulation. This seems to be v. ECKERMANN'S opinion, and it is not the aim of this paper to dispute it. As far as the evidence of the curves goes this assumption is in no way contradicted, but I am anxious to stress that nor does this evidence give any proof of its correctness. The curves only tell us that two different processes have been operating — one following the smooth curves, the other causing departures from them.

In this connection a few words should be added concerning the rather peculiar conclusions that have been drawn by v. ECKERMANN from his recent discovery of such departures. In an earlier paper (KROKSTRÖM 1937) I have suggested that a certain caution is not out of place when using the differentiation curves: »Now experience seems to have brought out that the projective points of a series of consanguineous magmatic rocks tend to fall along simple and approximately rectilinear curves, whereas a markedly aberrant analysis suggests that the rock in question does not belong to

the *main* suite. The reverse conclusion is, however, not valid. If an analysis fits in with the differentiation curves we are entitled to conclude that the chemical relations do not speak *against* its belonging to this series, but no decisive proofs whatever may be gained from this fact», (*loc. cit.* p. 271, italics inserted here).

In other words, I wanted to stress the possibility that a rock without being in any way genetically connected with a certain series of mutually consanguineous types may nevertheless fit the curves of this series. This fact was illustrated by some actual examples. Now v. ECKERMANN argues that some of the rocks of an indubitably consanguineous series may very well depart from smooth curves. This is entirely in accordance with my own views as expressed above, but I utterly fail to see how this fact can in any way tend to strengthen the reliability of those curves. Translated into plain language the argumentation can be illustrated by the following dialogue.

K. »I think we should not judge a mineral by its birefringence only.

A green mineral of low birefringence is not necessarily a chlorite.»

E. »Well, there are chlorites of quite high birefringence. That should warn you not to doubt the diagnostical significance of this quality».

By this devastating logic v. ECKERMANN arrives at the conclusion that I am discussing a problem »without a sufficient knowledge of the subject». In the interest of decency I shall refrain from returning this graceful compliment to its author.

In order to find any logic in v. ECKERMANN's argument we are forced to the conclusion that he wants to include even the aberrant analyses in the differentiation curves, thus rendering them a sort of unintelligible zigzag lines. As a matter of fact he has adopted this method in his ordinary variation diagram (1938 fig. 13) although not in the corresponding NIGGLI diagram (fig. 12). Such a tendency must, however, be strongly objected to, as it would, at last, bring about unlimited anarchy. That would imply that if a series of analyses should fall along smooth curves we would consider this fact as speaking in favour of differentiation, whereas, if several of the analyses should happen to depart from such curves, we would calmly announce that »vertical differentiation» with one or several »differentiation axes» had been operating, and we would still feel entitled to consider the result as speaking in favour of differentiation. It appears to me that in such a case the very »*raison d'être*» of the variation diagrams — except merely as graphical records — would have to be seriously questioned.

I should like to quote a few lines from BOWEN (1928, p. 92): »We shall find that the answer to this and to similar questions is forthcoming when variation diagrams are examined with the problem ever in mind of trying to decipher from them the changing composition of the liquid and of eliminating from them such complications as are imported by the fact that

not all rocks correspond in composition with a possible liquid». As was stated earlier I am not inclined to accept BOWEN's rather extreme views as to the all-important rôle played by crystallization-differentiation in rock genesis. I think, however, that *mutatis mutandis* the above quotation still expresses in principle the essential features of our problem. We will have to examine the variation diagrams with the purpose of deciphering from them the main trend of evolution and of eliminating, in the beginning, such complications as are due to subsidiary processes. If a certain process — be it differentiation, be it granitization, be it hybridization — has been continuously at work, then it seems extremely probable that it should have effected an evolution along reasonably smooth curves. Our problem is, in the first instance, to locate these curves and to try to find out from them, if possible, the nature of this process. Complications due to subsidiary processes will then have to be taken into account and interpreted, but our chief aim must be to hold apart the effects of different factors that may have been operating independently.

A rather good illustration of this necessity is afforded by v. ECKERMANN's paper of 1928, where his conception of »vertical differentiation» is for the first time announced. In this paper a number of separate, mostly carbonatitic dikes along the East Coast Railway were described. They show a marked affinity mutually and with some rocks from the Alnö region, and v. ECKERMANN concludes that they all represent products of magmatic differentiation. It is not quite evident by which means he considers the differentiation to have been effected but some of his expressions certainly suggest the idea of liquation (*cf.* quotation below).

The analyses were plotted in a NIGGLI diagram which is reproduced as fig. 1 here, and a number of rather complicated differentiation curves were constructed. The author concluded that from a »differentiation centre» at $si = 60-63$ »the magma . . . differentiates . . . either horizontally or vertically, following the arrows, into an ultrabasic and an acid or a femic and a calcic partmagma.» His conception is further illustrated by a schematical diagram which is also reproduced here (fig. 2). It would appear from the above expressions that the idea of liquation was in the author's mind, whereas in the case of the Nordingrå anorthosite the same »vertical differentiation» was explicitly stated to have been effected by gravitative crystallization-differentiation.

Now there must be raised a rather serious objection against the diagram fig. 1, inasmuch as the Niggli values fm and c are extremely unsuitable when it comes to illustrating the evolution of a series of highly carbonatic rocks. So for instance fm includes the two oxides FeO and MgO which in these rocks combine in entirely different ways — FeO with SiO_2 , MgO mainly with CO_2 . As will be shown below it is indeed this unfortunate method of plotting that is chiefly responsible for the inauguration of

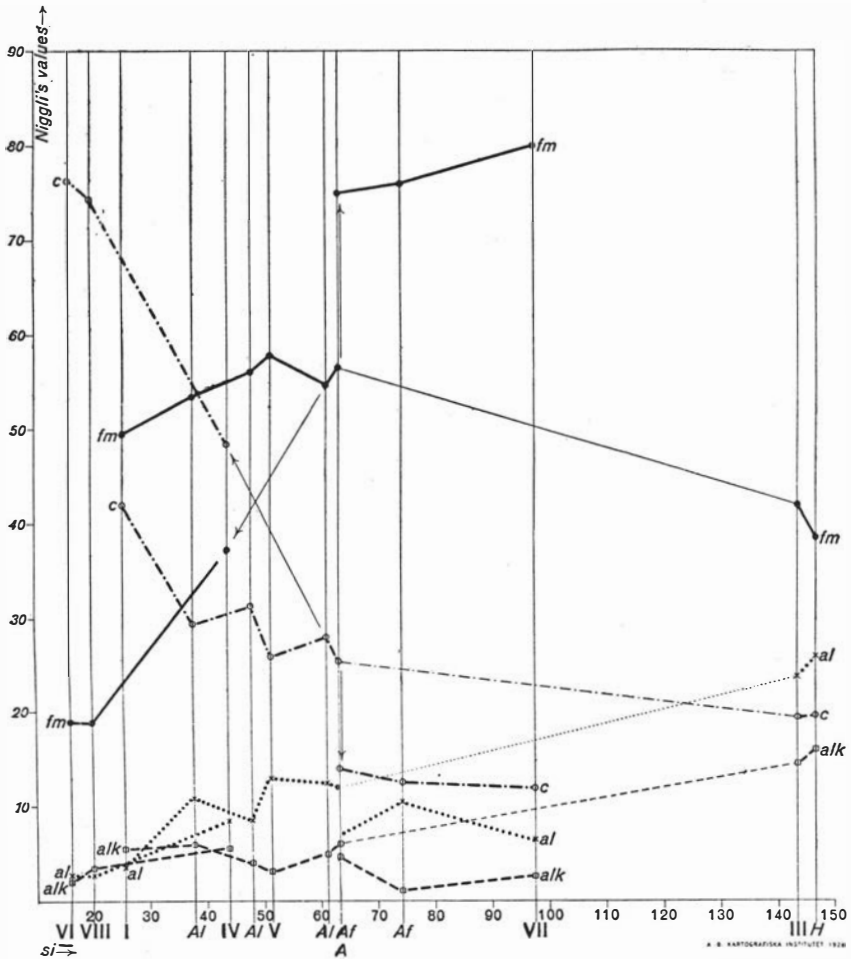


Fig. 1. »Differentiation diagram» of the alnöitic dikes according to v. ECKERMANN (1928 Pl. 55).

»vertical differentiation». In fact, the almost perfectly symmetrical shape of the *c*- and *fm*-curves in fig. 1 should at once have aroused suspicion. The obvious way of plotting is to separate MgO from FeO and, instead, unite it with CaO. In this way the FeO-curve will represent part of the siliceous portion of the rocks whereas the (Ca, Mg)O curve will mainly illustrate the carbonatic portion. It is true that MgO to some extent enters into silicates also, but except for one rock there is found in the series in the left part of the diagram no single Ca-bearing silicate mineral.

In view of these considerations a common variation diagram is in this case decidedly to be preferred to a NIGGLI diagram, and in fig. 3 such a one is given, the oxides being calculated as weight-percentages. For reasons that will presently be apparent CaO and MgO are represented by the value

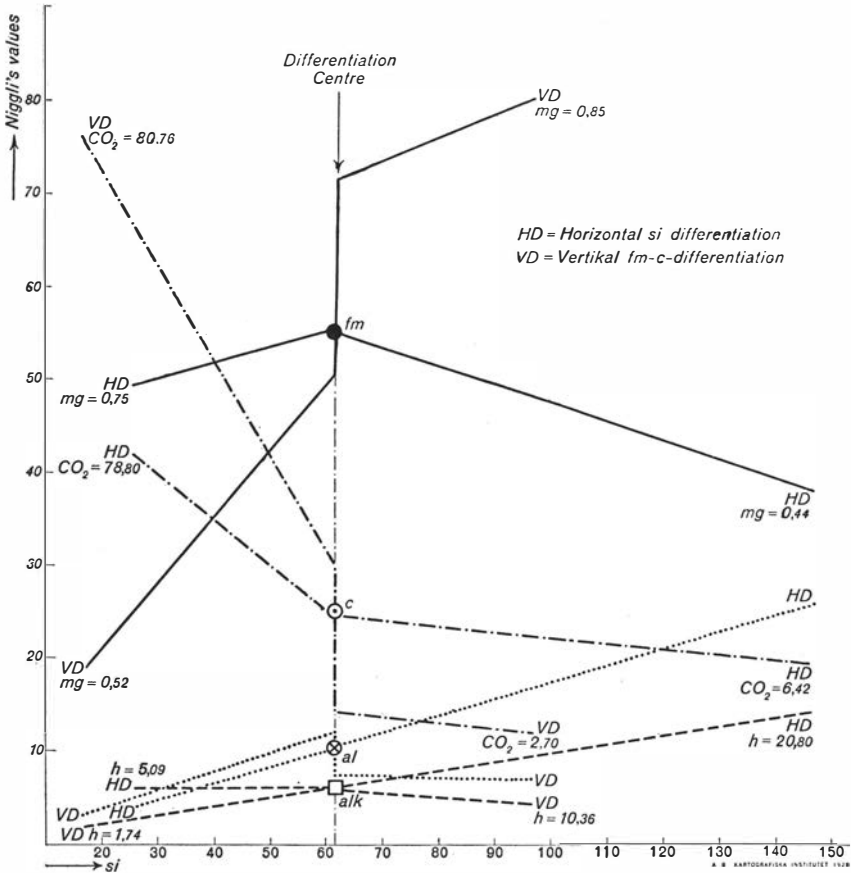


Fig. 2. v. ECKERMANN's »Schematical differentiation diagram of the perimagmatic derivatives of the alnöites». (1928 Pl. 56).

(Ca, Mg) which stands for $\frac{(\text{Ca, Ba, Sr})\text{O} + 1.4 \times \text{MgO}}{100 + 0.4 \times \text{MgO}}$. That is, all CaO + MgO

is calculated as CaO and a reduction is made so as to bring this value to correspond with the sum 100 of the analysis. For convenience the analyses are reproduced in table I in order of increasing silica, the numbers being the same as in v. ECKERMANN's paper. Attention should be drawn to the interesting fact that except for the specimens III, V, VII and H the value SiO₂ + CO₂ varies only within very narrow limits. Whereas SiO₂ ranges from 8.93—30.69 %, the sum SiO₂ + CO₂ is confined between 38.46 and 43.51. Calculated as molecular percentages the range becomes still narrower, viz. 43.23—46.52. This fact seems rather inconsistent with the idea of a central magma differentiating into four different part-magmas. On the contrary it strongly suggests that a process of replacement has been at work. This hypothesis is moreover convincingly supported by the variation diagram fig. 3, which shows the variation of

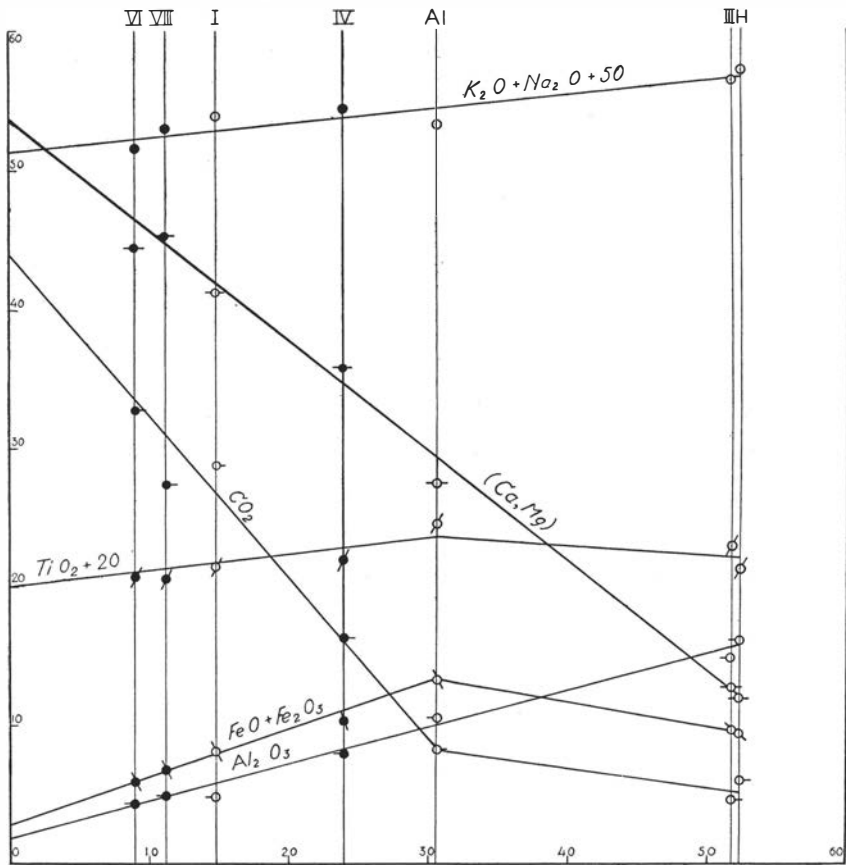


Fig. 3. Variation diagram of the alnöitic dikes (weight-percentages). The filled circles connote those rocks, which according to v. ECKERMAAN are connected by »vertical differentiation». (Ca, Mg) stands for $(\text{CaO} + \text{BaO} + \text{SrO} + 1.4 \text{MgO}) : (100 + 0.4 \text{MgO})$. See text p. 111.

the oxides to be almost perfectly linear, the alcalies, alumina, iron and titania rising, CO_2 , MgO and CaO falling with increasing silica. It seems perfectly obvious that no magmatic differentiation — be it horizontal, be it vertical or be it in the third dimension — can be held responsible for a variation along such lines. The relations between the rocks I, IV, VI, VIII and Al are in fact purely additional ones. It should be noted that those curves which rise with increasing silica fall to practically zero at $\text{SiO}_2 = 0$. That seems to establish beyond doubt that the quotients $\text{SiO}_2 : \text{Al}_2\text{O}_3$, $\text{SiO}_2 : \text{TiO}_2$, $\text{SiO}_2 : \text{FeO} + \text{Fe}_2\text{O}_3$ and $\text{SiO}_2 : \text{Na}_2\text{O} + \text{K}_2\text{O}$ tend to be approximately constant in all these rocks. Still more illuminating are, however, the values of CO_2 and (Ca, Mg) at $\text{SiO}_2 = 0$. We find $\text{CO}_2 = 44$, that is exactly the CO_2 -percentage of CaCO_3 whereas (Ca, Mg) falls at 54, that is 2% below the CaCO_3 -point. Indeed, a readjustment of the latter curve to 56 at $\text{SiO}_2 = 0$ would imply but slightly greater deviations of the projective

Table I.
Weigt-%.

	VI	VIII	I	IV	V	Al	VII	III	H
SiO ₂	8.93	11.22	14.78	24.00	26.20	30.69	51.00	51.72	52.32
TiO ₂	0.65	0.45	1.38	1.82	3.60	5.52	0.28	2.90	1.20
Al ₂ O ₃	2.30	2.90	2.94	7.97	11.34	10.48	5.97	14.70	16.08
Fe ₂ O ₃	0.93	3.32	0.74	4.99	3.97	5.76	0.47	1.40	2.53
FeO	4.68	3.59	7.18	5.53	13.89	7.47	6.82	8.31	7.02
MnO	0.40	0.20	0.21	0.22	0.22	0.55	0.11	0.07	0.07
CaO	39.24	41.05	20.60	25.22	12.36	13.25	5.44	6.51	6.58
SrO	0.74	—	—	—	—	—	—	—	—
BaO	0.11	—	3.35	0.27	—	—	—	—	—
MgO	3.59	3.54	13.98	8.40	9.33	11.10	24.45	4.69	4.07
Na ₂ O	0.25	1.18	2.15	1.37	0.67	1.41	—	3.38	2.96
K ₂ O	1.26	1.73	1.66	2.78	1.46	1.82	1.87	2.97	4.61
H ₂ O+	0.30	0.78	0.76	0.58	7.19	2.88	1.64	2.33	0.74
CO ₂	32.69	27.24	28.73	16.28	6.42	8.21	1.06	0.62	1.72
SO ₃	0.05	—	2.08	0.51	—	—	0.07	—	—
S	1.20	0.44	0.01	—	0.48	—	—	0.11	—
P ₂ O ₅	2.84	2.64	—	—	1.40	0.64	0.10	0.25	0.33
	100.16	100.16	100.55	99.94	98.29	99.78	99.28	99.96	100.23
SiO ₂ +CO ₂	41.62	38.46	43.51	40.28	32.62	38.90	52.06	52.33	54.04
(Ca, Mg)	44.51	45.37	41.22	35.78	24.51	27.57	36.14	12.84	12.08

I Biotite-carbonate dike (Beforsite) at km 257.6 (v. ECKERMANN 1928).

III Hamrongitic porphyrite at km 257.67 (*ibidem*).

IV Alnöite at km 263.3 (*ibidem*).

V Alnöite dike at km 264.4 (*ibidem*).

VI Strontian-bearing beforsite at km 264.4 (*ibidem*).

VII Stavrite at km 264.7 (*ibidem*).

VIII Calcitic beforsite at km 270.2 (*ibidem*).

Al Alnöite, Rödö. (HÖGBOM 1895).

points. The construction of the curve (Ca, Mg) instead of simply CaO + MgO was made in order to bring out this interesting relation.

It seems to me that these relations constitute a proof as perfect as can reasonably be expected in petrology, that the rocks in question were generated from a pure carbonate rock by a replacement process. The variations in the quotient CaO : MgO must be considered to depict the more or less dolomitic composition of this carbonate rock. It is by no means established that even this difference is entirely primary, but it must at any rate be considered as due to some process previous to that of the replacement just suggested. In fact there are some indications that such preexistent dif-

ferences may to some extent have brought about subsidiary variations in the process of replacement. An attempt to follow up this rather interesting problem would, however, fall beyond the scope of this paper.

The linear shape of the curves of fig. 3 allows us to calculate the approximate average composition of the material added. It comes out as:

SiO ₂	51 %
Al ₂ O ₃	16.5 %
FeO + Fe ₂ O ₃	22.0 %
Na ₂ O + K ₂ O	5.0 %
TiO ₂	5.5 %

By adding varying amounts of material of this composition to pure (Mg, Ca)CO₃ of varying Mg-content we will be able to reproduce rather closely the compositions of the rocks I, IV, V, VI and Al. The specimens V and VII, however, if plotted in the diagram, show conspicuous departures from the curves and must — if they are at all consanguineous with the rest — be considered as due to some subsidiary process that has been operating alongside with the main replacement reaction. It should be noted, however, that these very rocks were especially mentioned by v. ECKERMANN as displaying less conspicuous signs of belonging to the alnöitic clan. As for the peculiar, extremely potassic feldspar-carbonate dike (v. ECKERMANN 1928 specimen II) its high alkali value brings it to deviate markedly from the curves. Even v. ECKERMANN, however, seems inclined to assume in this case a contamination by wall rock material and does not include the analysis among those plotted in his diagram.

The above deductions, so far, pertain only to the rocks of the left part of the diagram. As for the more siliceous hamrongitic rocks, III and H, they cannot be brought to fit all of the curves. It is evident, however, that even these rocks agree fairly well with the curves for (Ca, Mg), Al₂O₃ and the alcalies, which would appear to suggest that they may actually be consanguineous with the rest of the series. In the diagram fig. 3 the oxide lines have accordingly been extended so as to include even the hamrongites. This implies a distinct break in the course of the FeO-, TiO₂- and CO₂-curves. On close consideration, however, it becomes evident that such a break is exactly the thing to be expected. So, for instance, in the initial stages the replacement reaction $\text{CaCO}_3 + \text{SiO}_2 \rightarrow \text{CaSiO}_3 + \text{CO}_2$ is practically complete, as depicted by the steep gradient of the CO₂-curve. As a result of this reaction the silicate portion of the rock augments steadily, and this must to some extent lessen the possibility of the reaction being completely effected. Silica will find ever-increasing possibilities to combine with other oxides and the obvious result will be a flattening out of the CO₂-curve. Of course this flattening is actually successive and does not appear as a sudden break as in the diagram.

The change of direction of the FeO-curve is most probably due to an analogous phenomenon. As long as CO₂ is abundantly liberated, all FeO of the invading material will be retained, as it may combine freely with MgO, alcalies, and SiO₂ to form biotite. In the same measure as the CO₂-SiO₂ replacement becomes less complete, as argued above, there will, however, be an increasing scarcity in MgO.SiO₂, and FeO will have less possibility to find a mode of combining that will enable it to remain in the rock. Likewise, the more accentuated the scarcity in CaCO₃, the less the chance for TiO₂ to settle down as titanates. In other words, in the initial stages a considerable part of the material introduced will be retained, whereas later on an increasing selection will take place, which will mainly affect FeO and TiO₂. Alcalies and alumina on the other side will always have the possibility in the presence of excess silica to combine mutually, as is also witnessed by the unbroken course of the corresponding curves.

The alnöitic dikes, then, afford a very good illustration in favour of the opinion expressed on p. 109 above, *viz.* that, when interpreting petrological diagrams, it is necessary to hold apart the effects of different factors that may have been operating independently. If this is not kept in mind we run the danger of losing our way amidst loosely inaugurated conceptions such as for instance »vertical differentiation».

The above considerations have still more strengthened my opinion previously announced (KROKSTRÖM 1937 p. 271), *viz.* that the analyses of rocks generated by the operation of a consequent process, tend to fall along smooth curves, whereas a markedly aberrant analysis suggests either that some subsidiary process has been at work, or that the rock in question is not at all consanguineous with the others. In recent years, however, there has been a tendency to regard smooth variation curves as a proof of an evolution brought about by magmatic differentiation. Such an opinion is certainly not justified, and it cannot be too strongly objected to. A replacement process or a granitization may give quite as smooth and regular curves as may magmatic differentiation. Even such an ardent defender of differentiation as BOWEN states: »We cannot determine solely from a study of chemical compositions of rock series the extent to which later differentiates — — — may have arisen from refusion» (1937 p. 19). I think that in order to be able to approach petrological problems without preconceived opinions the first step should be to avoid in the future the term »differentiation diagram» and substitute for it the older and more neutral »variation diagram». Similar views have recently been expressed by RITTMAN (1940 pp. 10—11), and I want to emphasize them still more.

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