6. On the association of granite and dolerite in igneous bodies.

By

Torsten Krokström.

The problem of the origin of granites and the frequent association of acid and basic rocks in igneous complexes has lately become one of the most puzzling questions of petrology. In the course of my investigations of the great dolerite dikes of Middle Sweden I have had several opportunities to dwell upon its various aspects (KROKSTRÖM 1932 a, 1932 b, 1936). In his very interesting papers on the Loos-Hamra region (1936) and on the Swedish Jotnian (1937) H. v. ECKERMANN has recently advanced a new conception of the Jotnian in which this problem plays a prominent part and it has consequently become of still greater interest to Swedish geologists.

From this reason it seems desirable that the question should be elucidated from as many sides as possible and the following lines are intended as a contribution towards the solution of this vital problem. As a starting point I will give a brief synopsis of my previous deductions bearing upon the subject. This seems necessary as they have been recently reviewed by v. ECKERMANN (1937, pp. 36—39) in a manner that, unfortunately, is misleading in more than one respect. Moreover, in this way also some readers mainly interested in these questions might get a proper idea of my views without taking the trouble of studying my previous papers in full.

When trying to unveil the genetical relations of the Breven granophyre, petrological and chemical data were considered alike. From a petrological point of view no definite conclusions could be drawn: »Still there remain the questions about the mechanics of the repeated intrusions and about the origin of the different magmas. *It is of course very difficult if not even impossible to propose a decisive solution of the last question, yet it seems most probable* that the successive intrusions emanated from the same magma reservoir and were brought to action at different stages of the differentiation of the magma.» (1932 a, pp. 311—312, italics inserted here).

In this connection attention was also drawn to the hypothesis of refusion of country rock, then recently advanced by HOLMES (1931), and it was considered impossible to find »any decisive proofs for or against this very fascinating hypothesis». (*ibidem*, p. 312).

As regards the chemical evidence I was able to show that the granophyre analysis could be nicely fitted into the variation diagram of the main dolerites. It was stressed, however, that this does not prove the consanguinity of the granophyre and the dolerites (*ibidem*, p. 318): »It is true that there is a rather wide interval between the analyses 6 and 7 and thus a slight change of the projective points of no. 7 would imply but a small disturbance of the curves. Thus perhaps rather a great number of granites would fit into the diagram as well as the Breven granophyre, but this hardly reduces the validity of the conclusion arrived at above.» (*viz.* that the marginal olivine dolerite and the epidolerites do not belong to the same line of differentiation as the main dolerites). It was also pointed out that in the c-alk-fem triangle »the granophyre falls at a considerable distance from the rest» (of the rocks) »and is separated from them by a rather wide field of discontinuity containing no transitional rocks».

It is thus readily realized that the investigation of the Breven rocks had brought out no definite proofs of the consanguinity of the granophyre and the dolerites. It is true that, in summing up, I gave it as my opinion that the different types of rock all belonged to the same line of differentiation. Such a conclusion seemed to be justified as the intimate geological association made it probable and as there had been found no facts definitely opposing it. The previous discussion, however, brought out clearly that, as far as the granophyre was concerned, I was fully aware of the rather weak foundations upon which this conclusion was based. As a matter of fact the other deductions made in the paper were in no way affected by it.

A few months later I had the opportunity of discussing a little more in detail the problems connected with the differentiation of basaltic magmas (1932 b). From theoretical as well as empirical reasons I reached the same conclusion as had already been convincingly defended by FENNER (1926, 1929, 1931), *viz.* that the evolution of basaltic magmas of the type under discussion does not tend in a granitic direction. In particular it was very difficult to conceive how such large granitic masses as the Breven granophyre — quantitatively almost equalling the corresponding basaltic rocks — could have been generated in this way. The problem becomes more puzzling still if we consider that in the dike there are no primary rocks whatever of a composition intermediate between granite and dolerite.

Consequently I was forced to the conclusion that the Breven granophyre could not have been generated from the basaltic magma by means of crystallization differentiation. As a matter of fact I pointed out that the relations within the dike were far more readily explained by the hypothesis of liquid differentiation. In view of the strong laboratory evidence that had been put forth against this hypothesis I refrained, however, from accepting it and consequently was at a loss how to explain the origin of the granophyre. My reluctance against an interpretation, involving differentiation as the main operating factor, was still more strengthened by KENNEDY's study of the late segregation veins in basaltic rocks (KENNEDY 1933). From an extensive analytical material he concluded that magmas of the olivine-basalt magma type do not differentiate in an acid but in an alkaline direction. Among his rocks not a single case was found in which olivine-basalt magma had produced a quartzose differentiate.

In this connection the pegmatite pipes in the olivine dolerite of the isle Säppi are of some interest. ESKOLA (1932, 1936) argues that the pegmatites are the result of crystallization differentiation within the doleritic magma and this is by no means inconsistent with KENNEDY's results. In Table I below the analyses of two Säppi pegmatites are reproduced from ESKOLA's paper (1936) and their norms have been calculated. It is realized that they both display markedly trachytic affinities and according to the C. I. P. W. classification they fall in the same subrang as a fairly large number of trachytes. If these pegmatites represent the residual liquids of the dolerite magma, as argued by ESKOLA, it is rather difficult to conceive how differentiation might proceed further so as to give birth to large quantities of extremely acid rocks.

Table I.

	I	II		III	IV
SiO_2	54.53	59.23	Qu	2.40	4.08
TiO_2	2.38	1.64	Or	25.58	27.24
Al_2O_3	11.06	14.34	Ab	34.06	41.39
Fe_2O_3	4.89	2.20	An	—	4.17
FeO	10.64	7.73	Ac	1.85	—
MgO	1.36	1.53	Di	18.21	4.99
CaO	4.16	1.95	Hy	6.91	11.78
Na_2O	4.19	4.82	Mt	6.26	3.25
K_2O	4.23	4.56	Ilm	4.56	3.04

I. Dolerite pegmatite, Säppi. ESKOLA 1936, p. 2, analysis 4.
II. » » , » . *Ibidem*, analysis 7.
III. Norm of I.
IV. Norm of II.

In order to permit a comparison with the analyses of Table III MnO, P_2O_5 and H_2O were omitted.

Another interesting example is afforded by the pegmatite veins traversing the great eucrite ridge of the Ardnamurchan peninsula, N. W. Scotland (RICHEY and THOMAS 1930, p. 196 ff.). They are considered by RICHEY as »residual liquid portions of the intrusion in which fluxes had become concentrated» and are often composite, consisting of »augite-rich marginal portions and a felspathic centre». I have had the opportunity of examining these pegmatites in the field and they certainly do not suggest an evolution in a granitic direction.

Differentiation thus being made highly improbable, not to say being disproved, the next possibility to be considered is that of assimilation. I have pointed out (1932 a, p. 312) that the changing composition of the basaltic magma might to some extent have been governed by processes of assimilation and the same possibility was stressed by ESKOLA (1936) for the Säppi rocks. As regards the Breven rocks, however, a mere glance at the analytical data shows that such processes could in no conceivable way have been solely responsible for the generation of the granophyre. To such a purpose assimilation would have had to be effected in an entirely unimaginable extent. This conclusion seemed so obvious that I did not even care to stress it and v. ECKERMANN (1937) consequently goes to uncalled-for trouble in trying to disprove such a hypothesis.

Nevertheless his calculations in this connection are of some interest. He has found (*loc. cit.* p. 37) that »in order to obtain a Breven granophyre magma one part of the Breven dolerite has to melt 49 parts of the following remarkable magma . . . (see column I of Table II below) . . ., which means a norm containing nephelite and a mineral composition of something like quartz, albite, orthoclase and alkaline diopsidic pyroxene.»

It certainly seems rather puzzling that the subtraction of about 2 % of dolerite material from the acid granophyre should produce such a very marked change in its composition. Unfortunately, however, v. ECKERMANN's statements are slightly erroneous as may be seen from columns A and B of the table below. Of course the analysis of the »remarkable magma» contains no normative nephelite — indeed, magmas of a silica content of 73.5 % very seldom do! — and it is rather difficult to conceive why alkaline diopsidic pyroxenes should have to enter into the mineral composition. As a matter of fact the »remarkable magma» is fairly well matched by several granitic rocks. Some of them are entered in Table II and their number could easily be multiplied. It is of interest to find that two of these analyses, *viz.* no. III and no. IV, were considered by VOGT (1908, p. 67) to approach the composition of the »granitic eutectic», and it might be inferred that the »remarkable magma» would represent a fairly low-melting mixture.

As previously mentioned, however, this means a digression from the subject, as I have never made even the slightest attempt to explain the

Table II.

	I	II	III	IV	V	VI	VII	VIII	A	B	
SiO_2	73.5	73.03	74.92	73.89	73.69	73.48	73.52	73.94	1225	Qu	29.0
TiO_2	0.05	0.10	—	—	—	—	—	—	—	Or	25.7
Al_2O_3	13.1	13.09	13.04	14.44	12.25	12.22	13.34	12.59	128	Ab	38.4
Fe_2O_3	2.1	2.13	2.03	0.43	2.90	2.79	1.68	2.11	13	An	2.8
FeO	1.3	1.43	—	1.49	1.55	1.10	1.03	1.67	18		95.9
MgO	0.0	0.17	0.25	tr.	0.04	0.20	0.15	0.14	—	Di	1.2
CaO	1.0	1.12	0.92	1.08	0.31	0.75	1.05	0.89	18	Woll	0.4
Na_2O	4.3	4.32	4.28	4.22	4.64	4.64	4.28	3.94	69	Mt	3.0
K_2O	4.65	4.61	4.56	4.45	4.62	4.82	4.95	4.72	49		4.6

I v. ECKERMANN's »remarkable magma» (loc. cit. p. 37).
II Granophyre from the Breven dike (1932 a, p. 305).
III Spherulite from spherulitic obsidianite, Teneriffa. (Lagorio 1887, p. 440).
IV Mesostasis between basic »concretions» in granite from Peterhead, Scotland. (Philips 1880, p. 13).
V Granite, Quincy, Massachusetts. (Washington 1917, no. 27, p. 116—117).
VI Granite, Blue Hills, Massachusetts. (*Ibidem*, no. 28, p. 116—117).
VII Quartz pantellerite, Texas. (*Ibidem*, no. 62, p. 120—121).
VIII Granophyre, Loch Ba, Mull, Scotland. (*Ibidem*, no. 114, p. 126—127).
A Molecular proportions of »remarkable magma».
B Norm of »remarkable magma».

All analyses are recalculated as water-free on a sum of 100.

Breven granophyre as a result of assimilation. — I had better return to the main theme.

It is a pity that v. ECKERMANN did not try to apply his method of calculation to the hypothesis of differentiation, which he is so fervently defending. The result would certainly have afforded food for thought. In Table III below I have calculated the normative composition of the residua that would result if 100 parts of dolerite magma were assumed to furnish 5.76, 7.41 and 18.04 parts of granophyre magma. It is found that the amount of normative nephelite is of the same order of magnitude as the amount of granophyre that is supposed to have been generated. I think anybody will admit the inconsistency of a hypothesis according to which the generation of a granophyre magma would involve the simultaneous formation of such large amounts of nephelite. Now it is true that nephelite may to some extent enter into the normative composition of basaltic rocks without being represented among the actual minerals and I have previously discussed this phenomenon at some length (1936, p. 136 ff.). In these cases, however, the Ne-values are always rather low and can by no means be compared with the high amounts of the norms tabulated below.

Table III.

Normative composition of the residua left after subtracting granophyre magma from the Breven dolerite magma (1932 a, analysis I).

Granophyre	5.76 %	7.41 %	18.04 %
Ne	6.48	6.35	11.53
Or	4.52	3.94	1.10
Ab	14.62	13.83	3.42
An	31.04	32.19	35.40
Di	13.61	15.57	17.25
Ol	22.78	21.16	23.88
Mt + Ilm	6.95	6.96	7.42
	100.00	100.00	100.00

Anybody claiming that the Breven granophyre should have been generated from the olivine-dolerite magma by crystallization differentiation, must be prepared to give a solution to this problem. He must explain how the extreme undersaturation in silica of the material removed by crystallization can be effected by means of a normal mineral assemblage. Until such an explanation is provided I feel justified in rejecting for this case the differentiation hypothesis.

Table IV.

Normative composition of the residua left after subtracting pegmatite material from the Säppi olivine dolerite.

Pegmatite	23 %		16.5 %	
Ne	2.84		0.58	
Or	—		1.69	
Ab	19.37		23.90	
An	35.83	58.04	34.93	61.10
Di	13.59		10.07	
Hy	20.71		21.30	
Mt + Ilm	3.95	41.96	4.00	38.90
		100.00		100.00

It might be of some interest to make a similar calculation for the Säppi pegmatites and dolerites, since the chemical character of the pegmatite material does not *à priori* oppose the conception of differentiation. In Table IV there are given the results of such a calculation starting from the olivine dolerite (ESKOLA 1936, analysis 1) and the most acid of the two pegmatites (analysis 7). It is realized that there is nothing very extraordinary in the norms obtained. The maximum amount of pegmatite that may be generated in this way is 23 % and even then the material subtracted shows only 2.84 % normative nephelite, that is an amount that might very probably be found in many dolerite analyses. The Breven olivine dolerite, for instance, shows 1.99 % nephelite in the norm.

As far as I have been able to discover, v. ECKERMANN provides only one positive argument in favour of differentiation, *viz.* that the granophyre analysis »fits in beautifully with the differentiation curves» (*loc. cit.* p. 38). I will dwell a little upon this method of reasoning.

In fig. 1 the differentiation diagram of the Breven-Hällefors rocks has been redrawn on the same scale as in v. ECKERMANN's paper. According to his suggestion the curves have been extended so as to include the granophyre. Further there have been plotted the NIGGLI values of four other rocks, *viz.*:

1. Åmål granite, Åmål, Dalsland. (HOLMQUIST 1906, p. 264).
2. Granite, Virbo, parish of Misterhult, Småland. (SVEDMARK, 1904, p. 24).
3. Biotite granite, Tennberg, Dalarna. (v. ECKERMANN 1923, p. 470).
4. Granite, parish of Tanum, Bohuslän. (SVEDMARK, 1902, p. 23).

These analyses all »fit in beautifully with the differentiation curves» but I do not think even the extremest adherent of the differentiation hypothesis would claim from that reason that they are consanguineous mutually and with the Breven-Hällefors dolerites. Of course I am fully aware that I am entirely neglecting the geological relations but my intention is only to show that such a coincidence with a series of differentiation curves proves nothing.

I think it is high time that we should try to break the spell by which the differentiation curves have for rather a long time held many petrologists bound. As is evident from the diagram Fig. 1 no reliable proofs of the consanguinity of a couple of rocks may be furnished by such curves. This statement of course implies no criticism of the NIGGLI values and the NIGGLI curves as such — the objection pertains only to their uncritical application. Even the most useful instrument when not properly handled may do more harm than good. 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. If this discrimination could be more generally kept in mind many dubious conclusions might certainly be avoided.

Before leaving the question of differentiation a few words should be said about the quantitative relations, although it must necessarily be only a reiteration of arguments which have repeatedly been put forth by several

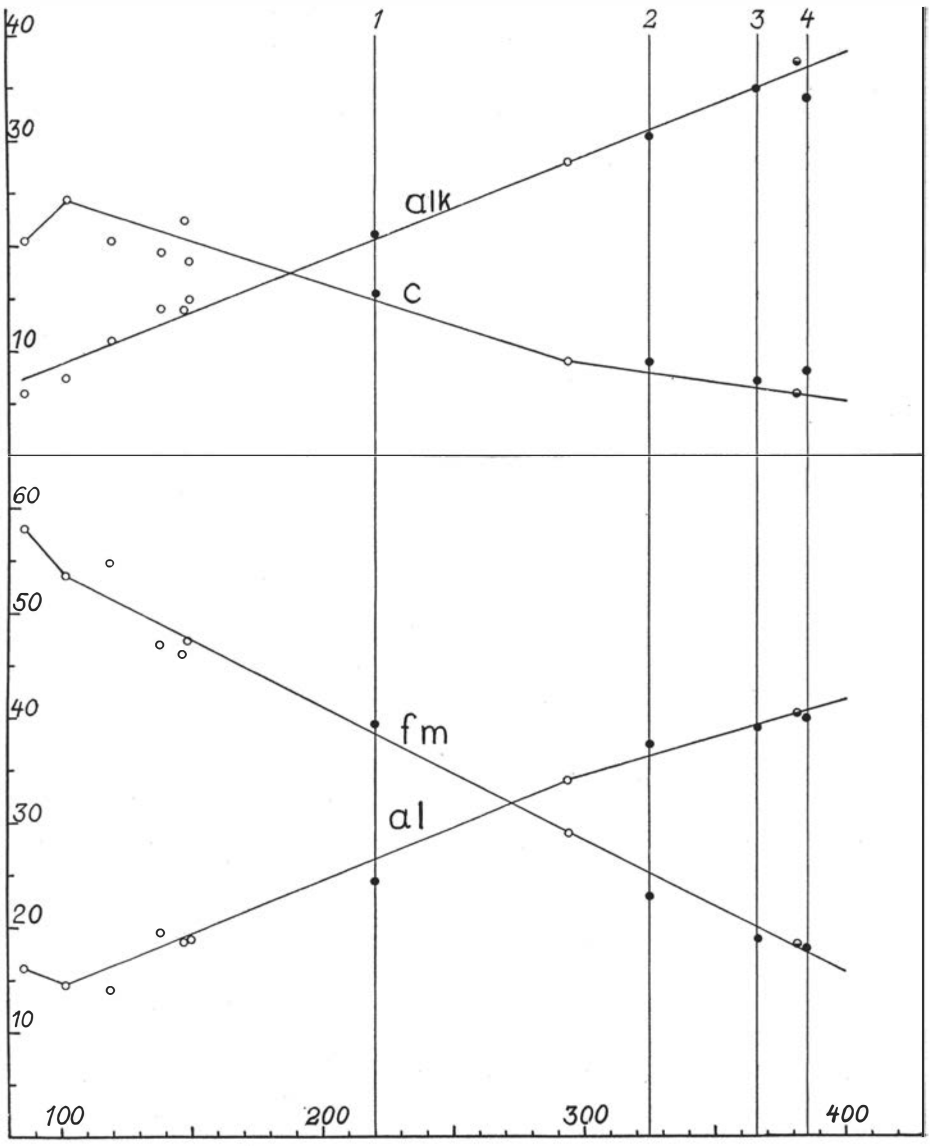

Fig. 1. Open circles represent rocks of the Breven-Hällefors differentiation suite, half-filled circles the Breven granophyre. Concerning the filled circles and for further explanation see text.

authors — myself among them. As regards the Breven dolerite a differentiation in a granitic direction seems to be out of question on behalf of the abnormal composition of the material to be subtracted. Starting from a less undersaturated magma, there is a theoretical possibility of obtaining a granitic differentiate. GROUT (1926, p. 549) has pointed out, however, that »as a maximum one-tenth of an average basaltic magma may become granite.» The obvious consequences were stressed by HOLMES (1936, p. 231) who concluded that it was highly improbable, not to say impossible, that such a very small granitic residue should be able to collect »to form a discrete body of granitic magma.» Similar views were long ago expressed by HOLLAND (1897). These considerations still more strengthen my above conclusions.

Differentiation and assimilation being both disproved there seems to be only one possibility left, *viz.* that the granophyre represents a product of partial refusion. Against this proposal v. ECKERMANN argues that the bulk-composition of the country-rocks is not that of the granophyre. This objection would be valid if a complete refusion of the country rocks were assumed, and I admit that in using the word »remelting» (1936, p. 248) I may have made my opinion liable to misunderstanding. As I was explicitly stating, however, that my interpretation was in agreement with the hypothesis advanced by HOLMES (1931), nobody acquainted with his paper should have been misled by this linguistic error. The correct term seems to be »refusion» and my interpretation by no means involves the assumption of a complete remelting of country rocks. In raising the objection just mentioned, v. ECKERMANN consequently once more misses the point.

Before entering upon a discussion of the refusion hypothesis I should like to make a few things clear. It is no concern of mine to offer opinions on v. ECKERMANN's new conception of the origin of the Rapakiwi granites. This would be of little value as my field experience from the Dalecarlian and Nordingrå regions is far too limited. As far as the Breven and Hällefors dikes are concerned, however, I think I have shown that they can impossibly be considered as supporting this hypothesis. On the contrary, the evidence gained from them seems to raise serious objections against it. v. ECKERMANN states that »the Breven and Hällefors dikes, properly interpreted, furnish additional support to the Jotnian conception advanced by the author». His reasons for such a statement are the following. He has disproved — more or less correctly — two interpretations that have never been proposed and he has shown that the granophyre analysis fits in with the differentiation curves of the dolerites. I am, indeed, very sorry I did not know earlier that a proper interpretation could be arrived at in such a very simple way.

I will now consider the possibility of refusion of country rock and in this connection some recent investigations at the Geophysical Laboratory of Washington are of great interest. It was shown (DAY 1931, pp. 78—79) that »dry» rocks of granitic composition melt completely at lower temperatures than »dry» basalts and while this difference of temperature is considerable »there is a much greater difference between the lowest temperatures at which melting becomes appreciable in these rocks. At how low a temperature it would have been possible to realize melting in granite by indefinitely increasing the time can not be said, for at such temperatures the reactions are exceedingly sluggish. Melting was plainly demonstrable in a particular type specimen, however, at 700° and some melting probably took place at 570°. In one week at 800° powdered granite became half liquid . . . In contrast to this it required a temperature approximately 300° higher to develop a corresponding degree of melting in the basalts.»

These investigations were carried out on material that had been previously heated so as to become essentially free from volatile constituents. It might thus be inferred that the lower crystallization temperatures of granites as compared to basalts are not dependent on the volatile contents of the granitic magmas but are a feature inherent even to the dry melts. I continue my quotation from DAY (*loc. cit.* p. 79): »It can no longer be argued that, although the granites did crystallize at lower temperatures than the basaltic rocks, they did so because of the volatiles then in the magma, and that they cannot melt with their present low content of volatiles until a temperature is reached above that necessary to melt the basalts . . . So in any supposed large-scale remelting of the surface layers of the earth, where the rise of temperature is necessarily slow . . . it must be considered that granites will melt and flow at much lower temperatures than basalts.»

It is quite evident then that in order to account for a partial refusion of acid country rocks there is no need to assume that the basaltic magma should have been superheated to any very great extent. If we accept HOLMES's views of a rising cupola of basaltic magma, which seem, indeed, to be strongly supported by the evidence of the tertiary central complexes of N. W. Scotland, it is quite clear that the rising thermal surfaces must be able to effect refusion of acid rocks to a fairly large extent. To such a purpose it is probably not even necessary to assume any very strong operation of convective currents within the basaltic magma.

The composition of the melt thus generated should be practically independent of the composition of the rocks affected — provided they are mainly quartzo-felspathic — as it must necessarily approach that of the lowest-melting mixture, *i. e.* the »granitic eutectic». It is very interesting then to find that the Breven granophyre is almost identical with this eutectic as deduced by VOGT (1908, p. 67; compare columns II, III, and IV of Table II of the present paper).

It would seem then, that whereas other theories fall short the hypothesis of refusion is able to furnish an interpretation against which no reasonable objections may be put forth. Consequently I venture to maintain my previous opinion that the Breven granophyre is a product of refusion. According to BACKLUND's nomenclature (1937, p. 234, foot-note) it should be described as a rheomorphic rock.

Of course such a generation of granitic magma by refusion must involve some contamination of the basaltic magma, and as pointed out on p. 268 above I am fully aware that such processes may have been operating (cf. also ESKOLA 1936, p. 3). There is no need, however, to assume this contamination to have taken place on such a large scale that true hybrid rocks should have been formed. We must keep in mind that the objections against liquid immiscibility pertain only to the process of unmixing in silicate melts but by no means oppose the view that two different magmas may coexist in the same chamber without mixing to any very great extent.

It is far more likely that only fairly small amounts of acid material were incorporated, and they might have produced a slight change in the initial composition of the dolerite magma. It is even possible that this contamination is responsible for the seeming consanguinity of dolerite and granophyre that is for instance suggested by the NIGGLI diagrams.

There remains the question of the geological age of the Breven and Hällefors dikes. When, in 1929, I entered upon an investigation of these intrusives, the prevailing opinion was that they were post-Jotnian (GEIJER 1922, ASKLUND 1923). The results of my work did not at first afford any possibility of proving or disproving this conception, but a few arguments were put forth that might throw some doubt upon its validity. One of these arguments was the association of dolerite and granite in the Breven dike, a feature that is generally considered as a characteristic of sub-Jotnian intrusions. Another argument was connected with ASKLUND's investigations of the faulting lines of S. E. Sweden, which seemed to have brought out that the N. N. W. fissures had been formed in Jotnian time. Now faulting along such N. N. W. planes has indubitably affected the Breven dike and was indicated in the Hällefors dike too. In view of these petrological and tectonic indications I ventured to point out the possibility of a sub-Jotnian age of the dikes, a final statement, however, being deferred until further field and laboratory evidence might be gained.

In the course of my later researches I then became convinced that the association of dolerite and granite was due not to a differentiation characteristic of a certain type of magma but to a rheomorphism of country rocks. As one cannot reasonably expect such processes to be limited to one geological epoch only, one of the before-mentioned arguments could thus no longer be valid. As to the second argument — the tectonic one

— I had from the very first beginning pointed out that it was open to serious objections and consequently I was forced to admit that my results did not justify an attack upon the time-honoured conception of a post-Jotnian age of the dikes in question. Nor is it possible to-day to take up a position for or against v. ECKERMANN's proposal of a Jotnian age. I have pointed out that the old conception is also based on rather weak foundations (1936, p. 256), but it is my opinion that before discrediting an earlier hypothesis we should be able to provide an interpretation that is decidedly better founded. v. ECKERMANN's statement that »in his last paper, however, KROKSTRÖM takes great pains to invalidate his former conclusions» (1937, p. 37) only shows that he does not feel this reluctance against attacking a hypothesis without being able either to disprove it or to advance an interpretation more consistent with the facts established.

It seems there is so far no possibility of proving irrefutably the exact age of the dikes in question. Moreover it should be kept in mind that in the Breven dike the different intrusions were evidently separated by fairly long periods of denudation. We cannot reliably ascertain the length of these periods as compared to the sub-Jotnian, Jotnian and post-Jotnian epochs, and it is thus by no means impossible that the forces responsible for the formation of the dikes may have been active during more than one of them.

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Abbreviations.

B. G. I. U. = Bulletin of the Geological Institution of the University of Upsala.
G. F. F. = Geologiska Föreningens i Stockholm Förhandlingar.
S. G. U. = Sveriges geologiska undersökning. (Geological Survey of Sweden).

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