

Comment on

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"Mineralogical and geochemical variations across three basaltic lava flows from the Faeroe Islands" by Aage Jensen.

In a recent paper, Jensen (1980) has presented a wealth of detailed chemical and mineralogical data on three Faeroese basalt flows which are a valuable addition to the existing information on the Faeroese basalts. He finds that chemical variation is most pronounced towards the contacts (both upper and lower) and attempts to explain this in terms of post-eruptional processes. In fact, the variation is quite small and the obvious post-eruptional alteration processes which have taken place (high $\text{Fe}_2\text{O}_3/\text{FeO}$ ratio, high water content and presence of from 2–14% green and brown alteration products) render the distinction between pre- and post-eruptional effects difficult. It may be questionable whether such a detailed study of single lavas might not be more profitably carried out on recent material where much of the post-eruptional effects can be shown not to have taken place. In this connection, Brooks (1976) noted an increase in the average degree of oxidation of iron from the post-glacial basalts of Iceland to the early Tertiary basalts of the North Atlantic region caused undoubtedly by the effects of percolating ground water and conditions of deep burial.

However, I wish to question the sentence (page 112), "Unfortunately the behaviour of the Fe-Ti oxides is frequently ignored in petrological work, and the way in which some petrologists have chosen to get around this problem, namely the freezing of the $\text{Fe}_2\text{O}_3/\text{FeO}$ ratio at some arbitrary value when comparing different rocks, is not in the opinion of the author an acceptable solution". Apart from the doubtful truth of the introductory generalization, the main body of the sentence displays a lack of appreciation of the reasons for using a fixed $\text{Fe}_2\text{O}_3/\text{FeO}$ ratio in norm calculations.

Brooks (1976) suggested using a fixed value of $\text{Fe}_2\text{O}_3/\text{FeO}$ of 0.15 in order to standardize the many systems then (and now) in use. Far from

being an arbitrary value, the basis of his argument was that the freshest volcanic rocks available for study are the submarine glasses reported on by several workers (references in Brooks, 1976) whose pristine nature with respect to several volatile constituents has been well documented by Moore (1970), Moore and Schilling (1973) and others. The arguments set out by Brooks (1976) have recently been largely accepted by Hughes & Hussey (1979).

The reason for applying a correction to the oxidation ratio of basalts is that petrologists wish to compare the normative compositions of *magma*s not *rock*s for petrogenetic purposes and they wish, as far as possible to remove the effects of surficial alteration of which the oxidation of Fe is one of the more serious. Hydration is another, but this generally does not figure in the norm and analyses are frequently calculated to 100% volatile-free. Jensen does not appear to take issue with the inability of the CIPW norm to cope with the green and brown mineraloids observed by him which also differ from the calculated normative constituents.

Jensen recommends the calculation of appropriate amounts of hematite and rutile to achieve a better correspondence between the norm and the mode. He claims that "generally it would also prevent quartz tholeiites being turned into olivine tholeiites." Most igneous petrologists would probably feel that such indices of alteration and oxidation as hematite and rutile in basalts are mostly best left uncalculated and would feel that it is not advisable to classify rocks as quartz tholeiites which have been turned into such by secondary oxidation processes. In this connection, it is clear from the work of Rasmussen & Noe-Nygaard (1969) that the Upper Basalts of the Faeroe Islands have a greater tendency to be olivine-phyric than lavas of the earlier series, particularly the Lower Basalts so it is perhaps not surprising that on applying a correction to the $\text{Fe}_2\text{O}_3/\text{FeO}$ ratio, these samples become olivine tholeiites (Jensen, Table 2).

I conclude that this paper, while being an outstanding piece of descriptive petrology, tends to present a misleading view of the aims of other petrologists more concerned with genetic problems.

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When writing that some petrologists have chosen to get around the problem caused by oxidation of Fe-Ti-oxides during the initial cooling of basalts by using an arbitrary value of the $\text{Fe}_2\text{O}_3/\text{FeO}$ ratio instead of the one given by the analysis, it was not my intention to disparage the aims of petrologists concerned with genetic problems – rather to make a suggestion I hope will be helpful.

So far I have in my work (Jensen 1978, 1980) given norm calculations of analyses both with the $\text{Fe}_2\text{O}_3/\text{FeO}$ ratio obtained from analyses and with the fixed value of 0.15 proposed by Brooks (1976).

However, I have shown that the use of the ratio 0.15 has some undesired effects, so I sug-

gested that instead of changing the analysis away from the actual rock, one could change the norm towards the Fe-Ti-oxides actually present in the rock.

The main difference in opinion between Brooks and me is how to handle rocks with high ratios of $\text{Fe}_2\text{O}_3/\text{FeO}$ when making comparisons, and in this respect I want strongly to advocate my point of view, but first I can say that when Brooks points out: 'Jensen does not appear to take issue with the inability of the CIPW norm to cope with the green and brown mineraloids observed by him which also differ from the calculated normative constituents', I think he is right: I have neither tried to make a so-called 'calculated mode' nor discovered how to get a CIPW norm to cope with the presence of glass, fresh or altered.

As an argument for not regarding the effects of using a fixed ratio of 0.15 as undesirable, Brooks says that it is perhaps not surprising that, using the ratio 0.15, samples from the upper basalt series become olivine tholeiites, as Rasmussen & Noe-Nygaard (1969) have shown that Upper Basalts have a greater tendency to be olivine-phyric than lavas of the earlier series, particularly the Lower Basalts. Perhaps not, but then it is all the more surprising that the use of the value 0.15 also turns samples from the lower series into olivine tholeiites (Jensen 1980, pp 104–105).

It is true that fig. 1 in Brooks (1976) shows an increase in the $\text{Fe}_2\text{O}_3/\text{FeO}$ ratio from post-glacial basalts of Iceland to the early Tertiary basalts of the North Atlantic region. No explanation for this is offered in the paper in question, but now in his comments to my paper Brooks concludes that this trend is caused undoubtedly by the effects of percolating ground water and conditions of deep burial. These may be contributory factors, but it is my firm belief that differences in oxygen fugacities during the initial cooling have an important influence on the $\text{Fe}_2\text{O}_3/\text{FeO}$ ratios given by Brooks (1976).

Brooks claims that his proposed value of 0.15 is far from being arbitrary. In his original paper (Brooks 1976) he considered his value 'not completely arbitrary' and states: 'The actual value chosen is of less importance than that a unique procedure be adopted by all'. Brooks now says that his arguments have been largely accepted by

Hughes & Hussey (1979). This statement conceals the fact that Hughes & Hussey (1976) proposed a value of 0.25 while Brooks (1976) proposed 0.15, and now Hughes & Hussey (1979) propose a compromise by using a value of 0.20. I still feel that there is something arbitrary about the values of the $\text{Fe}_2\text{O}_3/\text{FeO}$ ratio used by some petrologists.

The main reason for using values of $\text{Fe}_2\text{O}_3/\text{FeO}$ different from those obtained by analysis is that the use of magnetite and ilmenite in norm calculations makes cations which actually belong to silicates, go into oxides, thus creating a surplus of SiO_2 .

The Fe-Ti-oxides in basalts that have reached oxidation stage 5 (Haggerty 1976) no longer contain Fe^{++} , accordingly the use of magnetite and ilmenite as normative minerals is misleading. But if instead hematite and rutile are used, all Fe^{++} will be reserved for silicates. This seems a better practise than tinkering with the analysis. Petrologists who cannot accept the use of hematite and rutile instead of magnetite and ilmenite have the alternative of checking that the Fe-Ti-oxides have not passed oxidation stage 3 before they analyse a sample.

About my proposal of using hematite and rutile as normative minerals Brooks says: 'Most igneous petrologists would probably feel that such indices of alteration and oxidation as hematite and rutile in basalts are mostly best left uncalculated and would feel that it is not advisable to classify rocks as quartz tholeiites which have been turned into such by secondary oxidation processes'. It is not olivine tholeiites that have been turned into quartz tholeiites by secondary oxidation, but quartz tholeiites that are turned into olivine tholeiites as the result of use of a fixed non-analytical value of $\text{Fe}_2\text{O}_3/\text{FeO}$. I proposed that at oxidation stage 5 and over all analytical Fe_2O_3 and TiO_2 be transferred to normative hematite and rutile. At stage 5 pseudobrookite ($\text{Fe}_2\text{O}_3 + \text{TiO}_2$) has not appeared but it does so at stages 6 and 7. Pseudobrookite is not stable below 600°C and the formation of pseudobrookite in basalts takes place between 600 and 800°C (Haggerty & Lindsley 1970). It seems misleading to call oxidation at such temperatures secondary. The Fe-Ti-oxide development during the initial cooling of a lava is a fully legitimate part of the process of rock-formation and is

related to the character of the magma. The denial of this or the failure to determine the nature of the Fe-Ti-oxides present is the basis for my statement that the development of the Fe-Ti-oxides is frequently ignored in petrological work.

It was defeatism caused by the different 'correction factors' used by petrologists on the results of chemical analyses that discouraged me from a more formal proposal that hematite and rutile be used as normative minerals instead of magnetite and ilmenite when observations in reflected light have shown that magnetite and ilmenite are no longer present amongst the opaque minerals, which instead are hematite, rutile, and pseudobrookite. The idea of adapting the norm instead of the analysis when it is the norm that does not correspond to nature seems so logical that perhaps changing the norm has as big a chance of becoming standard practise as the use of a fixed value of $\text{Fe}_2\text{O}_3/\text{FeO}$ that is actually agreed on by all petrologists.

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