LETTER TO THE EDITORS

ARCHAEO EVOLUTION OF THE LEWISIAN COMPLEX OF GRUINARD BAY, ROSS-SHIRE

Sirs—Davies has presented a rather confusing discussion of the relationships between the metadolerite dykes and of the shear zones in the Gruinard Bay area.

He refers to two sets of shear zones (NW-SE and E-W) and one set of metadolerite dykes, the dykes trending 090°-100° and 135°, the 135° trend predominating.

On page 193, line 8 he states that the 135° trending dykes are “affected” by the NW-SE trending shear zones and that the same 135° trending dykes lie within the predyke, dextral shear zones. When no other explanation is offered, it is hard to see how a predyke shear zone can affect a 135° trending dyke, unless the shear zone controls the dyke orientation. However, he does not say what sort of “effect” the shear zones have on the dykes.

The E-W shear zones are said to be sinistral and that they “appear to have originated prior to dyke emplacement” and that they “offset dykes of the 135° set” (page 193, line 20). Given no other explanation the E-W shear zones cannot be predyke and post-dyke at the same time.

To explain these facts without contradiction, more facts are necessary; however, because he does not seem to find consistent relationships between dykes and shear zones, three hypotheses seem feasible: (1) the dykes are of more than one age, (2) shear zones on similar trends did not all form during the same episode, (3) the shear zones and the dykes were all roughly synchronous and the same shear zone may have moved both before and after dyke injection (or during).

The first hypothesis, involving multiple episodes of dyke injection, necessitates this sequence of events:

(1) Injection of 135° trending dykes
(2) Formation of NW-SE trending, dextral shear zones
(3) Formation of E-W trending, sinistral shear zones
(4) Injection of (a) 090° trending dykes
   (b) 135° trending dykes

The second hypothesis, that of shear zones forming at more than one time, necessitates the following sequence of events:

(1) Formation of NW-SE trending, dextral shear zones
(2) Formation of E-W trending, sinistral shear zones
(3) Injection of (a) 090° trending dykes
   (b) 135° trending dykes
(4) Formation of 2nd set of NW-SE trending shear zones

The third hypothesis, in which dykes were injected synchronously with the formation of the shear zones, does not alter either possible sequence of events, it simply implies a much shorter time gap between the various events.

Without the evidence on which the statements are based it is not possible to conclude

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which hypothesis (relating the shear zones and the dykes) is most likely to be correct. As a consequence, the conclusion (p. 193, line 25) that the shear zones represent a “conjugate shear stress system” is unsubstantiated.

In addition to the problems of shear zone-dyke relationships; there are several other points which have been made that seem to be unsubstantiated assumptions and conclusions. These are discussed in the order in which they appear in the paper. The Gruinard Bay area contains layered mafic rocks and “trains of agmatitic banded amphibolites” (p. 189, last para. and p. 190, last para.) which are similar (presumably mineralogically) to others scattered throughout the Lewisian. Because of this similarity, they are used to compare “the origin and relative ages of the felsic gneisses which make up the bulk of these complexes” (p. 190, line 5). Without isotopic ages and geochemical evidence, it would not seem possible to say whether or not the basic and ultrabasic bodies are of the same age or even of similar ages.

The felsic gneisses are made up a series of metamorphosed intrusive igneous rocks and there are “distinct differences in the way in which incoming granodiorite behaved relative to the basic units when compared with sheet-like intrusions of similar rocks intruded into early basic bodies at Scourie”. (Davies, p. 192, line 12) What these differences are and the evidence on which they are recognized is not made clear. Hence, any account of the geological history of the area must be regarded as tentative.

In discussing rock types, Davies (p. 192, line 16) mentions “discordant metadolerite dykes” and has argued on the assumption that all of the dykes are of the same age (apparently without isotopic evidence). Although he may be correct in this assumption, he does not appear to have explored the possibility that the dykes may be of more than one age. The proven existence of more than one set of dykes cutting the Lewisian, separated by phases of folding, at other localities such as south of Scourie (Bowes & Khoury 1965), Barra (Hopgood 1971) north of Loch Laxford (Dash et al. 1974), and on Iona (Fraser 1977) indicates that all localities should be examined with this possibility in mind.

Throughout the paper, Davies assumes that any metadolerite must be a member of the ‘Scourie Dyke’ swarm. Davies also states that the metadolerites (and by inference, all Scourie Dykes) are probably part of the same dyke swarm as similar dykes described by Escher et al. (1975, p. 193, line 7). He does this without reference to isotopic or geochemical data. In the light of the evidence of Bowes & Khoury (1965), Hopgood (1971), Dash et al. (1974), and Fraser (1977) it would seem unwarranted to correlate all metadolerites with other metadolerites from west Greenland.

That previously mentioned “conjugate shear stress” system in the paper has the major stress axis in the wrong quadrant when compared with the normal’ situation, a condition that is the same as that mentioned by Escher et al. (1975) from west Greenland. However Davies does not refer to this in his paper at all. In his two-fold-phase structural evolution, Davies infers the pre-refolding orientation of the early fold phase. This is done after the early folds have been refolded by “close or tight major folds” (p. 192, line 26). It would seem that after a major refolding, the axial
planes of the early folds would be refolded (as well as the rock layers themselves) and that reconstruction of the fold orientation before refolding would not be possible. In addition there is no evidence relating to any possible rotational (or other) movement of the block of crustal material in which the Gruinard Bay region lies.

Finally, Davies states (p. 192, line 34) that the age relationships of the shear zones relative to the "large early isoclines" is the same as that for other similar areas. Although the isoclinal folds are NW dipping (hence NE striking) and the shear zones trend NW-SE, he does not say what the relationship between the two features is. Regrettably, without a table summarizing the structural history of the area or comparing it with other areas, one is left to assume that the shear zones relationship to the "large early isoclines" is to cut the isoclines.

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Sirs,—I thank Dr. Field for his valuable additional information to the understanding of the geology of Gruinard Bay.

In reply to Mr. Price: I accept that in my effort to be concise over this descriptive work I may have condensed too far the factual basis on which some conclusions are based. I apologize for any confusion my statements have caused Mr. Price regarding the network of metadolerite dykes and their relationships to shear zones in the Gruinard Bay area. I re-emphasize that the network of dykes in question are amongst the largest members of a distinct swarm of dolerite dykes, the larger individual members of which have strike lengths of up to 20 km in length and are up to 150 metres wide. Detailed examination of the intersections of dykes of the two directional sets demonstrates that the dykes are part of a single magma injection; dykes of different directions do not cut each other but are part of a contemporaneous branching network. The bulk of the shear zones are cut by these metadolerites and therefore must have originated prior to dyke emplacement. A few of these displace, truncate or produce tectonite fabrics in large metadolerites which branch from the major dykes which cut the early shear zones. This information can be seen on the diagrammatic map (Davies 1977, Fig. 1). It is therefore clear that the simplest explanation for shear zone and dyke relationships is that whilst the bulk of the shear zones were active prior to dyke emplacement, some pre-dyke shear zones were active after the emplacement of this major dyke network, although not necessarily active along the entire length or across the entire width of the pre-dyke shear zones. The confusion for Mr. Price seems to be related to highly controversial observations which he cites, of minor ‘boot-lace’ dykes from other localities in the Lewisian which do not refer to Gruinard Bay or the type Scourian and Laxfordian for that matter and do not belong to the major group of dykes defined by the Geological Survey (Peach et al. 1907) specifically discussed in the paper. However, Mr. Price is right to the extent that this problem has previously been debated without agreement being reached and I should have taken more care about indicating how my conclusions were derived. The critical point is that the evidence indicates that emplacement of dykes of this major network was not punctuated by an interval during which some of the structures were formed.

I am aware that the unusual stress configuration also noticed by Mr. Price which occurs at Gruinard Bay, is certainly not the usual configuration cited in present text books, but this case is not unique (acknowledged by Mr. Price). I am not going to discuss this problem in these pages because it is an important observation relevant to the topical problem of ‘Indenter Tectonics’ (Molnar and Tarponnier 1975) which a number of researchers including myself, are currently investigating.

I consider my statements on the two major fold phase evolution of the region to be perfectly clear. It is a simple geometrical relationship of folds that the orientation of the axes of folds with consistent axial surface orientations are dependent solely on the
orientation of the earlier marker surfaces. Since at Gruinard Bay the early markers are
the limbs of the early isoclinal folds, the large-scale enveloping surface of the axes of
the later folds defines the orientation of the earlier major isoclinal fold limbs and hence
(by definition) the orientation of the coincident early axial surfaces.

I agree that as a general rule I would not rely on lithological similarity as a basis for
correlation. However, regarding the layered mafic rocks similarities extend not only
to lithologies but to lithological associations and relative ages of rock groups on a
regional scale and have been known since the time of the Geological Survey (Peach et
al. 1907). Their significance with which I agree, in regional correlations has been
emphasized by Bowes, Wright and Park (1964), Bridgwater, Watson and Windley
(1973) and Davies and Watson (1977). My opinions of the inferred regional signifi­
cance of the rocks of the Gruinard Bay complex are consistent with the published
views of these and other workers and on the results of a 600 sq km survey on 1:10,000
scale of the mainland Lewisian from Loch Inchard to Loch Torridon where I have
traced many individual layered mafic complexes for tens of kilometres throughout
extremely complicated structures. This survey has been accompanied by a major
investigation of the whole rock, mineral and isotope chemistry of several regions
including Gruinard Bay selected from this regional survey, now nearing completion
at the University of Leicester. This work demonstrates a close similarity based on
whole rock chemistry between the intrusive tonalitic gneisses at Gruinard and those at
Scourie (H. Rollinson, personal communication).

Dr. Field and Mr. Price have provided valuable additions to the geology of
Gruinard Bay. Of the criticisms, I do not believe that they detract from the informa­
tion I presented on the geological evolution of the complex or the important and I note
uncontested statement, that the Gruinard complex contains several distinct intrusive
phases and age-groups of Archaean gneisses of plutonic origin.

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