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CORRESPONDENCE AND NOTES

The English Lake District batholith – Ordovician, Silurian, Devonian or...?

SIRS – Perhaps it is surprising that such a speculative paper as that of Firman & Lee (1986) should appear in a symposium volume entitled *Geology in the Real World.* Nevertheless, as Sir Kingsley Dunham stated in his valedictory remarks to the conference (Dunham, 1986) 'geology like any other discipline can inhabit an unreal world of imagination; perhaps it can benefit from short spells like this'. Whether or not our paper proves, ultimately, to be 'real' or 'unreal' it was written to stimulate the imagination of others and, judging by the letters to the editors, it has done just that. However, the letter from Webb *et al.* is concerned as much with the contribution from Soper as with our original paper, and the contribution from Allen extends the discussion further into the realm of Caledonian plate tectonics. Gratifying as it is to see such discussion arising from our ideas, we will limit our comments mainly to points discussed in our original paper and also warn against both over-simplification and too early an application of the 'reinforcement syndrome'.

We would be unhappy if the Lake District batholith, formerly uncritically spoken of as Devonian, were to become known, as a result of our arguments, as either an Ordovician (Soper) or an early Silurian (Webb *et al.*) batholith. The Lake District batholith, like many others, is composite both in time and composition. Even if we are correct and the Ennerdale and Eskdale intrusions, together with much of the concealed batholith, are late Ordovician in age, gravity anomalies associated with the Devonian (*c.* 400 Ma) Shap and Skiddaw intrusions suggest that the volume of late granitoids is substantial. Moreover, neither our model nor the gravity data (Lee, 1984, 1986) precludes the existence of other Devonian granites and granodiorites peripheral to, or even underpinning, earlier intrusions. Indeed it seems likely that an early Devonian intrusion is responsible for the Crummock Water aureole which extends along part of the northern flank of the batholith (Cooper *et al.* in prep.).

Nor should the possibility of earlier plutons be totally discounted. O'Brien *et al.* (1985) showed that the Carrock Granophyre has such close geochemical affinities to the Llanvirn Eycott Volcanic Group (*c.* 470 Ma) that in spite of its apparent Rb/Sr age of 415±20 Ma (Rundle, 1979) the granophyre might also be of Llanvirn age. In addition, although the Threlkeld Microgranite (most recently dated at 438±6 Ma; Rundle, 1981) is best modelled as a high-level laccolith, the gravity data do not totally preclude the existence of a batholith component of similar age. However, we would not wish to infer from this that substantial parts of the batholith were intruded much before late Ordovician time.

Thus, although the true meaning of Rb/Sr isochrons in terms of crystallization, emplacement and metasomatism are debatable, the published Rb/Sr isochrons do indicate episodic acid igneous activity during a period of at least 40 Ma (from *c.* 440 Ma to *c.* 400 Ma) and possibly longer. Although we have argued that there is evidence for a substantial late Ordovician component of the batholith, the evolution of the batholith as a whole is related to a complex series of magmatic events.

Against this background, the statement of Webb *et al.* in their letter that 'the batholith' was 'emplaced early in the tensional regime under which Silurian basins developed' seems to us to be a considerable oversimplification. Moreover, there is evidence to suggest that the tensional regime during Silurian time is an unlikely period for the bulk of the batholith to have been emplaced. For instance:

(a) There is no evidence of Silurian volcanic activity in the Lake District. Minor intrusions cutting Silurian strata are rare and, as far as we are aware, were all intruded after the penetrative Devonian cleavage had developed.

(b) Measurement of 8600 Eskdale Granite joints (Firman, 1960) failed to reveal the patterns of radial and concentric joints which typify many intrusions which cool in tensional regimes. Instead the study showed a singular paucity of joints which could be ascribed with confidence to cooling cracks, and a wealth of joint patterns consistent with a stress system in which the principal compressive force was north–south. That this stress system was probably operative before the granite had completely consolidated is suggested by the development of zones of rotational shear adjacent to wrench faults within the granite, but not in the country rocks (Firman, 1960).

(c) The fabric of both the Eskdale Granite and Granodiorite is inconsistent with intrusion during a tensional regime. In the granite almost all the early quartz is strained, also in the granodiorite feldspars and garnets are fractured and biotites distorted (Ansari, unpub. Ph.D. thesis, Univ. Nottingham, 1983). These features suggest to us that either the intrusions suffered compressive stress during crystallization or that early-formed minerals were strained during the intrusion and emplacement of a partly crystallized magma.

(d) Throughout the Eskdale Granite, pervasive metasomatism, which possibly did take place in a tensional regime, has produced a later post-tectonic fabric of alkali feldspars, unstrained quartz and muscovite. The Rb/Sr date for the Eskdale Granite of 429±4 Ma (Rundle, 1979) may legitimately be regarded as either late Ordovician or early Silurian depending on whose timescale one chooses to believe (cf. Snelling, 1985). Whichever timescale is favoured it should be remembered that 429±4 Ma is not an emplacement age, it merely indicates the time when the Eskdale Granite became closed to Rb and Sr; most probably a post-tectonic metasomatic event.

Therefore, given a choice between an early Silurian or late Ordovician emplacement, and given the evidence for emplacement in a compressive regime, we opted for an emplacement late in the pre-Ashgill deformation when ductile periclinal folding was giving way to wrench faulting and associated jointing. We do not claim that this model of the emplacement of the Eskdale Granite is necessarily correct, but it is, in our view, a model which most closely fits the facts as we know them today. We need more definitive data from which we can more confidently judge the age of emplacement – perhaps by U/Pb zircon dating and Sm/Nd determinations. If we are correct it implies that faults and joints in the granite were developed before the Coniston Limestone was deposited. This aspect of our hypothesis

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needs to be rigorously tested, and for this reason we are delighted to learn of the remapping of the Ashgill unconformity; we await the results with interest.

Having concluded that the evidence suggests that the Eskdale Granite and Granodiorite were finally emplaced during a period of wrench faulting which followed the formation of pre-Ashgill folds such as the Ulpha Syncline, we further suggested that these intrusions may have caused, either during emplacement of later, an eastward tilt to the pre-Ashgill folds. Both Soper and Webb *et al.* mention in their letters that the Eskdale Granodiorite transects the Ulpha Syncline. We were aware of this and do not consider it inconsistent with our model, since diapirs frequently pierce the very structures they have created or modified.

We are pleased that Webb and his colleagues accept the existence of folds pre-dating the Coniston Limestone unconformity and we agree that these are periclinal, probably at least in part coaxial with the later Caledonian folds, and hence it is difficult to distinguish axial trends one from another. Nevertheless we feel that it is more than a coincidence that the orientation of the maximum principal stress deduced from the study of joints in the Eskdale Granite (Firman, 1960) is closely similar to that deduced from the axial traces of the Ulpha Syncline as mapped by Numan (in Soper & Moseley, 1978) and markedly oblique to that required for structures with 'Caledonoid' (NE–SW) trends. We are not yet dissuaded from our view that both are due to pre-Ashgill stress, but hope that Webb, Millward, Johnson & Cooper will agree that more research is needed, not only into possible effects on pre-existing structures of the intrusion of early parts of the batholith, but also of the likely effect of later folds (e.g. Black Coombe Anticline) with only slightly oblique trends on earlier folds (e.g. the Ulpha Syncline).

Whilst we hope that Webb *et al.* would agree that *if* the Eskdale Granite and Granodiorite are as old as we suggest, it is improbable that they had *no* effect on pre-existing structures and subsequent sedimentation, we would, however, not wish to overemphasize its influence. Although Dr Soper's suggestion that the Caradoc/Ashgill transgression might itself be due to cooling and contraction of the batholith is intriguing, it should be borne in mind that this transgression also occurs in areas away from the influence of the Lake District batholith.

It is but a small logical step from deciding that the exposed parts of the Eskdale Granite and Granodiorite are most probably older than the Coniston Limestone unconformity, to postulating that the pronounced negative gravity anomaly coincident with these intrusions represents a substantial volume of granite of the same age. To go from there to propose that the gravity anomaly coincident with the Ennerdale Granophyre might also represent a pre-Ashgill intrusion is, we admit, far more speculative, as is our proposal that much of the concealed batholith eastward to Haweswater might be of the same age. Gravity modelling (Bott, 1974; Lee, 1986; Cooper *et al.* in prep.) suggests that the Ennerdale Granophyre (dated at 420±4 Ma; Rundle, 1979) is a relatively thin body underlain by a substantial granite/granodiorite intrusion. Although the age of this and the rest of the concealed batholith is unknown, the form of the gravity anomalies seems to us to suggest an association with the Eskdale Granite rather than with the early Devonian Shap and Skiddaw granites, which are steep-sided plutons peripheral to the main batholith. Having, in Sir Kingsley Dunham's words, inhabited 'an unreal world of imagination' and we hope benefited from it, we felt obliged to examine the possible effects of such an early batholith in supplementing tectonic forces and influencing sedimentation in Ashgill and subsequent epochs.

We must reiterate that the point of our paper was not to suggest that the batholith was *wholly* responsible for Caradoc and Ashgill structures and sedimentation but merely that its emplacement and existence *aided* and *abetted* the tectonic forces which Webb *et al.* so rightly emphasize. It is this scenario which Soper finds 'entirely credible' and Webb *et al.* dismiss as 'overstressed'. When people so familiar with Lake District geology disagree the reader must make up his own mind from the available evidence. For our part, whilst we welcome Dr Soper's support, we are acutely aware that a great deal more research 'in the real world' is needed before our proposals can be considered to be reasonably proven. We therefore prefer to leave speculation about the wider implications of our model, both in the Lake District and the Pennines, until more evidence is available. We look forward to continued fruitful discussions and co-operation with our colleagues in the British Geological Survey and geology departments elsewhere and thank them for their stimulating contributions to date.

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