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Back-rotation during crenulation cleavage development: implications for structural facing and cleavage-forming processes: Reply

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1. Introduction

Kraus has commented critically on Johnson (1999a), but his criticism is based largely on misrepresentations and unfounded assertions, and so I take this opportunity to: (1) clarify what Johnson (1999a) said, versus what Kraus claims he said; (2) point out where Kraus makes unfounded, in some instances demonstrably incorrect, assertions regarding deformation and resulting geometry in these rocks; and (3) show that his main criticism effectively restates a principal conclusion developed by Johnson (1999a).

2. Misrepresentations

Below, I list three instances where Kraus misrepresents statements by Johnson (1999a). Brief clarification of these three instances is important because Kraus uses them as a partial basis for his criticism.

1. In his introduction, Kraus says "Johnson claims that an S_4 crenulation cleavage developed from an S_3 differentiated layering in the incompetent pelitic beds, but not in the psammitic beds." In contrast, Johnson (1999a, p. 142) stated "S_3 and S_4 are readily visible in the metapelitic tops of the layers, but in the metapsammitic bases S_4 is poorly developed…".
2. In his introduction, Kraus says the following as part of his summary of Johnson (1999a): "In the competent psammitic beds, S_3 is undeformed, and is either parallel or at a low angle to S_0 with a sinistral S_0/S_3 asymmetry…". In contrast, Johnson (1999a) did not state that S_3 in the metapsammitic beds was undeformed.
3. In his introduction, Kraus says "The original orientation of S_3, he speculates, was identical in all rock types, and is preserved in the psammitic beds…". In contrast, Johnson (1999a) did not "speculate" about the original orientation of S_3.

3. Discussion

In his discussion section, Kraus raised five issues, which I address in order.

1. Johnson (1999a) was concerned with specific geometrical relationships and their implications for structural facing determinations; however, details of the deformation history in the area, and answers to Kraus's specific questions, can be found in Johnson (1999b).
2. Kraus makes several unfounded assertions regarding strain fields, foliation refraction and shear strains in the Cooma rocks, some of which are demonstrably incorrect. For example, Kraus asserts that "…S_3 in the pelites must have been approximately parallel to S_0 after F_3, because, otherwise, S_3 would not have been in the F_4 shortening field and subject to crenulation…". I do not know how Kraus determined the incremental strain ellipsoid or defor-

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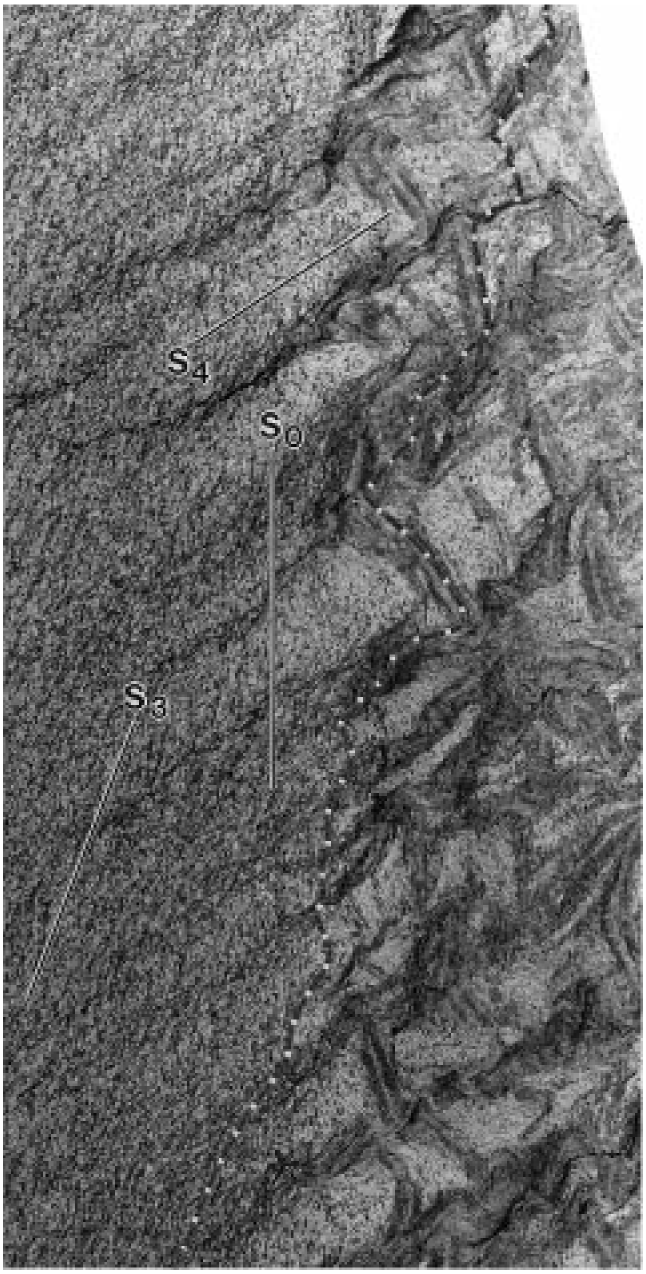

Fig. 1. Photomicrograph of the same sample shown in fig. 7 of Johnson (1999a), illustrating the transition from metapsammitic base (left) to metapelitic top (right) in a graded metaturbidite couplet. No S_0 marker surface is present, and so the S_0 line represents the thin-section-scale trace of S_0. The dash–dot line follows the approximate trace of a single S_3 folium across the transition, well into the metapelitic top. Very small microfolds of this folium could not be precisely represented owing to limited flexibility of the tape used to mark the photomicrograph; however, the overall trace of the folium, as determined using a microscope, is faithfully reproduced. Any reasonable enveloping surface to this crenulated folium is nearly parallel to S_3 in the metapsammitic base of the couplet, contrary to Kraus's assertion that it is approximately parallel to S_0. See fig. 7 of Johnson (1999a) for further discussion.

mation path during D_4 in these rocks, which should be prerequisites to such a statement. Also, I do not know how Kraus determined an enveloping surface for S_3 through the S_4 crenulation cleavage by looking at the figures in Johnson (1999a). Accurate tracing of crenulated micaceous foliations through a well-developed crenulation cleavage can be difficult, particularly where recrystallization and neocrystallization of phyllosilicates in the septa have broken the continuity between septa and microlithons, and where the septa have been affected by subsequent deformation. Fig. 1 shows an attempt to trace S_3 in the same sample shown in fig. 7 of Johnson (1999a). A microscope was used to follow a single S_3 folium, which was simultaneously drawn on a photomicrograph. The resulting S_3 enveloping surface in the metapelitic top is in fact nearly parallel to S_3 in the metapsammitic base; thus, Kraus's assertion, upon which he bases much of his criticism and his 'reinterpretation', is demonstrably incorrect.

Kraus also asserts that "...the F_4-related S_0-parallel shear strains appear to be smaller than the F_3-related shear strains...". I do not know how Kraus determined relative shear strains by looking at the figures in Johnson (1999a), and the S_3 enveloping surface in Fig. 1 does not support his assertion. Finally, Kraus claims to present a "more realistic" interpretation regarding the pre-D_4 orientation of S_3 in the graded layers, but it is only realistic if he has some evidence for it. Fig. 1 shows that S_3 in the metapelitic top is nearly parallel to S_3 in the metapsammitic base, and so, in this instance at least, Kraus's interpretation is demonstrably incorrect.

I acknowledge that fig. 8 of Johnson (1999a) may not precisely represent the orientation history of S_3, but that was not an aim of the figure; it was only intended to show, schematically, the back-rotation process and the generalized relationships between S_0, S_3, S_4 and porphyroblasts. I concluded before writing Johnson (1999a) that small variations in S_3 orientation, owing to refraction of S_3 across S_0, was not an important controlling factor on the geometry described; I maintain this view.

3. Again, Kraus misrepresents Johnson (1999a) regarding the development of S_4 in the metapsammitic bases. S_4 is obviously present in the metapsammitic base shown in fig. 7 of Johnson (1999a), but I disagree with Kraus's assertion that it is "well-developed". These rocks are graded metaturbidites, and so the intensity of S_4 diminishes strongly towards the metapsammitic bases of the graded couplets, where, in my opinion (Johnson, 1999a), it is poorly developed. Regarding refraction of S_4 during F_4 folding, Johnson (1999a) acknowledged this in the caption to his fig. 8. However, some of the S_4

refraction in Cooma was caused, or accentuated, by subsequent deformation (Johnson, 1999b).

4. Kraus states his main criticism that "...Johnson has no compelling evidence for a dextral S_0/S_3 asymmetry in the pelitic beds and thus for contrasting structural-facing directions on S_3 in the psammitic and pelitic beds (his fig. 6). "In fact, the suggested orientation of S_3 relative to S_0 in the pelites is an *optical illusion*..." This comment surprised me, because I thought Johnson (1999a, p. 142) was very clear in developing this same idea as one of his main points; namely that interpreting relationships shown in figs. 3 and 4 of Johnson (1999a) as indicating downward facing on S_3 would be incorrect because the S_3/S_0 relationships are misleading (Kraus prefers to call it an "optical illusion"). Thus, Kraus seems to have rephrased, rather than contradicted this point. Kraus continues to defend his assertion that S_3 in the metapsammites is approximately parallel to S_0, but it is not (Fig. 1), and so his arguments seem irrelevant.
5. Kraus makes unfounded assertions regarding the development of crenulation cleavage in these rocks, apparently in relation to some assumed deformation history, and claims to know both the local and bulk vorticity sense during D_4. Apart from the fact that determining vorticities in deformed rocks is a notoriously difficult problem, crenulation-cleavage development is incompletely understood, particularly regarding the degree of non-coaxiality, sense of vorticity (at different scales) and timing relative to macroscale fold development. At any rate, the term 'back-rotation' was clearly defined by Johnson (1999a, p. 143) as rotation of S_3 in the S_4 microlithons relative to rotation of S_3 in the S_4 septa, the reference frame being the S_4 crenulation cleavage. I sought a non-genetic term for the process, and 'back-rotation' was what I came up with. Kraus is welcome to describe the process differently, but, in my opinion, genetic terminology should be avoided.

4. Concluding remarks

The principal aim of Johnson (1999a) was to document some unusual geometrical relationships that might help further our understanding of crenulation-cleavage development, and to point out that misinterpretation of these geometries could potentially lead to serious misinterpretation of macroscale structure. Johnson (1999a, p. 142) stated that downward structural facing on S_3 was possible after *initial field observations*, and that detailed microstructural work, combined with careful field observations at key localities, were required to make the correct interpretation. The rocks at Cooma are very well exposed in creek beds, and so the potentially misleading geometrical relationships were relatively easily resolved. However, in less well exposed areas, deformation-induced (apparent) reversals in facing and vergence might not be as obvious or as easy to resolve, and it was partly for this reason that Johnson (1999a) was written. A very similar point was made by Lisle (1988), who showed different ways in which vergence can be (apparently) reversed by later deformation.

Kraus concludes his criticism by stating that "...the 'back-rotation' proposed by Johnson has no bearing on structural facing as long as the orientations of crenulated foliations are determined correctly...". I was surprised by this statement because, in my experience, it is commonly very difficult to determine enveloping surfaces of strongly crenulated foliations in mica-rich schists (particularly in the field), owing mainly to the lack of reliable markers, and the inherent uncertainties of tracing micaceous foliations through crenulation septa. This is perhaps most fittingly demonstrated by Kraus's own incorrect determination of the S_3 enveloping surface for the rock shown in Fig. 1. I disagree with Kraus's apparent dismissal of the problem, and I also disagree with his statement that this discussion is particularly relevant to low-grade greenstone and slate belts; complex geometries and fabric relationships, and the development of multiple crenulation cleavages, are also common at intermediate to high grades of metamorphism.

Where crenulations are asymmetrical (i.e. on fold limbs) and the crenulation cleavage is well developed, unequivocal evidence for systematic microlithon rotation relative to the developing crenulation cleavage is relatively rare. The example presented by Johnson (1999a) seems particularly clear, and as such it has some important implications. Some of these were mentioned by Johnson (1999a), and here I reemphasize one of them. Porphyroblasts in the metapelitic top of Fig. 1 contain S_3 as inclusion trails, and grew either before or during the development of the S_4 crenulation cleavage (Johnson and Vernon, 1995). If the foliation in the crenulation hinges rotated counterclockwise relative to the developing crenulation cleavage, as argued by Johnson (1999a), then the porphyroblasts must also have rotated counterclockwise. Unequivocal evidence for this type of porphyroblast rotation is extremely rare, and the topic has caused considerable debate over the past 15 years (reviewed by Johnson, 1999c). Are the rocks described by Johnson (1999a) an isolated example, or is this type of porphyroblast rotation common during crenulation-cleavage development? One similar example that I am aware of was described by Henderson (1997).

Crenulation cleavage is one of the most common structures in multiply deformed metamorphic rocks, and a better understanding of its formation and mechanical significance may aid our understanding of other processes such as folding, and porphyroblast kinematics during folding and cleavage development. More studies of natural examples are needed, and the following features are desirable for determining systematic microlithon rotations relative to the developing crenulation cleavage: (1) the presence of bedding, a pervasive foliation cutting it at a moderate to small angle and a crenulation cleavage that overprints both; (2) a marked gradient in crenulation-cleavage intensity across layers of different composition; and (3) the crenulated foliation is well developed across all compositions, allowing a comparison of its orientation relative to bedding across the crenulation gradient. Where informative natural examples are found, questions that could be addressed include the following. (1) What causes back-rotation (if any), and does this depend on the deformation path and fold model? (2) What is the incremental strain history at both the sample and cleavage scales? (3) Does back-rotation occur very early during mesoscale/macroscale folding, or at some intermediate stage in the folding history? Does this depend on the fold model? (4) What fold models are compatible or incompatible with this process? (5) If this process is widespread, why are porphyroblast inclusion trails commonly consistently oriented around mesoscale and macroscale folds?

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