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Nucleation, growth and structural development of mylonitic shear zones in granitic rocks: Reply

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We thank Tikoff *et al.* (1998) for their comment on our paper and are delighted to have provided a forum for them to express their views and call attention to their own field investigations in the Sierra Nevada. In particular we appreciate their highlighting some of the recent publications on the intra-batholithic structures in the central Sierra Nevada. While we agree that we did not incorporate much of this regional geologic work in our paper (Christiansen and Pollard, 1997), we do not think that this material has much impact on our fundamental conclusions.

In reply, we would like to re-emphasize the objective of the original paper which was to study the processes involved in the initiation and development of shear zones at the *outcrop* scale. Many of the comments of Tikoff *et al.* (1998) pertain to their interpretation of the *regional* geologic significance of these and other structures, a subject upon which they themselves have clearly done considerable work. We intentionally did not discuss the regional tectonics because the focus of the paper was on processes at a much smaller scale. We disagree that an understanding of processes at the outcrop scale is 'hampered' by not being fully acquainted with the work described in Tikoff *et al.* (1998).

Although they apparently misunderstood our original objective, Tikoff *et al.* (1998) do bring up a number of interesting points which we would like to address directly.

1. Tikoff *et al.* (1998) are correct that our original paper did not describe the relationship of the shear zones we mapped to the Rosy Finch shear zone. They state that according to their fig. 1, the area we studied lies on the margin of the Rosy Finch shear zone, implying that examination of the geological map is itself a geological observation! We suggest that geological observations should determine the appearance of the geological map, not the other way around. If a regional-scale shear zone exists in the area we studied, the deformation is of a comparatively small magnitude, small enough that even a weak penetrative deformation fabric is not generally observed at the outcrop or hand-sample scale; apparently a microfabric exists that produces an anisotropy in magnetic susceptibility (Tikoff *et al.*, 1998). In contrast, deformation within the outcrop-scale shear zones is much larger: strain magnitudes of up to 100 were measured by Christiansen and Pollard, 1997 (their table 1). Even under the best of circumstances, drawing conclusions about the kinematic association of a geological structure with a strain magnitude too small to produce a visible fabric is likely to be very difficult. In this case, such a regional strain field clearly is overwhelmed by the much larger strains associated with the outcrop-scale shear zones.
2. Although the emplacement of the Mono Creek pluton is an interesting subject, study of pluton emplacement mechanisms was beyond the scope of Christiansen and Pollard (1997). In the sentence quoted by Tikoff *et al.* (1998), we did not intended to imply either an 'active' or 'passive' process of magma emplacement.
3. The pressure–temperature conditions under which the left-lateral shear zones formed are not well constrained. As discussed in Christiansen and Pollard (1997), existing radiometric dating and cross-cutting relations constrain the shear zones to have formed within 11 Ma, and probably within 6 Ma of crystallization of the host granitoids. From this, one can

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infer approximate limits of pressure and temperature (Christiansen and Pollard, 1997). However, further work is required to place more precise constraints on pressure–temperature conditions.

Tikoff *et al.* (1998) correctly point out lingering problems in precisely constraining the timing of events; they also correctly point out that processes in the post-magmatic batholith were likely characterized by steep and transient thermal gradients and complex kinematics.

In discussing outcrop-scale shear zones in the study area and relating them to regional-scale structures, Tikoff *et al.* (1998) neglect an important distinction. Unfortunately, this distinction was not sufficiently emphasized in Christiansen and Pollard (1997). The objective of our paper was to describe the origin and development of a *particular type of shear zone, namely sheared aplite dikes*. Sheared aplite dikes are cross-cut and offset by mineralized fractures of the type described by Segall and Pollard (1983), Segall *et al.* (1990), Martel *et al.* (1988), and Bürgmann *et al.* (1994). These mineralized fractures include opening-mode joints and left-lateral faults; white mica from within some of the faults gives a ^{40}Ar/^{39}Ar date of 79.2 ± 0.8 Ma (Segall *et al.*, 1990). Locally, these left-lateral faults served to nucleate zones of distributed shear (Segall and Simpson, 1986; Kronenberg *et al.*, 1990; Bürgmann and Pollard, 1992, 1994). Therefore, the field relations indicate that the sheared aplite dikes are older than the shear zones associated with mineralized fractures. Could it be that many of the shear zones described by Tikoff *et al.* (1988) (e.g. those illustrated in their figs 2 and 3) are of this second type and therefore are younger than the sheared aplite dikes described by Christiansen and Pollard (1997)?

4. The ^{40}Ar/^{39}Ar dates of 80–76 Ma cited by Tikoff *et al.* (1998) are similar to K–Ar and ^{40}Ar/^{39}Ar dates from throughout the central Sierran batholith. These dates are usually interpreted as cooling ages. As Tikoff *et al.* (1998) correctly point out, conditions within the batholith during and after magma emplacement probably involved large and rapidly changing thermal gradients. In such a setting, one must be careful about inferring the date of shear zone development as it may not be clear whether shearing took place at temperatures above or below the argon closure temperature. Indeed, how these dates constrain the age of shearing is not clear from the discussion of Tikoff *et al.* (1998). Incidentally, if two structures are 'kinematically linked', they are not just 'likely' to have been active simultaneously, but by definition *were* active simultaneously.
5. Outcrop-scale, left-lateral shear zones formed at broadly the same time as regional right-lateral shearing and under broadly similar pressure–temperature conditions. However, we disagree that the two types of structures are 'conjugate', in the sense that this term is usually used in structural geology. The Rosy Finch shear zone is a very wide zone of distributed shear (Tikoff and Teyssier, 1992), the magnitude of which is not well constrained but it appears to be several orders of magnitude less than the shear zones we studied. We agree that an overall synthesis of the geologic history of the area requires an understanding of the timing and kinematic association of structures of various types. But Tikoff *et al.* (1998) fail to illustrate the association of the Rosy Finch shear zone to the structures described in Christiansen and Pollard (1997), aside from the broadly synchronous time of formation, and their claim of a 'conjugate' orientation, which we dispute.

It is sound practice when studying the mechanics of shear zones to define a remote field in which the displacement gradients are negligible. This serves to focus attention on the shear zone itself, and obviates further consideration of structures in the remote field. This was the intent in Christiansen and Pollard (1997) for deformation associated with the Rosy Finch shear zone. Tikoff *et al.* (1998) have not persuaded us that this approach is flawed.

CONCLUSIONS

When studying the geological history of a sheared terrane, one must be careful to distinguish shear zones on the basis of more than simply their orientation and sense of relative motion. For the shear zones described by Christiansen and Pollard (1997), the offset of older structures provided unambiguous data necessary to determine the magnitude, sense, and direction of shearing. Furthermore, cross-cutting relations show unambiguously that there are *at least* two generations of outcrop-scale, left-lateral shear zones in the study area: sheared aplite dikes, and shear zones associated with mineralized fractures. Especially because it is not clear how much time passed between formation of the shear zones of different types, their origins were considered separately.

Lastly, we did not mean to imply that *all* shear zones form by shear localization along aplite dikes and apologize to any readers that may have got this impression. We believe that given the geological evidence described in Christiansen and Pollard (1997) it is clear that *at least these shear zones* formed by shear of aplite dikes. Specifically, we cite:

1. the preservation of aplite dikes with various degrees of mylonitic fabric development;
2. the observed qualitative correlation between the intensity of mylonitic fabric development and the apparent offset of crossing markers; and

3. the observed correlation between the orientation of a continuously curved aplite dike, the occurrence of mylonitic fabric, and measured offset of crossing markers.

In Christiansen and Pollard (1997), we present a well constrained field example of shear zone initiation on a pre-existing material heterogeneity and propose that this is a plausible mechanism for the initiation of at least some shear zones in other geological settings.

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