Weinberg et al. (2004) present an intriguing model to explain why plutons might be emplaced in low stress sites next to faults. In a Geology paper it is difficult to fully present all aspects of such models, and I remain puzzled by some of the authors’ interpretations. Thus, in the hope that further discussion will help the community carefully evaluate this model, I encourage Weinberg and co-authors to clarify their model through discussion of the following issues.

1. The authors state that there is a common link between synkinematic plutons and shear zones. It is true that most plutons are synkinematic, but many are not spatially associated with faults (Paterson and Schmidt, 1999; Schmidt and Paterson, 2000). Some plutons occur along faults, but since others do not, it seems important to search for and develop models and processes that would explain both relationships, particularly given that both types of plutons are typically temporally ascending in the same areas. This same issue arises in the second section, in which the authors note similar relationships in migmatites in which leucosomes and melts occur not only in shear zones, but also in other low stress sites. Migmatites also show examples of leucosomes and melt materials in other settings and commonly have larger dikes and stocks that crosscut structures. So shouldn’t we be paying attention to how all melts in migmatites and all plutons in other settings behave and not just selecting a few? This doesn’t preclude faults (or any structure that alters the stress, strain, and anisotropy) from playing a role during magma ascent, but it does suggest that models relying completely on faults may not be widely applicable.

2. Weinberg et al. (2004) describe four suites of plutons. They state that the oldest suite of plutons intrudes away from shear zones. This suite is ignored throughout the remainder of the paper. Of the 25–30 plutons shown on their map making up the remaining three suites, only 4 have “en echelon shapes with tails within shear zones” (p. 377). So only ~10–15% of the plutons show tails spatially associated with faults. Furthermore, regardless of shape, only ~11 (i.e., ~36%) of the plutons on the map intersect shear zones. Our largely unpublished computer modeling (see also Schmidt and Paterson, 2000) suggests that if plutons are randomly emplaced into faulted crust, around 30% of the plutons are expected to intersect faults just by chance. This percentage may dramatically increase or decrease depending on fault spacing, pluton size, and number of plutons. So the example in Weinberg et al.’s (2004) paper is well within the expected result for randomly emplaced plutons. Can the authors clarify their evidence for the preferential ascent of magma up faults and why they think plutons are not just intersecting faults by chance? Are they suggesting that their proposed model explains the emplacement of all the plutons on their map or just the 10% with tails?

3. Weinberg et al.’s (2004) interpretation of these four tails also puzzles me. They describe these tails as strongly deformed in the solid state. Can the authors show that there was previously a magmatic fabric in the tails and thus that the tails are not just a result of subsolidus deformation in the shear zones? One also wonders which way magma flowed in the tail, up and toward the pluton or away from the pluton and into the tail. I am also confused by the designation as tails since they are now exposed at the same crustal level as the main bodies of the plutons. The implication is that magma rose up the fault and then at the present crustal level abruptly turned 90° and fed a ballooning chamber outside the shear zone. Is this what the authors infer or do they have evidence of increasing depth along the tails? Vigneresse (1995) described a very similar pattern along the South Armorican shear zone, but the exposed tails are strongly deformed in the solid state and may be tectonic slices along the fault. Additionally, Vigneresse (1995) argued that the true feeder systems can be imaged below the plutons as vertical tails and that these tails do not merge with the nearby shear zones. A more convincing example of magmatic ascent in a shear zone was described by Rosenberg (2004) who showed that increasing pressures/depths occur along the tail and thus that the presently exposed section is tilted. Is there any such evidence for tails of this nature in the Borborema province?

4. Magmatic bodies are large thermal engines and temperature must play a huge role in their behavior, the behavior of the surrounding host rock, and in local stress fields. For example, Marsh (1982) and others have shown that the temperature effect of hot plutons can change local stress by several kilobars. What role(s) does temperature play in the Weinberg et al. (2004) model? I also could not determine how their model scales to natural systems. Is it the appropriate length scale to control the ascent and emplacement of large magma bodies? And what happens when magma starts to enter this region? Does the stress drop immediately or disappear?

Is the Weinberg et al. (2004) model appropriate for the ascent of large magma bodies, particularly by diapirism? I understand the role of stress variations in driving porous fluid of a fluid and how stress variations, if of the appropriate length scales, may control diking, but if a large mass of magma with a high effective viscosity, or potentially even a Bingham or Kelvin rheology, rises in the crust it potentially dominates the local stress (and strain) field(s) as Weinberg has shown in previous papers. Thus, how big of a role will local pressure variations have? Are they more important than the temperature variations or plastic yield strength variations in the crust?

5. Given the above concerns, why do Weinberg et al. (2004) focus on shear zones? Why not just conclude that any low stress sites of equal length scales to the ascending magma body may affect its ascent no matter how these low stress sites formed? And can we exclude alternative models, such as a model in which plutons ascend relatively randomly, partially controlled by a number of factors including stress variations, and that some ascended in or near faults, in which case they were affected by the local stress field around these shear zones?

6. Finally, would Weinberg et al. (2004) speculate on how the first suite of plutons ascended to their present crustal levels, since apparently shear zones did not play a role at that time, and why subsequent plutons could not have ascended the same way?

REFERENCES CITED


REPLY

Roberto F. Weinberg  
School of Geosciences, Monash University, Victoria 3800, Australia  
Alcides N. Sial  
Gorki Mariano  
Department of Geology, Federal University of Pernambuco, Recife, Pernambuco, Brazil

We appreciate the opportunity that Paterson’s comment provides to clarify and emphasize some important points made in the paper. In response to his point 1, we never implied that a single mechanism could explain syntectonic emplacement of granitic magmas. A number of competing mechanisms control magma ascent and emplacement, and our paper explores one mechanism that explains a common relationship between plutons and shear zones.

His point 2 is incorrect and indicates a superficial reading of the paper. First, we did not ignore the older, calc-alkaline suite. We used the fact that this suite is emplaced before shear zone development, and their lobate shape and distribution away from shear zones, to contrast with the en cornue plutons, emplaced later, in the vicinity of active shear zones. In the same rushed fashion Paterson misread the map in Figure 1. In that map, there is a large number of plutons that belong to the early calc-alkaline suite. In other words, there are not “25–30 plutons of the remaining three suites,” there are eight. Out of these, seven have en cornue shapes and are directly related to shear zones.

Point 3 is more interesting. Here Paterson wonders about the tails. We envisage a system where magma flowed initially as sheets within shear zones and that they ballooned outwards from there, into dilational regions of low mean pressures. We used the term tail in a purely descriptive sense to refer to the magma sheet in the shear zone. Magma migration and the feeding of the ballooning pluton is a three-dimensional problem and most of the magma that fed the growing pluton may well have come from underneath the exposed pluton. Unfortunately, due to straining of the tails, we are unable to demonstrate that a magmatic foliation predated solid-state deformation. Therefore we cannot entirely discard the possibility that the tails are parts of the main body of the pluton sheared in the solid-state by the shear zone. However, our main argument is the contrast in distribution of late plutons and the early calc-alkaline plutons. Late plutons lie in the immediate vicinity of shear zones, and early plutons lie away from shear zones.

In point 4 Paterson asks what role magma temperature played and what happens when magma enters the low mean pressure region. We can only speculate about those issues, as they were not part of our models and our speculative views are already expressed in the last paragraph of our original paper (Weinberg et al., 2004, p. 379). I would like to add here that the system is dynamic, and as magma batches intrude low-pressure zones seeking to equilibrate pressure gradients, low pressure is regenerated by the applied external stresses.

Why do we focus on shear zones (point 5)? Because they exert an obvious and direct control on contemporaneous magmatism. Sure, any dilational low mean pressure zone will play a role, but none may develop as strong a pressure gradient as large-scale faults. Finally, in point 6 we are asked to speculate on how the first suite of plutons ascended and why didn’t the later plutons ascend in the same way. Since we have not studied the emplacement of the early plutons, I would prefer not to speculate. To answer why the late plutons did not ascend in the same way, I go back to the first point above, that there are a number of alternatives for pluton ascent, and when the variables of the problem changed with the onset of localized shearing, the mechanism changed. The late en cornue plutons could have ascended in a number of ways independently of shear zones. The fact that these plutons, as well as many other worldwide, did not, must be telling us that shear zones play a key role in magma migration and emplacement.

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