PERGAMON

Journal of Structural Geology 23 (2001) 2029–2032

JOURNAL OF STRUCTURAL GEOLOGY

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# Reply to discussion of "Is there a close spatial relationship between faults and plutons?"

S.R. Paterson*, K.L. Schmidt

*Department of Earth Sciences, University of Southern California, Los Angeles, CA 90089-0740, USA*

Received 29 November 2000; accepted 12 March 2001

We are delighted that an ore deposits expert chose to write a discussion of our papers published in JSG and EOS, since one of our conclusions was that it would be interesting to apply our statistical approach to other geologic systems. Before proceeding, however, it is important to recognize that Richards' discussion is in part comparing 'apples and oranges'. Richards is concerned with ore deposits associated with low viscosity fluids that originated from or circulated near magma bodies, whereas we examined the regional distribution of large batches of high viscosity crystal mushes. It is not clear to us that the two systems should behave the same. However, we hope that quantitative studies, rather than qualitative visual observations would be used to evaluate both systems. In this context, Richards' conclusion that even the low-viscosity fluids associated with ore deposits tend to utilize second-order structures rather than regional faults is intriguing. Even so, although Richards twice states in his discussion that he in general agrees with our main conclusion, we believe that he is doing so for the wrong reason(s). We suggest that his comment is based on the same misconceptions prevalent in the pluton ascent/emplacement literature. Below we will address these issues along with other concerns expressed by Richards.

Richards begins by expressing surprise over the title of our second article (not published in JSG but in EOS). This was a title chosen by the EOS editors, although we had ample opportunity to veto it. Clearly it was meant to be a 'newsy' or provocative title and apparently served its purpose. In fact, we (and others completing statistical studies) do not find a close spatial relationship between regional faults and magmatic bodies, suggesting to us that these faults do not focus magma or play a significant role during emplacement of most plutons. A consideration of rates of processes also suggests that fault displacements cannot play a significant role: faults slip too slowly to accommodate magma ascent or emplacement rates. However, we have always recognized that pluton-related processes and fault-related processes may influence, and be influenced by, stress fields, thus providing a mechanism by which the two systems may 'interact' (e.g. Schmidt and Paterson, 2000). For example, there is an extensive literature that documents an interplay between stress fields and magma bodies at shallow crustal levels (e.g. Parsons and Thompson, 1991; Ildefonse et al., 1993).

Richards goes on to state that he agrees with our results because they only pertain to 'compressional orogens'. We assume that he means contractional orogens and in fact, we have statistical analyses from a variety of contractional to transpressional orogens ranging from those dominated by strike-slip faults to those dominated by thrust systems (Paterson et al., 2000; Schmidt and Paterson, 2000). All show the same general result, that is, lack of a close spatial relationship between plutons and regional faults. With regard to extensional settings, we note that it is much harder to statistically evaluate the relationship between regional faults and magmatism in these settings for three reasons: (1) in extensional settings faults typically dip moderately to shallowly, and thus vertical magma ascent may occur at moderate to high angles to these faults; (2) distance measurements between plutons and faults require projection above or below the map surface, thereby introducing further complexity to the analysis; and (3) very different crustal levels are commonly juxtaposed across these faults. Even trying to address the more general question of whether magmatism is more prevalent in regions undergoing extension (independent of the role of faulting) is problematic, because to do so it would be important to compare the amount of magmatism in these regions with the amount of magmatism at the same crustal level in unextended regions.

We also question Richards' statement in this paragraph that "most hypabyssal plutons related to magmatic

* Corresponding author. Tel.: +1-213-740-6103; fax: +1-213-740-8801.
*E-mail address:* Paterson@usc.edu (S.R. Paterson).


PII: S0191-8141(01)00041-4

hydrothermal ore deposits are emplaced in tensional or transtensional arc environments, albeit perhaps in a broadly compressional regime". We are uncertain as to what exactly Richards means by this statement since he is mixing stress and strain and because many of his referenced examples pertain to transpressional systems dominated by strike-slip faults. Assuming that his statement refers to settings where extension occurs in topographically high regions while deeper levels are under transpression, we note that it is often difficult to ascertain whether plutons in these settings were emplaced during extension or contraction (e.g. Paterson and Miller, 1998). We also wonder whether many of his examples represent cases where plutons were emplaced during regional contraction while nearby ore deposits formed in inferred low stress sites in the heterogeneous strain fields that typically occur around plutons.

Richards continues by presenting an example (his Figure 1) in which "structural controls on the localization of ore-forming magmatism are evident". We find this example unfortunate and its qualitative treatment in his comment is precisely the type of approach that we dispute in our JSG and EOS articles. Several issues are worth discussing from this example. First of all, his 'evidence' is a map of a huge portion of the South American Cordillera in which the faults and ore deposits occur in a region "30 km wide by 1000 km long". If this is Richards' best example we find it ironic that he later criticizes us for the types and scales of maps we used in our studies. The scale and detail on his map are inappropriate for either visual or statistical comparisons of ore deposits and any regional structures and suggest that this example is no different than saying magmatism, ore deposits, and faults occur in orogens. We would certainly agree with this statement, but note that it does not require any genetic relationship between these processes.

Second, many of the 'controlling structures' in his example are lineaments, and we are left wondering about the significance of these features when we do not know what they represent. We note that those on Richards' map do not offset older faults or younger volcanic fields and, thus, have no apparent displacement along them. Our experience with proposed lineaments elsewhere is that they are typically inferred from indirect evidence and may reflect a variety of features including weathering patterns, joint density, metamorphic zonation, rock composition, and, thus, need not necessarily play any role in the ascent and emplacement of magma. If these lineaments are removed from Richards' Figure 1, then the spatial relationship to faults becomes even less clear.

Third, Richards uses this example to propose a relationship between plutons and faults. He does so indirectly by showing porphyry Cu deposits that are associated with plutons, but does not give us any information on the distribution of Eocene–Oligocene plutons that are presumably of a similar age to the faults and lineaments. We, thus, have to conclude that although there may be a relationship between ore-forming fluids and faults, we can say nothing about the relationship between plutons and faults.

Finally, we also had trouble evaluating all of the references Richards uses in support of his statement regarding structural controls on the localization of ore-forming magmatism since about a third of them are not yet published or reside in difficult to access publications. Of the ones that we could actually check, none had statistical analyses and several pointed out exceptions or complications to the exact nature of structural controls on ore deposits (e.g. Cornejo et al., 1997, p. 46; Titley, 1981) or related the location of ore deposits to joints (e.g. Rehrig and Heidrick, 1972). Intuitively, we would expect that low viscosity fluids would preferentially migrate to low stress sites that potentially occur in a wide variety of settings in heterogeneous rocks. However, we need to quantify this belief rather than rely on our intuition.

In the second paragraph Richards makes the statements that "in compressional orogenic settings magma penetration of faults is not favored", and that "Trans-crustal shear zones serve broadly to concentrate fluids (and sometimes magma) flow from depth". We find these two statements to be conflicting (if fault penetration by fluids is not favored then why would faults tend to concentrate fluids near faults?) and do not agree with them with regard to magmatic systems. Our study and studies by others of magmatic and volcanic systems provide quantitative data that directly contradicts the second statement for a range of crustal depths from 0 to 20 km. We also believe that the first statement is not supported by the geologic record nor by a consideration of rates of processes and/or mechanics. Since this issue also arises in papers discussing magmatic systems, we wish to discuss it in some detail but will do so with regard to Point 4 below.

Richards goes on to express four concerns regarding our methodology and presentation and we respond to each of these below.

Point 1. The first issue concerns our example of increasing scale (decreasing the area examined) and whether doing so should increase the number of faults. Here we need to distinguish between regional faults (defined in our studies as those with fault lengths greater than the long axes of plutons) and small faults with little displacement. We agree that the number of small faults may or may not increase with increasing scale but argue that the regional faults may not. Studies of modern fault systems indicate that large displacement regional faults establish a particular fault spacing (e.g. An, 1997), and, although debated, many studies conclude that these large faults take up a majority of the displacement required by relative plate motions (e.g. Humphreys and Weldon, 1994). We further suggest that nature may prefer a limited number of regional faults, since each fault requires additional energy to form and only a few are needed to accommodate the required displacement.

Point 2. This leads to Richards' second concern regarding the size of faults selected. We agree that the choices one makes during any analysis (not just statistical) affects the outcome as well as the types of questions one can and cannot address. But we wish to emphasize a fundamental point. We began our statistical studies with the goal of evaluating whether or not there is a close spatial relationship between regional faults and plutons, since others have used this relationship to argue that these faults play a fundamental role in magma ascent and emplacement. We did so by using the *exact same data* (i.e. the same faults and plutons on the same scale maps) that others have used to argue in favor of a close spatial relationship. Since these data fail to show a close spatial relationship when quantitatively evaluated, we know of *no data* that supports this relationship. Thus, any criticism of our data sets must also be leveled at previous studies, leading us back to our original question of whether there is any data that supports a close spatial relationship between regional faults and plutons. We also reiterate that other research groups, using other statistical techniques, working at different scales, and using different igneous bodies also obtained results incompatible with the idea that regional faults influence the position of igneous bodies.

Even so, it is worthwhile to think about the issue of small faults and magma bodies. If we consider the ascent of large magma bodies (we discuss dikes below), then faults with decreasing lengths and displacements must play a diminishing role in the ascent or emplacement of these bodies. If carried to the extreme case of large magma bodies ascending through crust riddled with many very small fractures with little displacement, we would argue that the only role these fractures would play would be to contribute to the overall rock strength. We also recognize that magma ascent can form local faults, which may play a role in localizing fluid flow around plutons, but cannot be the cause of why the pluton was ascending at that location in the first place.

Point 3. Richards also raises the issue of dikes, which he implies are 'the simplest case'. In fact, we used all reported plutonic bodies in our analyses and made no distinction based on size, shape, and implied ascent or emplacement mechanisms. Again, our initial goal was to evaluate the same data sets used by others. If any plutonic bodies are missing in our analyses then they are also missing in the original studies.

We also disagree with the implication that somehow magma ascent or emplacement by diking is a 'simpler' mechanism. The processes of magma collection and channeling into dikes remains poorly understood, particularly in the lower crust. The growth of large dike-fed chambers is especially problematic (e.g. Miller and Paterson, 1999; Albertz et al., 2000). And finally, detailed field studies of dikes suggest that complex cracking, crystal plasticity, local melting, and/or stoping of host rocks and internal freezing and remelting of dike margins, and dramatic changes of magma viscosity all play a role during dike growth. Most of these processes are incorporated into neither mechanical nor theoretical models of diking.

Point 4. Richards' final concern centers on two related statements that "The ability of magma to penetrate a fault will depend in large part on the prevailing state of stress" and that "extensional faults will be much more readily intruded than faults under horizontal compression". Here again Richards is apparently confusing stress and strain, is basing these statements on intuition, and seems unaware of an extensive literature that contradicts these statements. It is certainly intuitive that the more a rock is squeezed the harder it should be for materials to move through the rock but, to be realistic, we need to consider magma transport in viscoelastic systems. We can illustrate this very simplistically using air bubbles in honey as an analogy (bodies of magma in higher strength host rock). When the honey container is squeezed in one direction, the air bubbles are further squeezed by an increase in hydrostatic stress. Since the bubbles are less dense than the honey, they still want to buoyantly rise no matter how great the compressive stresses.

In natural systems, all rocks below the surface of the earth, including magma bodies, are under large compressive stresses with the only major difference between 'extensional faults' and 'contractional faults' being the orientation of the relatively insignificant principal stresses. If magma or fluid pressures exceed $\sigma_3$ then 'effective stresses' may become tensional across individual surfaces even though the ambient stress field remains highly compressive. Furthermore, any large ascending magma body represents a buoyant, thermally active system in which magma pressure is $>\sigma_3$ and around which thermal stresses alone may be quite large (up to 2–4 kbar, e.g. Marsh, 1982). Thus, during magma ascent, ambient stress fields are probably completely dominated by thermal and buoyancy stresses around magma bodies. In the case of diapirs, regional stress fields probably do not play a significant role during ascent, and in the case of dikes, these stresses may largely influence their orientation.

With regard to the above discussion, we also want to raise the issue of the relative rates of fault motion and magma ascent and what a body of magma 'sees' as it ascends. It is now well established that magma bodies ascend at rates of orders (diapirs) to many orders (dikes) of magnitude faster than long-term fault slip rates (see review by Paterson and Tobisch (1992)). Thus, rising magma bodies will not 'see' fault motions and would only know of the existence of a fault because of a change in local stress fields or rock properties. Moreover, a large ascending diapir must 'process' a huge region of crust, only a small part of which can be in a fault zone. We suggest, therefore, that regional faults will play little to no role in the ascent of diapirs and only influence the orientation of dike ascent via stress fields and rock anisotropy. In regions of extension in which dike emplacement occurs, normal faulting and dike injection are not coupled. The faults only 'interact' with dikes by providing one deformation mechanism by which a long-term reduction in the ambient stress field occurs (e.g. Bursik and Sieh, 1989; Parsons and Thompson, 1991).

Finally, there are many field studies that have documented both dike and diapir emplacement during regional contractional deformation (e.g. see Paterson and Miller (1998) and references therein). Even the world stress map shows that many active modern arcs are under sub-horizontal compression (Zoback, 1992), which is presumably driving contraction at deeper crustal levels. Apparently nature is unaware that it is harder to ascend magma through regions undergoing contraction.

Richards finishes by implying that our statistical approach is misleading and that we are contributing to the "common prejudices against statistics". This is an unfortunate statement. The *interpretation* of statistical results is always open to debate. Our statistical analysis was designed to test a very specific question, whether or not regional faults and plutons have a close spatial relationship. Richards has extrapolated our interpretation of these results to a very different system (ore deposits in fractures or other low stress sites around plutons), and we suggest that it is his extrapolation that is the inappropriate use of our statistical data.

We thus conclude by considering how one might begin to quantitatively evaluate the spatial controls on ore deposits. One might begin by posing two questions that would be best addressed at different scales. First, are ore deposit centers or systems (i.e. temporally and genetically related deposits) preferentially located along regional or secondary faults or in a variety of settings? And, second, within any single ore deposit system, is there a greater percentage or higher grade of ore deposited in a particular structural setting? Both questions would require reasonably complete geologic maps at appropriate scales, coverage of regions that include ore deposits and those that have no deposits, and knowledge of the temporal history of both ore formation and structural history. If such data exists then statistical studies can provide a useful approach to addressing these questions. If such data/maps do not yet exist, then any statements about the spatial and genetic relationships between ore deposits and structures are premature.

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