FORUM

Comment and Reply on "Fault-related rocks: Suggestions for terminology"

COMMENT

C. K. Mawer, *Department of Geology, University of New Brunswick, Fredericton, New Brunswick E3B 5A3, Canada*

In yet another attempt to order fault-rock terminology, Wise et al. (1984) had noble motives, I suppose, but they seemed to be a little unclear about some of the phenomena associated with the development of these rocks, on which their scheme is based.

Strain versus recovery. "Strain is a change in the relative configuration of particles of a material (e.g., Jaeger, 1969, p. 21), and the concept of strain implies continuity of the material in question. As Jaeger pointed out, "if the displacement corresponds to translation and rotation as a rigid body there is no strain." Strain is not "manifest as brittle fracturing causing grain-size reduction" (Wise et al., 1984, p. 391). Strain is the change in shape or size, or both, of a body between specified initial and final stages, and it can be elastic (recoverable) or plastic (nonrecoverable). Which brings us to recovery.

"Recovery" is used in two senses in the materials-science literature. As indicated above, it can mean the return of an elastically strained body to its initial configuration. Recovery, in the sense Wise et al. (1984) perhaps meant, refers to several distinct but interrelated processes, chiefly those of annihilation of crystal lattice dislocations of opposite sign and of ordering of dislocations into stable arrays, so that the deformed crystal reduces its internal strain energy. Recovery involves the formation of low-angle crystallographic boundaries—subgrain boundaries, for example. *Recovery is distinct from, and competes with, recrystallization, though both reduce the internal strain energy of a deformed crystal.* Recrystallization involves the formation of high-angle crystallographic boundaries—grain boundaries, for example—and their migration through volumes of deformed crystal, acting as sinks for dislocations as they progress. It seems, throughout their paper, that Wise et al. (1984) were unclear as to what recovery means or how it is distinct from recrystallization (e.g., the sentence on p. 391 of their paper beginning, "Thus, much of the recovery . . ."). What then are we to make of their fault-rock classification, presented as their Figure 1? What does "rate of recovery" mean? Does it mean "rate of reduction of stored internal strain energy"? Or does it mean "amount or recovery"? Or "amount (or rate) of recrystallization"? Or something else entirely?

Mylonites. At the Penrose Conference on mylonites (report by Tullis et al., 1982), one thing all of us more-or-less agreed about was that *any* absolute grain-size or length/width or other measurement should not appear in the definition of mylonite. To define a hard cut-off value, of whatever sort, is an attempt to introduce false and misleading precision into what must necessarily be a subjective classification. *Any* attempt to subdivide a continuum of deformation effects (especially one as wide and complex as "mylonites") leads to an artificial classification, based on the particular type of material considered and how it was deformed, the classifiers' experience and prejudices and tendency toward slumping or splitting, and so forth. A cut-off value of 50 μm for grain-size reduction is of little use. What about mylonites formed in high-temperature–low deviatoric stress situations, which have recrystallized syntectonically from a coarse crystalline parent, and whose average new grain diameter is greater than 50 μm? And what does "intense . . . grain-size reduction" (Wise et al., 1984, p. 393) mean? How intense? Does a scale of grain-size reduction intensity exist? How is it possible to introduce this without being subjective?

"Mylonite," for better or for worse, has always had and most likely always will have a genetic connotation, from Lapworth's time up to the present day. Mylonites are associated with ductile fault zones; if not, *the rock in question is not a mylonite,* but a schist, or phyllite, or something else (see Williams et al., 1982, especially Chapters 16 and 18). This means that to call a rock a mylonite, complete description of it and its occurrence at all scales is essential. A mylonite will occur in a restricted, long but narrow zone with displacement of the rocks on one side relative to those on the other, and will possess certain distinctive textural characteristics or features, at mesoscopic and microscopic scales (see, e.g., Simpson and Schmid, 1983).

Faults, etc. The same objections to absolute measurements in the definition of mylonite hold for Wise et al.'s (1984) definition of "fault." And I am sure that very few structural geologists would agree to class strongly attenuated fold limbs as faults, on the basis of an argument such as "some highly attenuated folds ultimately must have displacements transitional into faults" (Wise et al., 1984, p. 393).

Strain and recovery history. The comments made above with regard to Wise et al.'s 1984 Figure 1 apply to their Figure 2. How does one specify "rate of strain" and "rate of recovery" (whatever it is)? In any case, whether rocks behave in a brittle or ductile fashion depends on more environmental factors than those two—factors such as confining pressure, temperature, activity of various chemical components, and so on.

Summary. The use of foliated vs. nonfoliated and porphyroclast: matrix ratios as relatively simple field criteria for distinguishing fault-related rocks may be adequate for a start (though potentially quite misleading; see Williams et al., 1982, p. 506–508), but I stress the necessity of determining the rock's deformation context—i.e., in a fault zone, be it predominantly brittle or predominantly ductile. Application of the various fault-rock names ultimately relies on microscopic study combined with careful fieldwork, as Wise et al. (1984) indeed mentioned. The rocks *must* be described carefully and completely, at all scales of observation. This is the basis of any good petrography and any interpretations derived from it, and it seems to be in great danger of being neglected while we battle through the semantic jungle of fault-rock terminology.

REPLY

D. U. Wise, *Department of Geology and Geography, University of Massachusetts, Amherst, Massachusetts 01003*
D. E. Dunn, *Department of Earth Sciences, University of New Orleans, New Orleans, Louisiana 70148*
J. T. Engelder, *Lamont-Doherty Geological Observatory, Palisades, New York 10946*
P. A. Geiser, *Department of Geology, University of Connecticut, Storrs, Connecticut 06268*
R. D. Hatcher, *Department of Geology, University of South Carolina, Columbia, South Carolina 29208*
S. A. Kish, A. L. Odom, *Department of Geology, Florida State University, Tallahassee, Florida 32306*
S. Schamel, *Earth Sciences and Resources Institute, University of South Carolina, Columbia, South Carolina 29208*

The heart of Mawer's objections to our paper (Wise et al., 1984) and the part of his discussion with which we most vehemently disagree is his and many others' attempts to force a debatable genetic definition of mylonite (i.e., it must be fault-produced) on a rock type that is commonly *but not always* produced in this way. For example: (1) Wiener (1983) described a fold axial planar origin for a 6-km-wide zone in the Adirondacks of mylonites and protomylonites in which there is minimal fault displacement. (2) Waters and Krauskopf (1941) described protomylonitic rocks of the border zone of the Colville batholith, produced more or less as part of the batholithic intrusion process. (3) Coward (1976) described mylonitic-associated shear zones from Botswana and from Scotland with the zones characterized more by flattening under pure shear rather than deforming by the simple shear of typical fault motion. All of these examples are shear zones; the argument hinges on when a shear zone must be called a fault.

If Mawer's genetic-based definition of mylonite is accepted, then any ductile shear zones containing that rock type, even those cited above, would have to be called faults, with all the engineering and environmental consequences attached thereto. In our paper we tried to define a rock type, mylonite, without requiring the questionable genetic connotation of production in a fault. Thus, if mylonites do not absolutely require a fault origin, the classification system must also define when a ductile shear zone should be called a fault, a second error that Mawer thinks we committed.

In our defense, we point to Carey's well-known illustration of Rheid folds showing extreme limb attenuation along the flow planes. When the ratio of displacement to width across such ductile shear zones reaches 100:1 or 1000:1 we believe most structural geologists would call the zone a fault, even if Mawer would not. To our way of thinking, a 1-cm-wide zone with 1 m of displacement is a ductile fault and should be so designated!

Mawer also argues that any attempt to quantify or place limits on a continuum results in an artificial and unnatural classification. He proposes, as an extreme example of the mylonite continuum, those coarse-grained "mylonites" formed from coarse-grained parents by syntectonic recrystallization at high temperature and low deviatoric stress. These "mylonites" seem to us like common, medium-grained metamorphic rocks; why not call them by their ordinary rock names? The argument that a continuum should not be subdivided on any quantitative basis is a prescription for inexactness and misunderstanding, a reasonable description of the field of mylonite classification today. One wonders where petrologists would be today if they were forbidden to place quantitative values on the An content of plagioclases just because they represent a continuum.

Mawer does make a valid point in arguing that our distinctions between recovery and recrystallization are not sufficiently clear. His paragraph emphasizing the distinctions is a very good one, and we agree that the *x* axis of our figures would be less ambiguous (but more cluttered) if marked "rate of reduction of stored internal strain energy" rather than merely "recovery."

Mawer also raises the argument that different rocks behave in differing ways under various conditions of pressure, temperature, etc., thus rendering uncertain just where and how our Figures 1 and 2 are applicable. One can hardly disagree that rocks have a variety of behavior mechanisms. The figures were designed to illustrate a possible system of classification which ultimately might be quantified for differing rock types and conditions. That is why these figures have no scales on their coordinate axes.

Mawer also charges us with ignorance of the proper definition of strain, using the argument that strain no longer occurs once brittle failure occurs. As long as one's world is thin-section scale, such a definition might apply. However, much larger rock masses can deform continuously without overall loss of cohesion even though much of the deformation is by local brittle mechanisms. For example, Ramsay and Huber (1983, p. 254–255) illustrated the reassembly of rock masses that have been fractured and brecciated. Ramsay and Huber, apparently ignorant of such fine definitional distinctions, proceeded to draw strain ellipsoids for these fractured masses and to calculate their strain parameters. We contend that strain can be a scale-dependent word and that we are not in the worst of company in misusing it with respect to Mawer's interpretation.

In summary, we believe that even though Mawer may not have intended it as such, his discussion makes a very significant contribution to the mylonite controversy. It illustrates the kinds of arguments that could permit the participants in the Penrose Conference on mylonites to be unable to agree on a definition of mylonite. Mawer's discussion seems to mix (1) a questionable genetic requirement that all mylonites be formed in fault zones, with (2) an overly rigid, scale-insensitive definition of strain, with (3) a claim that a continuum should never be quantitatively subdivided. We obviously disagree on all three points.

If readers of our paper were uncertain previously of the need for some generally acceptable, simplified set of definitions and classification for fault-related rocks, Mawer's discussion might serve as a prime example of the differences in philosophy causing the legalistic and definitional morass from which the classification attempts to climb.

COMBINED REFERENCES CITED

Coward, M.P., 1976, Strain within ductile shear zones: Tectonophysics, v. 34, p. 181–197.
Jaeger, J.C., 1969, Elasticity, fracture and flow with engineering and geological applications: London, Methuen and Co., 268 p.
Ramsay, J.G., and Huber, M.I., 1983, The techniques of modern structural geology: New York, Academic Press, 307 p.
Simpson, C., and Schmid, S.M., 1983, An evaluation of criteria to deduce the sense of movement in sheared rocks: Geological Society of America Bulletin, v. 94, p. 1281–1288.
Tullis, J., Snoke, A.W., and Todd, V.R., 1982, Penrose Conference report: Significance and petrogenesis of mylonitic rocks: Geology, v. 10, p. 227–230.
Waters, A.C., and Krauskopf, K.B., 1941, Protoclastic border of the Colville batholith: Geological Society of America Bulletin, v. 52, p. 1355–1418.
Wiener, R.W., 1983, Adirondack Highlands–Northwest Lowlands "boundary": A multiply folded intrusive contact with fold-associated mylonitization: Geological Society of America Bulletin, v. 94, p. 1081–1108.
Williams, H., Turner, F.J., and Gilbert, C.M., 1982, Petrography: An introduction to the study of rocks in thin sections (second edition): San Francisco, W.H. Freeman and Co., 626 p.
Wise, D.U., Dunn, D.E., Engelder, J.T., Geiser, P.A., Hatcher, R.D., Kish, S.A., Odom, A.L., and Schamel, S., 1984, Fault-related rocks: Suggestions for terminology: Geology, v. 12, p. 391–394.