SUMMARY OF DISCUSSION ON PAPERS CONCERNED WITH FAULT GEOMETRY, ESPECIALLY INVOLVING OBSERVATIONS IN THE UPPER OR BRITTLE PORTION OF THE CRUST

By Art McGarr

Generally, papers in this conference were presented in order of the depth at which the observations were made with the first day being devoted largely to papers concerned with analyses of faults in the upper 10 km, or so. The first scientific controversy, probably the result of resentment associated with this ordering, came to a head when some of the Precambrian geologists suggested that surficial evidence of faulting may have little relevance to what goes on at depth, the implication being that deep-seated processes are all-important. The people who studied shear zones in Precambrian terrains seemed to feel that the conference should have worked its way from the bottom upward. Shear zones in the lower crust, it was suggested, are much simpler than the very complex rupture patterns described by Bonilla in his comprehensive survey of observations of historic surface faulting.

The Precambrian geologists' case foundered on both practical and scientific grounds. There was no argument about the relevance of surface fault patterns to possible damage to engineering structures. This point was reinforced during the field trip following the conference when we observed damage to the foundation of a house due to surface cracks associated with the recent earthquake near Landers.

Regarding the scientific relevance of surface rupture patterns, Wallace, Bonilla and others argued that the large scale of many of the complications in fault geometry was good evidence for their downward continuation to substantial depths. Aftershock hypocentral distributions, presented by Hanks and Bonilla, tend to support this conclusion. Unfortunately, there is little direct evidence about the nature of the continuation of faults in surficial sediments downward into harder rock. Bonilla mentioned one such comparison for the 1930 North Zu, Japan, earthquake for which fauting was observed at the surface and in a railroad tunnel at a depth of 160 m; the horizontal ccmponent of slip in the tunnel was more than twice that observed at the surface.

Ramsey pointed out some mechanical difficulties associated with extrapolating some of the complex surface patterns of faulting, described by Bonilla, to depth. From the following discussion and from some of the other papers, however, there seem to be several ways out of the difficulties. First, the time sequence of the faulting is not known. For example, Sibson mentioned that one of the scarps observed following the 1970 Gediz, Turkey, earthquake may have increased in height after the main shock and Bonilla suggested that the scarp might have been associated with a large aftershock two hours later.

A second way out of the mechanical difficulties of complex surface patterns is connected with the observations presented by Wallace and Morris to the effect that faults show a considerable variation in orientation with depth. From their evidence it is clear that dips, for example, measured at the surface cannot be extrapolated with any certainty to substantial depths. Thus, there is little point in being concerned about mechanical difficulties at depth based only on surface evidence.

On a more fundamental level Ramsey attacked E. M. Anderson's theory of faulting by noting that faults are rarely, if ever, observed to occur in conjugate sets. In a given zone faults tend to be of one sense only. Although this point was accepted by most of the conference participants, there was the feeling that perhaps this was not a very profound problem. Once a major fault forms the stress analysis of Anderson's theory breaks down because the stress field to cause further faulting is no longer homogeneous. Thus, to pursue the geometrical implications of synchronous conjugate shear failure, as Ramsey did, seemed like a pointless exercise.

En echelon patterns of fault strands received considerable attention because they are such a common feature of fault zones on scales up to 10 km, or so. On a small scale Sharp showed how strike-slip surface crack patterns simplify with depth. En echelon offsets at the surface converge downward into a simpler throughgoing fault. The mechanism of the downward simplification is related to the fact that most earthquakes are the result of motion on pre-existing faults that serve to channel the shear failure and inhibit en echelon offsets and other complexities. Near the surface, however, the sediments are mechanically homogeneous either because they were depositied since the previous faulting episode or because of healing due to weathering processes. The complexity, then, is the result of fracture occurring in previously intact material. This situation is surprisingly similar to one described by McGarr et al. involving shear zones induced by mining gold. In both cases the fractures break previously intact sediment and form highly similar patterns of en echelon segments.

The most interesting feature of those small-scale en echelon segments, whether in surficial sediments or in the strong brittle quartzites deep in the Witwatersrand gold mines, is their consistent sense of offset. In Sharp's terminology, the offset is invariably of the opposite sense; that is, for right lateral motion the fault steps to the left and vice versa.

On large scales, greater than the depth of surficial weathered sediments, en echelon offsets of both senses occur with apparently equal frequency. The limiting size of the offset fault strands may be related to the thickness of the brittle layer of the crust, at least in the vicinity of the San Andreas fault according to Wallace, who drew a diagram showing the size distribution of fault strands.

On the general theme of fault zone complexity, the central California "creeping" section of the San Andreas presents a relatively simple appearance compared to the fault system in southern and northern California according to the seismicity data presented by Hanks. In central California the seismicity is largely close to well-established fault planes rather than being very regionally distributed as it is to the north and south. With the apparent exception of central California, however, most of the seismicity evidence argues for considerable complexity. In particular, precise aftershock locations generally suggest strain release distributed throughout a volume. Hanks also made the unorthodox point that aftershock distributions may grossly overestimate the source dimensions.

One of the more important topics of the conference was fault zone width and its dependence on depth and displacement. Most of the evidence presented here appeared to favor an increase in the width with depth, or at least a constant width. Wallace noted that in central California the San Andreas fault zone width, as measured at the surface, is about 2 km for a total displacement of 200 km, or so. Sharp said that in old terrains fault zones are wider and consist of more strands than in young sediments. Seismicity data for California reviewed by Hanks is generally consistent with broad fault systems at depth rather than narrow planes. McGarr et al. presented evidence suggesting that shear zones increase in width with increasing confining pressure or, equivalently, depth. One implication of strain softening limits leading to flat-topped strain profiles across deep shear zones, discussed by Ramsey, is that these zones must broaden with progressive shear deformation into very wide zones. In the Mina region of Nevada, and probably throughout the Great Basin the brittle near-surface faults ramify downward into broader shear zones, according to Speed. The primary evidence for this downward broadening is the distribution of earthquake foci and the effect is particularly marked in the western Mina region where the orientation of west-striking faults appears to be controlled by a strong pre-existing anisotropy. Here the shear deformation at depth is broadly distributed to the extent that it is not possible for strain energy to accumulate to the extent necessary for a large earthquake.

Another interesting aspect of the western Mina area which came up in the discussion is the fact that the T axis of the composite fault plane solution does not coincide with the extension direction inferred by Speed from other evidence. This significant lack of agreement presumably results from the strong control on fault orientation due to the anisotropy generated during a plate collision in pre-Cretaceous times.

Fault gouge and other products of shear zones did not receive a lot of discussion during the first day but a few of the more interesting points should be mentioned. First, shear zones that form in the deep crust differ chemically from the wall rock. They tend to be relatively rich in H_2O and K feldspar according to Ramsey. Bridgewater mentioned that adjacent shear zones may show different chemistries and Watterson elaborated on this by remarking that even small zones not connected to the "general plumbing" show such differences.

On a shallower level (depthwise, that is) Wallace and Morris emphasized the weakness of clay gouges observed in mines. This observation presents a problem in trying to explain mechanisms for fault zone growth. Morris' description of fault gouge that formed at a depth of 18 km in Utah drew a surprised reaction from the conference participants.

Summary of Discussion on Kohlstedt's, Weathers', and Etheridge's Papers
by David Kohlstedt

All three papers emphasized the marked correlations between (1) differential stress and dislocation density and (2) differential stress and grain size observed in laboratory high-temperature, plastic deformation experiments. Each applied these paleopiezometers to a specific fault zone to estimate the stress level during plastic deformation.

The discussion emphasized questions related to applying these laboratory derived relations to problems involving geologic times (Bridgewater), Several people were concerned about problems of overprinting during a secondary deformation event or of erasure during a late static annealing event. Also, Etheridge raised the point that water, which is believed to weaken many minerals, may change the stress - grain size relation,

The following responses were made: (1) The dislocation density and grain size in stready state are established in response to forces (stresses), only the kinetics of reaching steady state should be influenced by going to geologic strain rates and temperatures; ie. there should be no problem in going from laboratgry to geologic times. The good agreement between the stress estimated from dislocation density and that estimated from grain size in many olivine rich rocks supports this argument, (2) To sort out secondary events, it is often useful to know the geologic setting in detail (see the systematic behavior observed in the Moine thrust zone discussed by Kohlstedt). Often two sequencial events can be recognized because the kinetics of recrystallization are much more sluggish that those of dislocation motion. In fact, two events can often be clearly identified. This problem of unraveling complex deformation events will be easier to approach once systematic stress change and static anneal experiments are in hand. (3) The role of water on recrystallized grain size is apparently up for grabs. Etheridege used field observations to argue that water has a major effect on the stress - grain size relation. Kohlstedt suggested that water only influences the kinetics of the process. If water is to affect the steady state grain size, it must have a major influence on the grain boundary energy. Again, this area is ripe for the experimentalists.

It is important to point out here that the reported stress - microstructure relations are based on very few experiments. While the basic trends seem quite Clear and are supported by an extensive materials science literature, additional experimental work is essential. Numerous questions can be raised as to the importance of elemental and second phase impurities, particularly on the steady state grain size, and concerning the microstructures developed during complex deformation events.

Yet, it is exciting to note that several field geologist expressed interest in applying this new technique to evaluate the stress levels during faulting in a variety of fault zones.

Comments on Talks by Watterson, Sibson, and Logan

by T. Engelder

The Tuesday afternoon and evening sessions consisted of three papers with different themes: Watterson on depth crustal shear zones; Sibson on the similarities and differences between the Outer Hebrides and Alpine fault zones; and Logan on experimentally generated shear zones.

Watterson's main message concerning the deeply eroded terrain of Greenland was that in Precambrian basement the rocks with the simplest structures are those that are most highly deformed. These highly deformed shear zones formed at depths of 20 km or more are not always symmetrical. Some shear zones have sharp boundaries with less deformed rock. Structural changes within the shear zones include the re-orientation and flattening of structures causing the structures to appear streaked out. The tectonic setting of the Precambrian shear zones in Greenland were compared with Asian tectonics of today.

Major discussion of Watterson's paper included questions concerning the distribution of shear strain profiles across a shear zone where $\gamma > 6$ is difficult to measure. Models for variation in strain rate and stress across the shear zone seems open to question. Other questions concerned the mechanisms of deformation in the shear zones.

Sibson's treatment of the Outer Hebrides thrust showed several rock types including zones of pseudotachylite, ultracataclasite, phyllonite and mylonite. Many of these same lithologies can be found along the Alpine fault in New Zealand. When contrasting the fault zones Sibson pointed out that pseudotachylite pervaded the Alpine fault zone whereas it is restricted to the lower part of the Outer Hebrides thrust which may have been dry at the time of faulting. Likewise the mylonites formed under dry conditions along the Alpine fault whereas water played an important role in the formation of the mylonites along the Outer Hebrides thrust.

Major discussion centered on the role of thermal shock in forming pseudotachylites versus shear strain melting. Other questions concerned the significance of the metamorphic zonation next to the Alpine fault and its relation to shear strain heating along the fault zone.

Logan's discussion focused on the frictional data generated in laboratory analogues of fault zones. His lab experiments include studies of the frictional properties of gouges with various mineralogies. Both monominerals and bimineralic gouges have been tested. Of the various petrographic studies one of the most fruitful appears to be calcite gouges deformed in a suite of experiments through the brittle-ductile transition for calcite. In the ductile field calcite gouges resemble natural shear zones of the type that Ramsey and Watterson describe.

Discussion of Logan's paper focused on the mechanisms for stick-slip sliding within some shear zones. Another question concerned the possible correlation of shear strain within the calcite gouge with actual slip measured during the experiment.

U.S.G.S. CONFERENCE ON FAUL ZONE GEOLOGY

PALM SPRINGS, APRIL 2-5, 1979

SUMMARY OF DISCUSSION, PAPERS 20 to 23 - M. A. ETHERIDGE

20.) "Chemistry and Mineralogy of deep-seated movement zones (Nagssugtoquidian belt) in Greenland and interpretation of fluid environment." D. BRIDGEWATER.

This paper described the chemical changes accompanying deformation in some deep-seated shear zones, and showed how they may be used to constrain the physical and chemical properties of the fluid phase introduced during deformation. In this case, the fluid must have been acidic and strongly oxidizing, and probably was of high salinity. The most surprising aspect of the chemical data was the lack of evidence for substantial hydration, and the author speculated that the fluid may have been CO_2-rich, and may thus have been largely mantle derived. In the face of the geochemical evidence, the assembled body of structural geologists and geophysicists maintained a largely respectful (ignorant?) silence!! However, there was some discussion (Lambert, Watterson, Etheridge) on the origin of the fluid phase, with the possibilities of a dehydrating lower crust, and downward circulating fluids being specifically mentioned.

21.) "Field evidence for fluid flow around faults : evidence for dilatency pumping." R. H. SIBSON.

Rick Sibson presented two pieces of evidence in favour of "dilatency pumping" associated with faulting; 1) the water output in streams around the White Wolf fault following a significant earthquake, and 2) a variety of dilational structures associated with faults in very young sediments in S.E. Iran. Terry Engelder pointed out that the amount of water estimated from the White Wolf outflow is only a very small fraction of the expected porosity in a reasonable "dilated" volume around the fault, to which Rick replied that he would expect that only a relatively small rock volume around the fault would contribute to the surface outflow. The Iranian structures are very impressive, and no suitable alternative explanation of their origin was produced.

22.) "Deformation and fusion of two fault plane rocks in relation to their depth of formation : pseudotachylite of the Silvretta nappe (Alps) and hyalomylonite of Langtang (Himalaya)." L. MASCH.

The concept of very shallow melting at the base of huge landslides is facinating, and adds a new dimension to the interpretation of deeper level pseudotachylites. It truly represents the high strainrate, low P-T end of the fault zone regime that extends to the highest metamorphic grades. There was very little discussion, apart from expressions of amazement, although mention was made of two possibly related occurrences of glassy material, 1) on the margins of caldera collapse structures (Sibson), and 2) on the margins of breccia pipes (Morris). However, it was agreed that significant non-frictional heat sources could have contributed to melting in both these examples.

23.) "Metamorphism, argon deplention, heat flow and stress on the Alpine fault (New Zealand)!' C. SCHOLZ, J. BEAVAN and T. HANKS.

This paper pointed out very clearly the thermal implications of high shear stresses on major fault zones. This was achieved in a reverse fashion by arguing that the metamorphic zonation on the S.E. side of the Alpine fault is a result of shear heating during Mesozoic movement, and calculating the shear stress necessary to produce the observed thermal effects. Shear stresses of the order of 100 MPa are required. It was also suggested that argon depletion in the wall rocks resulted from shear heating during the most recent movement episode (5 m.y. to present). Rick Sibson cast considerable doubt on 1) the geological evidence for two discrete movement episodes, and therefore for substantial Mesozoic movement, as well as 2) the precise geometric relationship between metamorphic isograds and the fault plane. There was also some discussion of the argon diffusion data used to model the most recent movement (Evernden, Kohlstedt). Despite these objections, there was general agreement on the necessity for more careful evaluation of possible thermal effects around other large fault zones.

In addition to these formal contributions, John Ramsay showed a series of spectacular photomicrographs of antitaxial fibrous veins in low grade metamorphic rocks. There is a very obvious tendency for vein minerals to be precipitated on like phases in the vein wall, and it was pointed out that this has some potential implications for classical pressure solution theory.

Comments by John M. Logan

I have left this meeting with a feeling that the work of the competent people assembled here have served to point out how little we know about the physical and mechanical properties of fault zones. I believe that the views presented reflect the high quality of the workers and yet I left reflecting that even with this, the basic ideas of fault zone geometry are in doubt and dispute (Ramsey, Watterson, Wallace and Morris). More advanced problems of effective active fault zone widths (the sites of displacements), permeabilities, and even compositions appear much more tenuous or without any hard data. The other facet that I found disturbing is the reluctance to talk about the mechanical behavior of fault zones and fault-zone materials, among field geologists. This is puzzling, for to me it seems that this is the goal of those studies, for without an understanding of the mechanical response of fault zones, no predictions of behavior can be attempted nor any extrapolation from one locality to another can be made. It is in this area that theoretical and laboratory studies should be particularly helpful and illuminating, for the mechanical properties determined here must place as much of a contraint upon the interpretation of behavior as the geometry does. It is also clear that the information provided by seismic techniques (McGarr, Hanks) and other indications of failure within the crust must be taken into account when considering the mechanical behavior. One of my concerns is that the conference is too "in house," that is, one of the intentions of the meeting is to demonstrate a need for active field investigations of fault zones and yet all of the people at the conference came with this belief already in hand. So that in effect, one of the purposes of the conference can only be served by the Proceedings; that is to demonstrate to others that an active program of investigation into fault zone properties has to be undertaken. Hopefully, the papers given here will set the state of knowledge (or ignorance) and provide some guidelines as to what must be done, but without any subsequent research being generated on this problem, I would consider the conference to be a failure. This area seems to be so critical to the earthquake prediction program that it is embarrassing that so little has been done to date. Hopefully, the Proceedings will stimulate further research in this area.