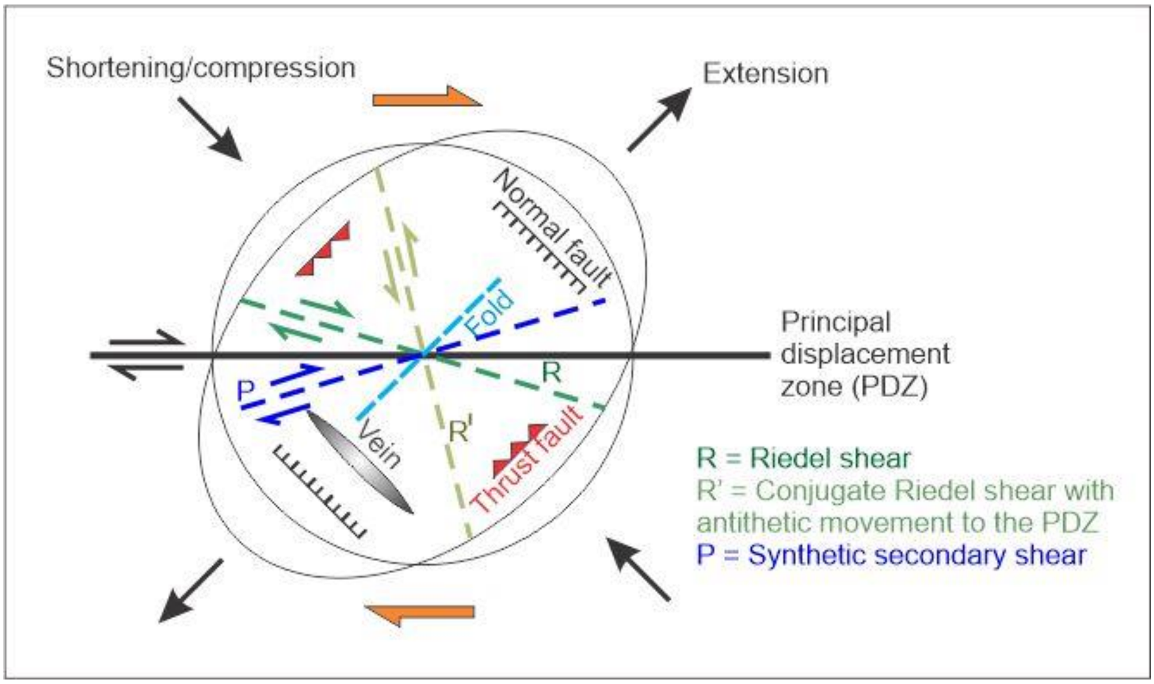
WHY I DISLIKE RIEDEL SHEARS

Riedel shears. A lot of geologists love them. Me, I have grown, possibly unfairly, to dislike them – but we'll get to that.

Background

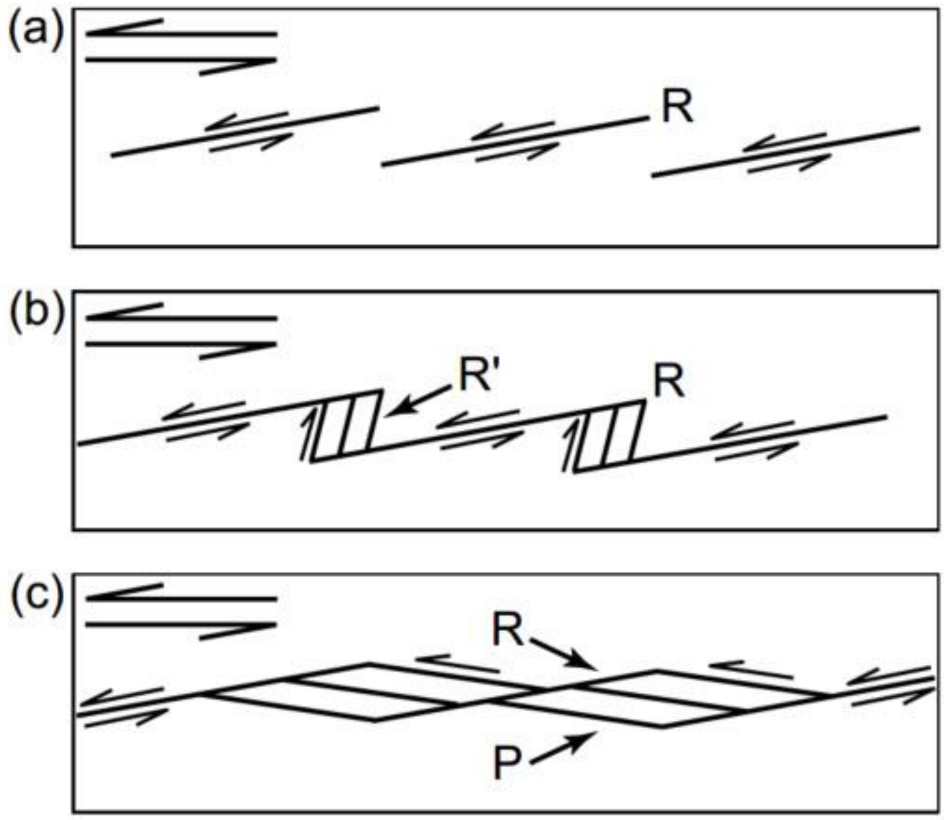
Most geologists have heard of Riedel structures, even if they aren't familiar with the workers who defined them. They are named for W Riedel, based on his 1929 paper and first reported by Cloos (1928). They appear in most of the widely distributed structural geology textbooks with an oft-repeated diagram showing the variously oriented structures, similar to the one I have drafted below.



I have heard some people call these diagrams Tchalenko diagrams, but I disagree with this because, although Tchalenko (1968) used the terminology, he never cited Riedel (1929). For me, Riedel shears are a well-documented phenomena associated with strike-slip faults developed in relatively homogeneous rock-types and typically (but not always) in a cover sequence over a transcurrent basement structure. There are exceptions and modifications to this general view, mainly as a function of the orientation of the principal stress axes relative to the principal displacement zone that has developed in the same deformation regime. However, the general description is adequate for the thesis of this post. This general view is consistent with the good correlation of structures developed in the original modeling of features developed in clay layers over a sheared substrate with those seen in nature.

There are characteristic angular relationships and kinematics associated with the structures that develop over time and under a fairly consistently oriented stress regime (diagram above). The basic geometry of the Riedel structure consists of conjugate shear bands arranged in en-echelon arrays and denoted by R and R'. The other important structure is the P surface. The sequential development of shear surfaces in natural Riedel systems is poorly understood, but the general consensus is that R and P shears develop before the R' shears, which form at a much higher angle to the principal displacement zone. Just as importantly, individual shears generally remain active after the other shear types develop. This means that, as the shear zone evolves, the strain within the Riedel system is accommodated by synchronous movement on all fractures comprising the zone.

There is recognition that the Riedel shear systems evolve from Proto-Riedel Zones (terminology from Ahlgren, 2001), and a recent LinkedIn post by Applied Stratigraphix showed a good example of these. The diagram below is from Ahlgren (2001) and shows the sequential development of a Riedel system developing in a sinistral strike-slip regime.



Where's the problem?

Now here's the rub. Perhaps wrongly, I have grown to dislike Riedel shears. More correctly, I have grown to dislike *the mention* of Riedel shears. Although they are part of a great model of high-crustal level deformation processes, they are misused. In my work I get to review a lot of company reports and documents. Very commonly, in zones of rock in which faults are mapped or inferred, a model of Riedel shear development is invoked by company geologists and consultants.

So, where is the problem? In most cases, the models of Riedel shear systems have been invoked because someone has thought the fault geometries are consistent with the Riedel model. However, the thinking process seems to stop there. Many of the faulted volumes I see 'described' have been affected by more than one deformation event, commonly with development of foliations at high angles to each other. So, the stress axis orientations relative to the oldest fault populations can have changed markedly. This, in turn, means that the kinematic history on any one structure can be complex. The old chestnut of fault reactivation throws the use of Riedel shear models to explain fault geometries into doubt. Why? Because I very rarely see a geological history provided for the area described in reports. That is, overprinting histories that define whether progressive, sequential or coeval structure development occurred are absent. And the movement senses on the structures have not been resolved such that the kinematic histories can be integrated with geological history.

I visit mining and exploration projects in rocks of all vintages, from Archaean to Proterozoic to Palaeozoic to relatively recent. These rocks have had a lot of time to enjoy a lot of deformation, commonly with markedly varying orientations of far-field stresses over time. However, the Riedel template is commonly employed with no regard to structural history. This has major implications for understanding the structural controls to volumes of fault-controlled hydrothermal mineralisation, especially if people are interpreting fault mosaics as Riedel systems. Fault populations have to be interrogated and characterised, with their overprinting histories and kinematics commonly being more important than their orientations with respect to each other. Furthermore, many of these 'models' are applied irrespective of the lithologies in which they are developed. The distribution of lithologies plays a huge role in the way strain is distributed, with zones of shortening strain, shearing strain and strain accumulation all being strongly influenced by rock-type and variations in rock-type. So, to blithely interpret linear fault geometries across markedly different lithological packages and then ascribe it to a Riedel system shows a lack of understanding and/or interest. Either way, it is doing the rocks a disservice.

If people are honest with themselves, I suspect there are many deposits out there that are touted as forming in a Riedel system that have been totally misinterpreted. Worse still, there is one consultant (who will remain nameless) who thinks every system interpreted from geophysical data is a Riedel system, which compels this consultant to invoke formation of every structure during the same orogenic event with the same stress axes, regardless of established geological history. I suspect the consultant in question is not alone. The few breaths of fresh air I get are when I see reports by colleagues such as David Rhys, Molly Turko, James Lally, James Siddorn etc, because I know they will have done the structural history justice and resolved the kinematics, instead of simply throwing a structural geological frisbee with set angular orientations over a map and instantly proclaiming it is a Riedel system.

Being true to the rocks – concluding comments

As geologists working on hydrothermal mineral deposits, we should be dealing with understanding processes over time. We should be resolving changes in kinematics, identifying structure reactivation, understanding the controls on permeability, documenting overprinting and geometric relationships, undertaking vein system analysis, integrating alteration and fluid flow events etc in a coherent history. All of these are fundamental to the tenets of mineral system analysis. Simply ascribing a geometry to a Riedel system based on orientations and angular relations and moving on is not.

Anyway, this is my soapbox based on the frustration of seeing the poor or incorrect application of a very good structural geological concept. Unfortunately, it has made me dislike them, or, more correctly, the mention of them, and that should not be the case. Enough said!

References

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