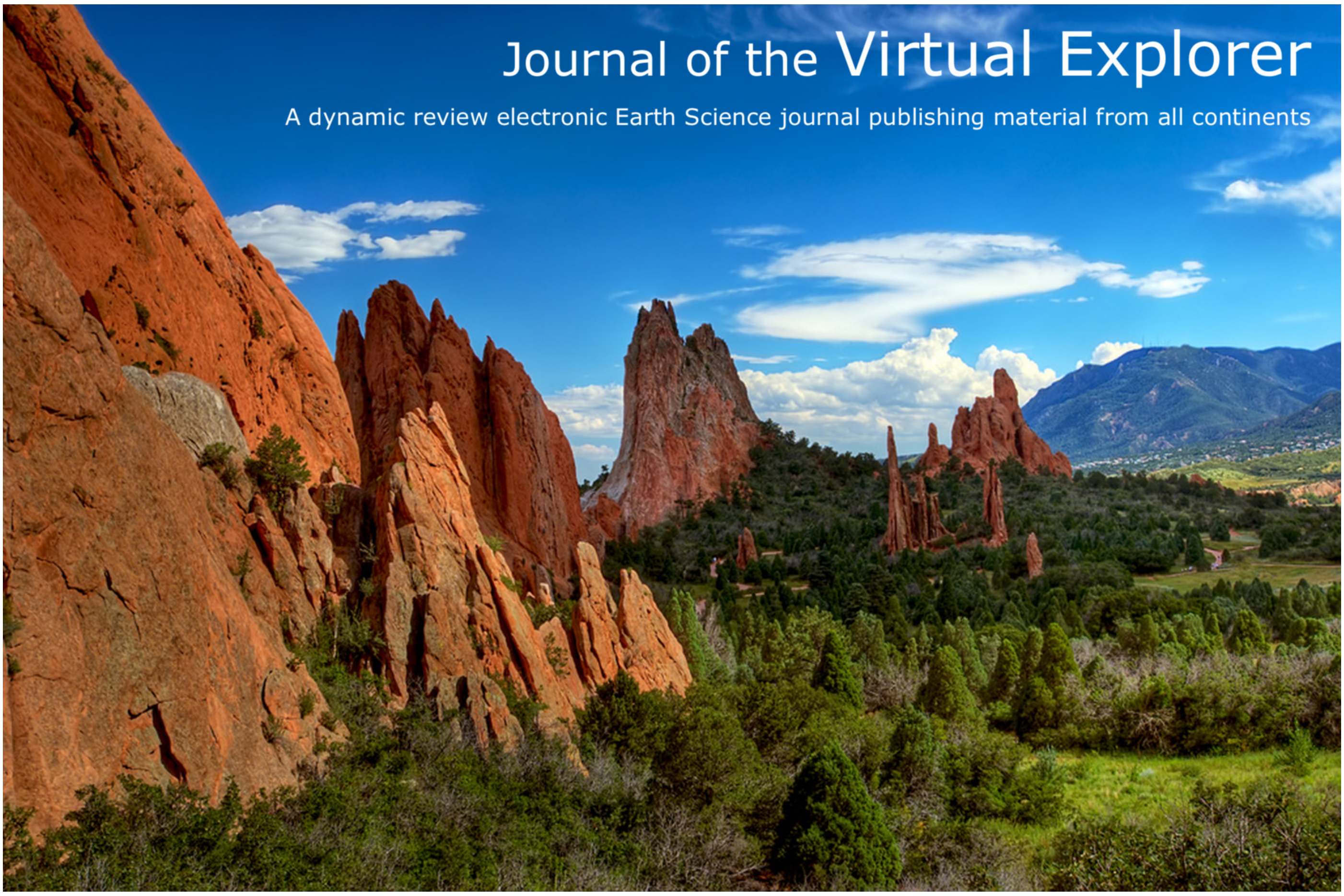


# Retrospective on "Sense of Shear" in relation to deciphering Metamorphic Core Complexes

*George H. Davis*

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**Abstract**: *"Making sense of shear"* was an important part of the challenge and opportunity presented by recognition of metamorphic core complexes in the mid-1970s. I have found it useful - admittedly with the inherent limitations of partial recall - to think back 3 decades later on the impediments and challenges experienced in describing and interpreting metamorphic core complexes, and how these hurdles were cleared. The scientific frenzy that accompanied mapping, analysis, and interpretation of metamorphic core complexes both advanced and benefited from meaningful new insights regarding shear zone deformation, fault rocks, deformation mechanisms, and sense-of-shear criteria. Structural geologists and tectonists who carried out the discovery-mapping of metamorphic core complexes in the Western Cordillera of the North America elevated extensional tectonics as a formidable mechanism for producing complex mountain systems, doing so in the face of significant resistance. They contributed to the advancements in part through innovative field work and structural geologic mapping of complex regional systems, but also through bringing colleagues together in field and conference settings to puzzle over unresolved questions, many of which revolved around interpretation of fabrics and timing of deformation. A paradigm shift in managing fault-rock taxonomies and demystifying sense-of-shear determinations based on 'reading' fine-scale fabrics *in the field* came importantly from geoscientists working and/or trained outside of the United States, who collectively pioneered a reformed look at fault rocks, deformation mechanisms, and sense-of-shear criteria. U.S. structural geologists and tectonists working on metamorphic core complexes benefited 'instantly' from the cross-talk. The immediacy of impact across the structure-tectonics community is historic as related to research, teaching, and PhD training. Altogether it is a perfect example of the transformative power of grassroots interdisciplinary research across boundaries.

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## Setting the Stage

This volume on “sense-of-shear” causes me to look back on the discovery and interpretation of perhaps the most impressive category of shear zones exposed today on the surface of the globe, namely the brittle-ductile shear zones for which metamorphic core complexes in the Basin and Range Province of the American Southwest owe their existence (Crittenden, Coney, and Davis, 1980). The impressiveness of this brand of shear zones is not simply a function of thickness (up to at least 4 km), or trace length (up to at least 125 km of strike length in exposed parts of in individual core complexes), or regional extensiveness (~30 core complexes in the Basin and Range), or bold clarity of exposure in a vast semi-arid desert region. Instead it derives largely from (1) the stark contrasts between rocks, structures, and fabrics in upper versus lower ‘plates,’ and (2) the telescoped structural gradients at and immediately beneath the detachment faults, which separate upper from lower ‘plates.’

It is becoming more and more uncommon for major discoveries in geology to emerge strictly from fieldwork, but I have heard it argued that the discovery and elucidation of metamorphic core complexes by the structure-tectonics cohorts working in the Western Cordillera is one of the compelling examples. In a brief period of time in the mid-1970s, more than 40 metamorphic core complex terranes were identified in the North American Cordillera, and the fundamental structural characteristics were identified (Crittenden, Coney, and Davis, 1980). Steps to discovery included field rendezvous of structural geologists and tectonists with graduate students meeting-up in the field at locales marked by an arrangement of fabrics, structures, and contact relationships that taken together, at the time, seemed unique in the world, and were unaccounted for in the structure-tectonics literature. Debates and discussions were loud, strong, sustained, and enjoyable.

Agreement on the basic physical and geometric characteristics associated with metamorphic core complexes proved easier than interpreting them. Thinking back on how challenging it was for the structure-tectonics community to interpret the tectonic significance of “metamorphic core complexes,” I imagine that some would say that the main hurdle was the illusiveness of the timing of ‘what happened when,’ and especially the difficulty of establishing the age(s) of protolith of the igneous rocks now overprinted by core complex deformation. This point is underscored in the last sentence of Peter Coney’s introduction to Geological Society of America Memoir 153 on metamorphic core complexes (Coney, 1980b, p. 6), where he stated: “It is extremely important to date the terranes better, and if preliminary results are any indication, the dating of these basement terranes will be a geochronological nightmare which only detailed, multiple-attack methods combined with very careful field control will resolve.”

Each geoscientist who was engaged in the splendid frenzy of field investigations of metamorphic core complexes in the late 1960’s, 1970’s, and early 1980’s will have his/her own ‘take’ on the challenges of interpretation. My emphasis here is that they derived primarily from not recognizing clearly in the very early going that the mylonitic tectonites of metamorphic core complexes were expressions of shear zones, and that rigorous understanding of shear-zone theory would be a critical guide to interpretation. Part of the barrier was the very real mapping-and-visualization challenges associated with determining the three-dimensional shape(s) of the bodies of mylonites in the metamorphic core complexes. Because of the great size and breadth of metamorphic core complexes, and the fact that high-angle (*bona fide*) Basin and Range faulting had pulled them apart, parts are buried beneath deep broad basins, and thus it was never quite sufficient to work within the confines of single mountain ranges to grasp the full geological picture of metamorphic core complexes. Furthermore, there was a steep learning curve in recognizing mylonites and cataclasites for what they were, and another steep learning curve for grasping the relationship(s) between mylonites and shear zones.

When the shear-zone nature of important dimensions of metamorphic complexes finally ‘clicked’ into place, investigators were able to turn almost immediately to a burgeoning, converging new literature that gave insight regarding “fault rocks,” “shear zones,” “deformation mechanisms,” and yes, “sense-of-shear” criteria. Full utilization of these concepts and topics required understanding the close interrelations among all four of these phenomena.

I call on the thoughts of Peter Coney again to underscore this set of points. Consider what he chose to emphasize in the last paragraph of his introduction to the GSA metamorphic core complex volume (Coney, 1980b, p. 5-6): “The most important controversy still remaining is the origin and significance of the mylonitic gneiss fabrics so characteristic of the basement cores of the complexes. Without question, the dramatic resemblance of these fabrics to those produced along deep-seated thrust faults in other parts of the world is the last remaining obstacle to what might be called a new consensus. Some workers have concluded that even this aspect of the complexes was produced by Tertiary regional extension. This fact will demand and inspire much-needed intense wok in the future. It is remarkable how little detailed petrography and petrology have been done in these terranes."

Coney emphasized this for a reason. The first mention of "metamorphic core complexes" in the literature was in 1977 (Crittenden, Coney, and Davis, 1977). Between then and at least 1980 there were strikingly contrasting working-views on the tectonic significance of the fabrics and structures associated with metamorphic core complexes. Identifying promising avenues of interpretation would depend importantly on discerning not 'simply' the ages of the fabrics and structures, but the tectonic transport directions embodied in them, as well as the depth-temperature environment(s) in which these fabrics and structures developed.

Permit me to provide a sampling of the diverse inclinations on tectonic significance of core-complex structures and fabrics, as reviewed by Coney (1980a, p. 8-12). Back in the 1960's in eastern Nevada and western Utah, Peter Misch and his students were trying to prove a connection between the Snake Range decollement and the Sevier-Laramide 'eastern" thrusts,' but could not (Misch, 1960). Roberts and Crittenden (1973) and Hose and Danes (1973) viewed the decollement and younger-on-older faulting as the result of extensional (gravity-driven) faulting of cover off of the hinterland core region, eastward, and producing in the toe region the folding and thrusting marking Mesozoic Sevier fold and thrust structures. Armstrong and Hansen (1966) and Armstrong (1968) were predisposed to a mid-Mesozoic orogeny that remobilized and metamorphosed basement rocks and produced a Caledonian-type remobilized core zone and infrastructure. Coney (1974) advocated middle Tertiary low-angle gravity sliding of unmetamorphosed cover rocks off metamorphic basement on a decollement surface. Further north in the Albion-Raft River-Grouse Creek ranges, Compton and others (1977) concluded that the characteristic core complex fabrics had been imprinted on a mid-Tertiary pluton.

Further south Greg Davis and I saw things differently from one another at that time, especially in regard to whether mylonities exposed in the lower plate beneath detachment faults were formed in Mesozoic and/or early Tertiary (G. A. Davis and others, 1977) as a result of compression and thrusting, or in the mid-Tertiary in an environment of regional extension (G.H. Davis, 1977). Based upon mapping within the Tortolita Mountains in southeastern Arizona, my students and I concluded that the characteristic core-complex lineation and foliation was Tertiary in age and of extensional origin (Davis and others, 1975). We visited one another's field areas to compare and contrast, in the same way that Peter Coney and I visited one another's field areas in 1974, where we confirmed what we thought was the case: amazing similarities between structures and fabrics in the Snake Range of Utah and the Rincon Mountains of southern Arizona (Davis, 1973, 1975; Coney, 1974).

In short, there was tremendous excitement related to recognizing enormous regional tracts of common tectonic properties, and a lot of confusion in trying to assess regional tectonic significance. Amidst the confusion was a sense that interpretations of "tectonic transport directions" held promise in sorting things out.

## Pennrose Conference on "Metamorphic Core Complexes"

GSA Memoir 153 was a product of the (1977) Penrose Conference on "Tectonic Significance of metamorphic core complexes in the North American Cordillera," sponsored by the Geological Society of America, and convened by Max Crittenden (U. S. Geological Survey) and Peter Coney and I (The University of Arizona) (Crittenden, Coney, and Davis, 1980). In 1976, while planning for the conference, we targeted a sweeping descriptive review of eight subregions of enigmatic but apparently homogeneous structure/tectonic character comprising two-mica garnet-bearing granitic intrusions; zones of medium- to high-grade metamorphism; augen gneisses; Tertiary-on-older fault relations (which none other than P. B. King took particular note of during the first evening of the Penrose); low-angle decollement; high-standing domical topography; subhorizontal foliations; consistently oriented penetrative mineral lineations; tracts of geology profoundly rotated (about horizontal axes); and expansive regions of reset K/AR ages. Because the reset K/AR ages were found to be consistently Eocene in the metamorphic core complexes north of the Snake River Plain but Miocene in core complexes to the south of it, we wanted to make certain that both regions were represented in invitations. We chose to cover the following terranes to the north of the Snake River Plain: Frenchman's Cap, Thor-Odin, Pinnacles, Valhalla, Okanogan, Kettle, Selkirk, Bitterroot, and Pioneer. The terranes we chose to cover to the south of the Snake River Plain were Albion-Raft River-Grouse Creek, Ruby, Snake Range, Death Valley Turtlebacks, Whipple, Harcuvar, Harquahalla, South Mountains-White Tank, Picacho, Tortolita, Catalina-Rincon, Santa Teresa-Pinaleno, Comobabi-Coyote, Pozo Verde Magdalena, Madrea, and Mazatan to the south (Crittenden, Coney, and Davis, 1977).

Seventy-two earth scientists and ten student associates from Canada, the United States, and Mexico attended, and based on what we knew then this seemed to be a sufficient range of invitations in terms of expertise, disciplines, and topics represented. (A list of participants is at Appendix A.) Foremost we knew that the invitation list emphasized geologists who had done the mapping of these complex systems, and had paid attention to tectonic setting and history. However, back in 1976 during the planning stage for the conference, we did not appreciate the degree to which the deciphering of the tectonic significance of metamorphic core complexes would depend upon developing an understanding of "shear zones" and "fault rocks," in fact an understanding that could be applied practically in the field based on outcrop-scale observations. Looking back to 1977, "shear zone deformation" per se was not a part of the conversation at Tanque Verde Guest Ranch in Tucson, even though many of the participants had a reasonable working knowledge of the seminal work by Ramsay and Graham (1970) on shear belts and strain variations within them. (Not until 1980 would the clear and comprehensive review of shear zones by John Ramsay appear, an article that made fundamental understanding of shear zone geometry much more accessible to the structure-tectonics community than it had been to date).

Furthermore, most of us who participated in the core-complex Penrose Conference were not really clear on the distinction between "mylonite" versus "cataclasite," nor the specific fabrics to which these terms applied. Part of this can be explained by the fact that the pre-1970 literature on what we now call "fault rocks" did not lend itself to ease of understanding, a view I will expand upon in the next section of this essay. Part of this was due to "mylonites" and "cataclasites" hardly being foremost on the minds of the tectonics community, and thus many of us were simply not paying sufficient attention to a new literature that was taking shape. Bob Compton (Stanford University) was one of the notable exceptions, and thus as conveners of the Penrose Conference we urged him to bring thin sections with him to Tucson to instruct on taxonomy and fabrics. "Fault rocks," which structural geologists now regarded as the essential fourth category of major rock types (after igneous, sedimentary, and metamorphic), were simply not yet an integral part of the experience and vocabulary of most. Ironically, Rick Sibson's classic paper on fault rocks and mechanisms appeared precisely in 1977.

Had we as conveners been prescient, I suspect that the invitation list would have been altered and broadened to include a number of additional geoscientists, and notably more from outside of the U.S., who had been productively engaged in field-mapping and analysis of well exposed shear zones in other parts of the world, and investigators who quite literally were in the process of illuminating the physics of fundamental "deformation mechanisms," and developing practical methodologies and insights regarding how to interpret fabrics in terms of "sense-of-shear."

## Digression on pre-1970 "Structural Petrology"

In the 1970s, when modern structural geological mapping and analysis were just beginning to be applied to terranes in the United States that were to become known as metamorphic core-complexes, relatively few geologists were paying close and informed attention to the structure-tectonic significance of what we now would refer to as "fault-rock fabrics" (especially mylonites and cataclasites) or to shear zones, and certainly not to "sense-of-shear" criteria in any way comparable to what would soon be the case.

It is my impression that prior to the 1970's in the U.S., theoretical and practical knowledge of fabrics was held by a very small number of "structural petrologists." Field-oriented structural geologists on the outside of this subdiscipline 'looking in' may have felt that microscopic petrofabrics held promising insights into interpreting structures and fabrics, but most may have concluded that functioning in this subdiscipline would have required becoming expert in petrofabrics, and incorporating routinely the use of the U-stage, or a modern pole figure device.

After all, the tour de force literature seemed to be anchored in the work of Bruno Sander (1930), and not even Knopf and Ingerson (1938) were able to extract advantageous insights and applications that could be harnessed productively in the course of geological mapping and detailed structural analysis. It proved to be very difficult for many field-oriented structural geologists and tectonists to extract from the general quartz-petrofabrics literature some practical methodologies for use in their ‘real world’ of field-based structural geology and tectonics. Within this literature there is an overarching emphasis on the orthorhombic symmetry of quartz fabrics, and accordingly a proverbial gulf existed between interpreting such fabrics and interpreting shear or slip kinematics. Turner and Weiss (1963) opened the door to providing deeper insights regarding the origin and significance of mesoscopic (outcrop) and microscopic fabrics, but in the end U.S. field-oriented structural geologists found themselves in an environment where “structural petrology,” whatever that expression might have meant, remained largely in the purview of metamorphic petrologists.

Of course the literature on “mylonite” and “cataclasite” had been established very early, with publications on these rocks and fabrics going back into the 19th century, thanks to the seminal insights by Lapworth (1885). It was not a shortage of pre-1970 papers on “mylonite” and “cataclasite” that was the problem. Rather, the problem was due to a combination of other factors.

First, as noted above, few investigators addressing the structural significance of metamorphic core complexes in the late 1960’s and during most of the 1970’s had a commanding understanding of these fabrics and their significance. Notable exceptions were Peter Misch (1960), to whom GSA Memoir 153 was dedicated, Bob Compton (e.g., Compton, 1980), and Vicki Todd (e.g., Todd, 1980).

Second, the developing of a commanding understanding was made difficult by so many individual contributions in the literature on “mylonites” and “cataclasites” that were tantalizingly scant and/or vague and/or inaccurate. Higgins (1971, p. 81), in reference to the work of Conley and Drummond (1965) carried out in the Appalachian Piedmont, writes: “The terms mylonite and ultramylonite are incorrectly used for microbreccias and cataclasites. Except for this inaccuracy, the descriptions are good.”

Third, at the other extreme, descriptions could be elaborate but still confusing, with taxonomy and jargon that would delight John McPhee and his penchant for capturing the ways in which geologists love to name things in multisyllabic ways (McPhee, 1981). For example, in Higgins’ (1971, p. 89) annotation of the work by Reed and Bryant (1964) along the Brevard zone, we read: “ ‘The nomenclature of polymetamorphic cataclasite rocks is complex and confusing, so we therefore define the following terms…(p. 1180):’ …blastomylonite, blastomylonitic gneiss, phyllonite, and phyllonitic schist… They use phyllonite for rocks that should be called diaphthoritic phyllonite.” And on the work of Williams, Turner, and Gilbert (1954) in their introduction to the study of rocks in thin sections, Higgins (1970, p. 94-95) writes: “Divides cataclastic rocks into three categories...: cataclasites, mylonites, phyllonites. Defines: mylonite, pseudotachylite, augen gneiss, cataclasite, phyllonites. Discusses flaser granite and flaser gabbro. All definitions relatively general. “

Fourth, the literature, when viewed as an entire body, can seem internally contradictory even at first-order. When Higgins (1971) published his U.S. Geological Survey Professional Paper on “cataclastic” rocks, he caught the fresh attention of the broad structural geologic community, particularly because he reviewed all the literature, provided a glossary of terms as well as annotations for most of the individual contributions, and laid out a classification system that taught how to use the terminology in an informed and systematic way. But even this work held its own first-order confusing dimensions. Higgins used “cataclastic rocks” as a general term for all fault rocks ranging from fault breccias and fault gouge; through microbreccia and cataclasite; through protomylonite, mylonite, and ultramylonite; to mylonite gneiss and blastomylonite. Yet Higgins observes this was in many ways opposite to the taxonomy of Christie (1960), who applied the expression “mylonitic rocks” as the overarching term for all of what Higgin’s (1971) later described as “cataclastic rocks.”

Fifth, few investigators took advantage of the insights clearly and accurately available in the older literature. There were missed opportunities in leveraging what had been learned about the character, and changing character, of mylonites and cataclasites in certain belts of mylonite. It was as if field geologists attempting to understand mylonites and cataclasites were focusing too much on the taxonomies and microscopic properties and insufficiently on some critical observations on map patterns of these fault rocks." There are some illustrations which apply particularly to core complex terranes. For example, Peach and others (1907), based on their work on the Moine thrust, concluded that "flinty crush-rocks and mylonites were…formed simultaneously in different parts of the same line of movement'" (Higgins, 1971, p. 87). Termier and Boussac (1911) "describe a complete transition between undeformed granite and mylonite" (Higgins, 1971, p. 92). Phillips (1937) notes "In a crushed quartzose rock, lenticular grains of highly strained quartz lie in a fine-grained mylonitic matrix. … this mylonite presents an interesting example of the differential yield of quartz grains to the shearing according to the attitude of their internal structure in relation to the impressed shear planes" (Higgins, 1971, p. 88). Armstrong (1941) describes zones that "outline lenticular masses of less sheared rocks and in some places attain a thickness of a quarter of a mile" (Higgins, 1971, p. 78). Based on their work in Tanganyika, Sutton and Watson (1959) distinguish and map "wide 'shear belts" and thin 'mylonite belts'. The mylonite gneisses can be arranged in a series showing increasingly complete mechanical breakdown, while the noncataclastic types form a second series which appears to show increasing degrees of crystallization' " (Higgins, 1971, p. 92).

## New Directions

In the 1970s, profound changes appeared in the ways in which deformational fabrics were being addressed and interpreted. The clearest signal of this to the broader structure-tectonic community occurred as an awakening,' with the publication of the new structural geology textbook written by Hobbs, Means, and Williams (1976). This text bore the unprepossessing name: An Outline of Structural Geology. Inside this 'outline' were riches for the structure-tectonics community in the United States, including those of us working on metamorphic core complexes. The book contained detailed and accessible overviews of "principles of microstructural development," "microstructures developed in rocks undergoing deformation," "crystallographic preferred orientations in deformed rocks developed by slip and rotation," "crystallographic preferred orientations in deformed rocks developed by recrystallization," and "mylonite zones." Some of the new insights were built upon a surge of new 1970's contributions on mylonites, mica fabrics, and quartz fabric deformation mechanisms (e.g., Bell and Etheridge, 1973, 1974; Elliot, 1972; Lister, 1974; Lister and Hobbs, 1974; Lister and Paterson, 1974; Etheridge, Paterson, and Hobbs, 1974; and Tullis, Christie, and Griggs, 1973).

Compton (1980) had grasped this before he framed an important part of his work on deformed quartzites in the Raft River Mountains (Utah) through addressing deformation mechanisms and fabrics. His research there included attempts to partition "pure shear" versus "simple shear" based on quartz-petrofabric analysis. I now note with some interest that his contribution to GSA Memoir 153 included just three bibliographic citations related to deformation mechanisms and the behavior of quartz: Lister's (1977) discussion on fabrics in quartzites plastically deformed by plane strain and progressive simple shear; Tullis' (1977) paper on preferred orientation of quartz produced by slip during plane strain; and Lister, Paterson, and Hobbs' (1978) model for fabric development in plastic deformation and its application to quartzite.

Most investigators of metamorphic core complexes in 1977 had not yet been personally impacted by the new opportunities, methodologies, and concepts that were coming available through the emerging literature. I can illustrate this with a simple, single observation. Max Crittenden, Peter Coney, and I wrote a brief "Penrose Conference Report" which was published in Geology in 1977. In that 2-page write-up there is not one reference to "mylonite" or "cataclasite." Furthermore, the post-Penrose papers presented (just 5 months after the conference) at the 1977 National Meeting of the Geological Society of America in Seattle contain just two abstracts in which "mylonite" fabrics are referred to as such (G. A. Davis and others, 1977; G. H. Davis, 1977), in spite of the fact that all of the investigators had clear first-hand awareness of the penetrative foliations, lineations, and zones of cohesive breccias so dominant in the core complexes and so essential to interpretive models.

However, things were changing fast. By the time the GSA Memoir on core complexes was published in 1980, "foliations" and "lineations" were prefixed abundantly by the term "mylonitic," and references to various types of cataclasites, including "microbreccias," were being made with much greater discernment and confidence. The Citation Index reveals and calibrates certain aspects of when things began to change, and how fast. It is fascinating to harness the Citation Index and explore the frequency of investigators citing one another with respect to the topics of “shear zones,” “sense of shear,” and “fault rocks,” and then to compare these with frequency of citations for the topic “metamorphic core complexes.” There are literally no citations of the topics “fault rocks” and “sense of shear” before 1985, and essentially no references to the topic of “shear zones” before 1985. In 1991, the citations for each of these three topics ramped up profoundly. By the year 2007, “shear zones” enjoyed ~10,000 citations, “fault rocks” ~8000, and “sense of shear” ~3000 per year. The topic-citation numbers for “metamorphic core complexes” is quite similar to that for “shear zones” (except for raw numbers) in that the first citation is 1979 and the jump-off point is 1991. In 2007 total topic citations for “metamorphic core complexes” numbered ~1300.

It is not sufficient to view these Citation Index records as indicating that coherent new literatures had arrived, which they had. Rather, the Citation Index reveals a sustained surge in ‘cross-talk,’ one that began happening in ~1980.

## Personal Reflections

The ‘new consensus’ about metamorphic core complexes that had emerged by 1983 contained the following notions: 1/ a crustal-extension origin, 2/ in the mid-Tertiary, (3) with mylonites and cataclasites fashioned in character and geometry (4) by normal-slip ductile-brittle shearing. Paper upon paper published in 1983 and later affirmed time and time again these interpretive conclusions. That literature, and all that followed to the present, devotes itself to both fundamental and specialized inquiries that are adding needed clarifications of the details of the new consensus. One of the most profound and important of these is trying to fully understand the mechanics of origin of detachment faulting, and what causes such concentrated localization of strain. A breakthrough paper was Wernicke’s (1981), who “saw” the formation of metamorphic core complexes and detachment faulting at the crustal scale. His tectonic insights profoundly influenced the way in which crustal-scale regional cross sections of terranes of core complexes and detachment faults would be drawn from then on (e.g., G.A. Davis, Lister, and Reynolds, 1986).

I too tumbled on “shear zones,” but at a different scale, as the context to explain what I was mapping and analyzing in individual metamorphic core complexes, especially the Catalina-Rincon complex. How did I get there, exactly? For me, as a structural geologist and in the context of this volume on “sense-of-shear,” it has been interesting, and sometimes unsettling, to track vocabulary and concepts to see when “shear zones” and “sense of shear” and “fault rocks” and correct usages of “cataclasite” and “mylonite” entered my life as one of those engaged in the splendid frenzy.

In 1977 (Davis, 1977) I was using the language “augen gneissic cores,” “metasedimentary carapace,” “decollement,” and “decollement zones” in describing the structural components of core complexes. I had adopted “decollement” from Peter Misch’s original language for the Snake Range detachment, and Peter Coney’s (1974) follow-up usage. (It took me a long time to set that term aside). I emphasized that the “decollement zones” were composed of mylonite and mylonite breccia. I used the expression “mylonitic schist” in reference to strongly folded, blackish, fine-grained rocks (ultramylonite) full of folds and located at the interface between two varieties of gneisses. My interpretations emphasized mid-Tertiary deformation through a profound flattening and extension, transposition, and ductile through brittle extension and flow in the direction of mineral lineation. My analogue was megaboudinage on a crustal scale (i.e., coaxial strain). I still viewed folding in the cover rocks as having been produced by mid-Tertiary gravity induced folding (Davis, 1973, 1975). I imagined that the decollement zones were localized by ductility contrast at the great unconformity (sigh). I recognized “ductile normal shearing” that projected deep into basement on one side of the core complex.

In 1978 (Davis, 1978) I see that I was characterizing a fault stratigraphy within the so-called decollement zone, distinguishing the omnipresent ledge of microbreccia directly beneath the fault surface itself from what typically lay below: chlorite breccia, transitioning into breccia with preserved patches of thinly banded mylonite, and then downward into mylonitic gneiss with only localized brecciation or chloritic alteration.

In 1979 (Davis and Coney, 1979) I was gaining a better handle on using terms such as “mylonite,” “blastomylonite,” and “microbreccia,” but with tell-tale relapses into expressions such as “low dipping cataclastic foliation.” My dominant interpretive theme remained ductile-through-brittle extension and flow in the direction of mineral lineation, but still driven largely by vertical flattening and subhorizontal extension. “Shear zones” was not part of my working vocabulary as applied to core complexes, and thus I struggled with language in trying to describe what I was mapping and imagining (e.g., p. 123): “The normal faults…constitute a new concept in large-scale faulting, one that we term herein ‘ductile growth faulting.’ The faults are ‘growth’ faults in the sense that the crystalline surface on which metamorphic carapace materials are plated increases in area during the life of the fault. They reflect a mode by which the surface area of the crystalline basement can be increased during extension.”

In 1980 (Davis, 1980b) I am now almost always preceding “augen gneiss” with “mylonitic;” I am reporting that metasedimentary “carapace tectonites” are “smeared,” “welded,” or “plated” onto the underlying crystalline rocks; and I am noting that decollement zones are composed of “fine grained cataclastically deformed rocks.” I reemphasize that the structural fabric of mylonitic gneisses and carapace tectonites are intimately coordinated, and that decreasing ductility through time is recorded by features such as “folding of lineation and refolding of folds along ductile normal fault zones and superimposition of brittle normal-slip faults upon ductile faults.” I see that I had tacitly backed off on gravity-induced deformation (whew), stating that cover rocks became denuded both during and after the formation of lineated tectonite, and that “late-stage mid-Miocene listric normal faulting further denuded the cover… .”

By November, 1980, my thinking about the kinematic significance of metamorphic core complexes and decollement (i.e., detachment faults) had seriously shifted from dominantly coaxial strain to non-coaxial shear. “…, the mylonitic tectonite acquired its strain during progressive simple-shear rotational deformation and not by pure shear (although pure shear probably occurred locally). … With respect to present attitudes, the mylonitic tectonites appear to be zones of ductile normal slip (flow). If so, these tectonites may be partial exposures of ductile normal shear zones of regional extent. Mapping the tectonites as parts of regional ductile shear zones, and mapping both the sense of simple-shear and the slip-line path are necessary to assess the tectonic significance of Cordilleran metamorphic core complexes as a whole. …The position of the ductile shear zone represented by mylonitic tectonite appears to be well below the great unconformity” (Davis, 1980a, p. 157).

Consequently, in Davis, Gardulski, and Anderson (1981) the fault-rock language that I was using seems to have come together properly. By this time I have largely dropped “core,” “carapace,” and “cover” from my vocabulary (Davis , 1980a), and succeeded in staying away from references to “lower plate” or “upper plate.” I used “detachment” not as the fault itself but as what most would describe as “upper plate,” for I was increasingly concerned that we may never know the number and character of “plates” involved nor the number of decollement zones at depth. My use of “detachment” in this way was short lived. Primarily I emphasized that “the tectonite fabric is interpreted to have formed by ductile normal shearing within gently dipping curvitabular zones of simple-shear. As cooling of the system took place, some already-formed mylonitic tectonite was disrupted, rotated, and microbrecciated by closely spaced, mesocopically penetrative, normal-slip listric (?) faults… The kinematic coordination is perfect” (Davis, 1981a; Davis and Hardy, 1981).

I was impressed with the fact that even the low-angle faulting of Miocene fanglomerates translated these rocks in a direction perfectly parallel to the direction of penetrative lineation in the closest mylonite tectonites, and that this held for southeastern Arizona as well as the southern part of the western Cordillera (Davis, 1980a). This is the theme I chose to emphasize, and illustrate, in my presentation at the GSA “Frontiers of Structural Geology Symposium,” seminar, which coincided with the launching of the Structure-Tectonics Division of the Geological Society of America (Davis, 1981b), and which in turn was the basis for my paper on the shear zone origin of metamorphic core complexes (Davis, 1983).

The challenge I experienced in coming to recognize shear zone deformation at work was related importantly to my inability at that time to ‘read’ the mylonite fabrics for sense of shear. Determining the sense and direction of movement of faults in the “upper plate” was reasonably straightforward, especially where the rocks consisted simply of tilted Miocene fanglomerates and the low-angle normal faults were adequately exposed. It was just matter of analyzing bed strike, bed dip, fault dip, and slickenline orientations. For the Catalina-Rincon Mountains metamorphic core complex, I determined that the direction and sense of movement was ~S60°W. Determining direction and sense of shear in the tectonite carapace of mylonitic marbles and calc-silicate rocks proved to be manageable using Hansen's (1971) separation arc method in the analysis of the penetrative folding; direction and sense-of-shear once again proved to be ~S60°W. But defining sense-of-shear within the mylonites would have been impossible for me, had it not been for the bands of ultramylonites within the mylonitic gneisses, and being able once again to harness the folds contained therein using Hansen's separation arc-method. But this was spotty and slow going. Most challenging was discerning that there is a decipherable sense of fault transport in the cohesive breccias, and then determining what it was. Thankfully for me there is a beautiful set of exposures in Saguaro National Park (East) containing faulted and rotated mylonitic fabrics (foliation and lineation) that lended themselves to fine-scale mapping and stereographic analysis to work out rotations (Davis, 1980a).

## But Things Changed Fast

Perhaps the most significant agent of change, as well as the most tangible symbol of the changes taking place, was the Penrose Conference held in 1981 in San Diego, California, on the "Significance and Petrogenesis of Mylonitic Rocks." The conveners were Jan Tullis, Art Snoke, and Vicki Todd (Tullis, Snoke, and Todd, 1982). As a result of this Penrose Conference, knowledge of practical field methods for discerning sense-of-shear in outcrop spread like wildfire. I did not attend, but lore has it that at the first good exposure of outcrops of mylonites Carol Simpson, Stephan Schmidt, and Gordon Lister 'decoded' for others how to 'read' sense-of-shear, through various methods and fabrics! In an instant those of us doing structural geology in the Western Cordillera became aware of how to determine sense-of-shear on the basis of outcrop examination, so fast did the word spread. "Mica fish," "S-C," "C'," "shear bands," "delta-type porphyroclasts," and "sigma-type porphyroclasts" became part of the working vocabulary. Soon these techniques, and the deformation-mechanisms behind them, would become broadly available in the literature. The work of Simpson and Schmid (1983) in particular had immediate major impact. A byproduct of this paper was their calling clear and appropriate attention to the seminal discoveries by French tectonists, who first introduced the concept of "S-C fabrics," one of the finest sense-of-shear indicators in mylonites derived from granite (Bérthe, Choukroune, and Jégouzo, 1979a; Bérthe, Choukroune, and Gapais, 1979b). The French cohort of structural geologists and tectonists in the mid to late 1970's had been picking-apart shear-zone deformation within and along the South Armorican shear zone in Brittany (Jégouzo, 1980). With the passing of time we have had, of course, additional essential and broadly accessible references on shear-sense indicators (e.g., Simpson, 1986; Passchier and Simpson, 1986; Hanmer and Passchier, 1991).

The report on the 'mylonites' Penrose is worth reading carefully (Tullis, Snoke, and Todd, 1982), as is the overview of fault rocks (Snoke and Tullis, 1998), which also was an outgrowth of the conference. These contributions include detailed summaries of the tortuous pathways along which our present understanding of "fault rocks" and "mylonites" have traveled from the time of Lapworth (1885). To be sure, the decision to hold the conference was motivated by the "continuing controversy over the nomenclature of fault rocks" (Tullis, Snoke, and Todd, 1982). We learn as well from Tullis, Snoke, and Todd (1982, p. 230) that participants concluded that " 'cataclastic rock' should be abandoned as a general term," and that the use of the term "mylonite" should be applied to rocks marked by concentrated strain, grain size reduction, narrow planar zones, enhanced foliation and/or lineation, and a deformation mechanism that was ductile, with little or no microcracking. I particularly appreciate their closing observation (Tullis, Snoke, and Todd, 1982, p. 230): "As structural geologists, we are interested in the processes and conditions of deformation; we need to determine criteria for the recognition of faults and shear zones; rather than arguing about terminology."

The photographic atlas of fault rocks (Snoke, Tullis, and Todd, 1998), though published 13 years following the Penrose Conference, filled an enormous void in the literature. It organizes fault rocks smartly (by brittle behavior, semi-brittle behavior, and ductile behavior), provides stunning photographic images of all kinds of fault rocks, conveys the structure-tectonic context of each of the examples, and presents a comprehensive bibliography.

## Colleagues and their Impact

I now distinguish two stages of my 'core-complex' life, insofar as related to the interaction with special colleagues. There were first the early "heroic years," which is what Peter Coney would call them, when the challenges and opportunities abounded in the 'tracking down' and mapping of these complexes, …even spotting them from the air when landing at Sky Harbor Airport in Phoenix. The list of colleagues and student-colleagues that particularly impacted my life and my thinking while working on the ground in southern Arizona is very long, and includes the likes of Peter Coney, Greg Davis, Lawford Anderson, Steve Reynolds, Tom Anderson, Bill Rehrig, Eric Frost, and Stan Keith. It was my good fortune to benefit directly from these individuals, and others, and to work closely with the many students who would address core complex problems (e.g., Terry Budden, Bob Varga, Monte Swan, Chuck Kiven, Gene Suemnict, Greg Benson, Steve Lingrey, Jim Hardy, Jean Crespi, Bob Krantz, Jerry Hansen, Lee DiTullio, Steve Naruk, Veronica Martins, Ann Bykerk-Kauffman).

Then there was a second stage of connection with special colleagues, ones who unveiled deformation mechanisms and sense-of-shear-criteria. I conclude by describing several such examples.

After the Penrose Conference on mylonites, in 1981. Gordon Lister came to visit. We had been corresponding about the quartzite tectonite fabrics beautifully exposed in the Coyote Mountains metamorphic core complex in southern Arizona. I had reported on discovering and interpreting these rocks and fabrics (Davis, 1977, 1980), and Anne Gardulski had carried out thesis work involving larger-scale geological mapping and quartz-petrofabric analysis (Gardulski, 1980). Anne and I 'showed off' these rocks and structures on a GSA field trip (Davis, Gardulski, and Anderson, 1981). Frankly, Anne and I could not make sense of the quartz petrofabrics, and at the suggestion of Jan Tullis, we sent samples to Gordon Lister. He proclaimed them at once to be the finest quartzite mylonites he had ever seen, and as gently as Gordon knows how, he told us that our quartz c-axes were misplotted. He had to see these rocks, and this is what caused him to make a visit. We went to the Coyote Mountains together, and he introduced me firsthand to "mica fish," "stair-stepping mica fish trails," and the use of these fabrics in determining sense-of-shear. Through the quartz fabric plots he had prepared for these same rocks, he showed how they conformed beautifully to deformation under conditions of non-coaxial shear (Davis, Gardulski, and Lister, 1987). Later, Lister and Snoke (1984) featured these particular Coyote-Mountains quartzites as their example of type II S-C mylonites.

In May, 1982, Gordon held forth in seminar at The University of Arizona, and walked us through crystal-plastic behaviors, strain partitioning, the vicissitudes of strain hardening and strain softening, S-C mylonites, pole figures connoting coaxial versus non-coaxial shear, and dynamic recrystallization. Again, in only the way that Gordon can do, he introduced us to "banana fish," which he explained are fashioned by the presence of mica veneers that give the impression that the fish are wrapped in banana leaves. He also delighted in debunking the "myth" of orthorhombic fabric.

At some time during Gordon's visits, he connected again with Steve Reynolds at Arizona State University, where Steve tested – to Gordon's delight – a new sense-of-shear indicator that he had discovered, namely "fish flash!" (see Davis and Reynolds, 1996, p. 523).

Carol Simpson visited in late 1982, or early 1983, … we cannot quite recall. Bill Dickinson and I gave Carol a tour of the Rincon-Catalina core complex. When we got to Saguaro National Park (East), she riveted on "the very fine grained, black phyllonites and mylonites …with recumbent isoclinal intrafolial folds and splendid mica fish and asymmetrical feldspar porphyroclasts in gneissic layers that all indicated down-dip movement on the detachment faults" (Carol's notes). She talked to us about S-C, shear bands, grain-size reduction, delta-type porphyroclasts with soft recrystallized materials in their tails, sigma-type porphyroclasts and their 'teapot' shapes, and mica fish with recrystallized tails. She pointed out domino-faulted feldspar grains, urging us to concentrate only on those that are cut by fractures at very high or very low angles to the zone, emphasizing that those at 45º, when rotated, become misleading. She emphasized that the dynamically recrystallized quartz creating oblique foliations yields only the final incremental deformation, so beware! She warned us to keep clear of the feldspar clasts. Carol's language, always appropriately nuanced, described circumstances under which we would surely draw incorrect conclusions if we did not understand and appreciate the deformation mechanisms. We examined together the so-called "mylonitic schists" and left these outcrops calling them "ultramylonites." We examined the extraordinary ultrafine-grained shiny black rocks I had mapped at the detachment fault position on Tanqe Verde antiform in the Rincon Mountains, for I wanted to know whether Carol thought any of these might be pseudotachylite. I was relieved when she agreed they were cataclasites, and we left those outcrops calling them “ultracataclasites.”

These visits by Gordon and Carol were similar to what was happening in any number of geosciences departments, where the transfer of knowledge was taking place at ballistic speed. Undergraduate and graduate students were in on the action, and they took up the language as if (in today’s term) they were hooking up their IPODS. Just as geomajors and graduate students today, they would have had no idea how much flailing had taken place before the introduction of practical, reliable sense-of-shear indicators.

The impact of these particular visits, the connectedness between research and teaching, and the literature that was rolling out, were profound. I re-read a tiny abstract by Naruk and Davis (1983, p. 650), where we write: “Structural and petrofabric analysis of mylonitic gneiss of the Pinaleno Mountains metamorphic core complex demonstrates 1/ that the mylonites represent a low-dipping zone of heterogenous simple shear, locally modified by pure shear and/or volume loss; 2/ that the shear strain within such zones can be quantified as a function of the angle between the mylonitic foliation and the shear zone boundary; and 3/ that the total translation across such zones can be quantified from the calculated shear strains and the inferred original thickness of the zone.” Moreover. “Microscopic schistosité (S) and cisaillement (C) planes, elongate subgrains, and asymmetric porphyroclasts are everywhere consistent with normal displacement, non-coaxial shear.” The changed language, the broader scope, the higher-order problems captured in these fragments reflect the results of the cross-talk that flourished in 1981 and 1982.

There are two other connections with colleagues that I wish to mention. Rick Sibson visited in November, 1984, and his timing was perfect for me and my students. It was important to learn firsthand from him his ‘take’ on fault rocks, and particularly the mechanisms of formation of same (Sibson, 1977, 1982). His descriptions of breccia formations took us into “fault sucking” and “implosion.” He graphed the granite rigidity as a function of depth and temperature. He drew longitudinal profiles of the San Andreas fault, showing the distribution of microseismic focal centers. He emphasized that thrusting effectively decreases the geothermal gradient, thereby resulting in a drop of the brittle-ductile transition; with detachment faulting working the other way around, as I was later to see so clearly along the Moine thrust. He gave us what I would call “pseudotachylite envy” as he described exposures along the Alpine fault, and explained that holding a sample of pseuotachlylite in your hand is like holding an earthquake, …a fossil earthquake.

Through these experiences, and the literature, I became much more aware of the significance of previous work along the South Armorican shear zone in Brittany. To my great pleasure in 1986 I heard from Jean Pierre Brun that he and some of his colleagues wanted to visit. Jean-Pierre, Pierre Choukroune, Jean Van den Driessche, and Gwen Gweron landed at LAX and drove to Tucson, checking out core complex localities and big faults along the way, and being staggered by the vastness of the region. Together we looked closely at mylonities and cataclasites within the Catalina-Rincon-Tortolita core complex, and Gwen succeeded in identifying a thesis area in the Durham Hills at the far north end of the Catalina-Rincon-Tortolita metamorphic core complex, where he worked on S-C-C’ relations in the context of strain and displacement. In 1989 I participated in Gwen’s jury defense at The University of Rennes, and this gave me the opportunity to visit some classic locales within the South Armorican shear zone, in the hands and through the eyes of Jean-Pierre Brun, Philip Davy, Dennis Gapais, Gwen Gweron, and Peter Cobbold. The South Armorican shear zone of course is a right-handed strike-slip shear zone, yet when you pull mylonite from the outcrop and look at it free-standing out-of-orientation, you have mylonite in your hand from the Rincon Mountains. This experience particularly brought home the realization that while structure-tectonics people in the Western Cordillera in the 1970s were working hard to try to unravel the tectonic significance of metamorphic core complexes, French tectonists from Rennes were similarly applying their minds to deciphering the mylonites in Brittany. Cross-connection between the two groups in the 1970’s would have accelerated interpretations of the tectonic significance of metamorphic core complexes.

On the other hand, looking back, I would not have wanted it any other way.

## Conclusions

I am fortunate to have been among the Cordilleran geologists who gained immeasurably from grassroots communication and partnership among structural geologists internationally. The immediacy of impact of these cross-boundary interactions is historic, I believe, as related to the intertwining of fundamental research, teaching, and PhD training. The National Science Foundation, which supported much of my work and for which I here again acknowledge appreciation, should feel some pleasure in their investments, which spread so far in terms of discovery and education. I feel particularly fortunate to have engaged in the full partnership in the early going: first by having hosted numerous structural geologists, taking these colleagues into the field, showing them the spectacular, controversial 'extensional' record of structures and fabrics in core complexes in southern Arizona; and in the process receiving benefits of reciprocation beyond full measure.

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