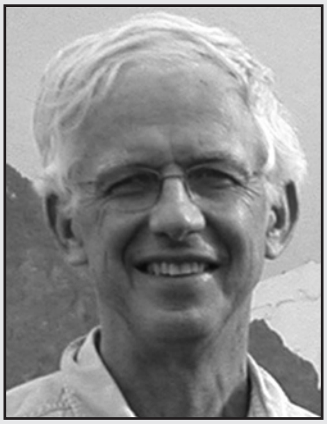
# STRUCTURAL GEOLOGY & TECTONICS DIVISION CAREER CONTRIBUTION AWARD

**Presented to John Suppe**



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### *Citation by John H. Shaw*

John Suppe is a preeminent scholar and teacher of structural geology, who has profoundly influenced our understanding of deformation in the Earth's crust. He is perhaps most renowned for his pioneering work on fault-related folding, a broad family of concepts and theories that quantitatively relate the growth of the two main classes of structures in the brittle crust - folds and faults. Building on his training and experience as a field geologist working in California and Taiwan, John recognized that the positions and geometries of folds in sedimentary strata were closely and predictably related to the shapes and displacements of underlying faults. John formulated an elegant theory, based on simple physical principals, that quantitatively related these structural forms. In his Landmark 1983 paper, "Fault-bend folding," John presented a formulation of these theories that allowed use of fold shape to predict fault shape and displacement. This theory rapidly became a standard approach for generating balanced geological cross sections in fold-and-thrust belts, and further sparked a field of structural geology dedicated to developing quantitative theories that describe other styles of fault-related folding. While many scientists have made important contributions to this subject, there should be no doubt that John's pioneering work is responsible for defining and inspiring this field. Based on a Science Citation Index Search of fault-bend, fault-propagation, and fault-related folding yields well more than one hundred works since the 1983, when John's initial paper was published, and none before.

Fault-related folding theories naturally expanded through their applications to the regional structural geology of orogenic margins throughout the world. Inspired by collaborations with the petroleum industry, John soon began investigating structures throughout the world using various types of subsurface data, including seismic reflection profiles. Working with seismic reflection data in offshore regions, John recognized how syntectonic sedimentary deposits were deformed by these structures into unique and revealing patterns that record the kinematics of folding much as magnetic anomalies record the process of sea-floor spreading. John then expanded his theories to describe folding of syntectonic growth deposits, again defining a major theme of research in this field which focuses on using growth strata to infer fault-related folding mechanisms as well as to determine rates of folding and faulting.

Collectively, these expanded growth fault-related folding theories have become widely used, both in academic and applied fields. In particular, John's methods are now regularly applied in the analysis of oil and gas prospects, and have contributed to the discovery of major fields in several of the world's most petrolific basins. Moreover, fault-related folding techniques have proven well suited to investigating active faulting and folding, providing means to define the subsurface positions, geometries and displacements of faults that are capable of generating destructive earthquakes. John defined the geometry of the Chenglupu fault in Taiwan more than 25 years before it ruptured in the 1999 (Mw 7.6) Chi Chi earthquake, and similar efforts have helped define active faults in southern California, including major blind thrust faults beneath Los Angeles. Insights from these studies have lead to a redefinition of seismic hazards in southern California, influencing how building codes are defined and emergency responses are planned. Few research topics in geology have proven to have so significant a financial and social impact.

John's development of, and contributions to, the science of fault-related folding clearly amounts to a stellar career accomplishment; however, it is important to note that he has made many other important contributions to related fields of science. These include defining the state of stress acting on the San Andreas fault using bore-hole breakout data, which is the basis for the weak-fault hypothesis, and helping to decipher the tectonics of the active Taiwan orogen. In this latter work with colleague Tony Dahlen and students, John helped developed a new quantitative description of how mountain belts such as Taiwan, and large thrust sheets that underlie them, form. The theory of critical taper wedge mechanics describes how fold-and-thrust belts behave much like soil pushed in advance of a bulldozer, deforming internally until a critical shape, or taper, is achieved and then sliding stably until more material is added to or removed from the wedge. The theory invokes brittle deformation mechanisms to relate the taper of the fold belt to its internal strength and that of its basal detachment, and has proven widely successful in explaining the mechanics of both active and passive margins fold belts. This remains an active area of research for John, and he will undoubtedly continue to provide us with exciting new insights.

Finally, it cannot be said that John's research has been provincial, in the spirit of the classical geologists who spent their careers working on the rocks and structures of a given region. Rather, he and his students have consistently sought the best datasets to solve fundamental challenges in our science regardless of geography. A case in point is the body of work by John and his students investigating structural styles and patterns of deformation on Venus—using synthetic aperture radar (SAR) data and altimetry collected by the Magellan mission to define patters of stress and deformation that reflect a system of plate interactions very distinct from plate tectonics on the Earth.

As an educator, John's career accomplishments include publication of his influential undergraduate textbook—*Principals of Structural Geology*. The book has been widely used as an undergraduate text, and a brief review of similar texts published before and since reveals how influential John's approach to the topic has been. Over his distinguished career, John has also served as a mentor to lineage of successful graduate students, who now hold distinguished positions in academia and industry, and has contributed through service to Princeton University, the National Taiwan University, and the broader field of structural geology through his guidance and leadership.

Based on this tremendous body of work and service, John Suppe is most worthy of the GSA Career Contribution Award recognizing his tremendous career contributions to the field of structural geology and tectonics.

***Response by John Suppe***

Thank you John for your gracious words. Actually, when I read your citation a few weeks ago, it brought to my mind several impressions. I would like to share these with you all, if you will indulge me.

My first impression was that when John mentioned various research contributions, what popped into my mind wasn't the science at all, but various people I know—former students, postdocs, collaborators in these projects, and other contributors to these fields. Of course science is people—all our structural geology and tectonics is done together, we're an intellectual and social community. This is true even if we publish single-author papers, because science is fundamentally discovering, communicating and testing ideas about the universe in public community discourse. Sociologists of science like Bruno Latour understand this very well. So if we somehow did our research in secret and we didn't share it with the community, then it wouldn't be science. It would be research but it wouldn't be science, because it wouldn't lead to robustly tested knowledge and it wouldn't be available for use by other researchers to fuel the growth in public knowledge.

But this public community of scientific discourse, if we are honest, isn't an idyllic utopian community—for a number of reasons. For example, one weakness in structural geology and tectonics is that a lot of the research is secret industrial research—which doesn't lead efficiently to growth in robust public knowledge. This holds us back and it's not going to change. So we have to make the best of this, and actually working with industry can be very fruitful. Another reason science isn't idyllic is that we can very easily get into serious conflicts that are not just scientific disagreements. We don't just disagree with each other; we make serious personal enemies. I know this because I've done it—and I think that this is true of everyone who stands up here to receive such career awards. But this isn't the way it's supposed to be. We need to be able to have strong-minded scientific disagreements and we need to be able to compete for scientific resources in ways that don't make us personal enemies. So when I say that John's recounting of various research contributions caused me to remember people, most of these memories are fond memories, but a few are painful and even embarrassing memories. But hopefully my enemies and I have patched things up by now and are becoming fast friends again, because truly one of the great delights of a career is the ongoing friendships spanning decades and spanning the entire globe. We really have a great racket in structural geology and tectonics.

My second impression is more elaborate and will actually take the remainder of my time to sketch out. John's citation, and the science he describes, for some reason made me think of Harry Hess. Now I imagine that some of the younger people here tonight might not know of Harry Hess—after all, even very great fame is actually quite ephemeral. Hess was a professor at Princeton and a very famous and influential guy fifty years ago. He was famous long before he made his best known contribution, which was the idea of sea-floor spreading. The only time I ever met Hess was when I was an undergraduate at UC Riverside in the early 60s and some of us drove to Pasadena to hear him give a talk at Caltech. By the time I arrived on the faculty at Princeton in the early 70s, Hess had already died, quite suddenly of a heart attack. In those days people who had known Hess were full of Harry Hess stories—it was very clear that he had made profound and diverse impressions on many people. Some of the stories were very funny; Hess was colorful.

But the story that made the biggest impression on me concerns his Caribbean Research Project and how he assigned students their PhD projects. It seems that Hess would give each student a quadrangle to map—many of them were in northern Venezuela—and it didn't seem to matter what the geology was. It could be all alluvium or all granite for all he cared. He figured that if you mapped your quadrangle and you wrote your thesis, you got your PhD. But he was also very confident that the better students would find important science to study in their quadrangle. And some of the students clearly did just that—for example one of the better ones was Ron Oxburgh, who later was knighted to become Sir Ronald and is now Baron Oxburgh. It seems that Hess had the confidence that you could plop down anyplace on Earth and there would always be something fascinating and fundamental to discover.

Now when I heard this story about Hess it sounded completely preposterous. It seemed to me that you should choose projects for their importance and likelihood to succeed. I remember arguing about this. But as I look back on my career, I have to admit that I blindly stumbled upon nearly all the important things I have discovered. I certainly did not set out to make any of these discoveries—they just plopped down in front of me like "Pennies from Heaven." I literally tripped over them. So I've come around to think that there are some fairly basic truths underlying Hess's research strategy. But I still wouldn't choose field areas at random, just like I wouldn't drill wildcat wells at random.

The fundamental reason I think Hess was right is that the Universe is very rich and it has many fascinating surprises that are largely unanticipated. Now this is a controversial idea. For example there was a book "The End of Science" written a dozen years ago by the journalist, John Horgan, who argued, based on his rather strange personal philosophy plus interviews of well known scientists, that science is getting mined out, that most of the big discoveries have already been made. This is actually a fairly light-weight book, but it is a serious discussion. A more substantial analysis comes from Nicholas Rescher, who is a well-known philosopher at Pittsburgh and an amazingly prolific guy, having published over a hundred books. Rescher argues that the Universe is intrinsically very rich with things to discover, providing essentially no practical limit to science. I'm not sure I buy his full argument, but my limited experience is that the universe of structural geology and tectonics is very rich.

But it is also true that science is like mining. Once discoveries are made you can't make them a second time. And areas of science clearly get mined out and are left behind as people move on to new rich opportunities. Subdisciplines in science typically last for less than a scientific career. We need to move on if we aren't going to inhabit scientific ghost towns well before we reach the ends of our careers. I remember that immediately after I defended my PhD at Yale, my advisor John Rodgers took me aside and told me that it was OK to keep working for while on my line of thesis research, which was the Franciscan terrain in California, but I shouldn't keep working on the same mountain belt for my whole career. Rodgers' advice was very good advice.

So we need to ask ourselves, are we miners or are we prospectors? Both are good ways to make a living; each suits different personalities. But if we are miners we need to ask ourselves, when is it time to move on to some richer mines? And if we are prospectors, how do we discover new fields, new subdisciplines, that would be exciting to mine? People who get career awards and people who get elected to the National Academy or receive Nobel prizes and other awards are largely people who have discovered new disciplines, subdisciplines, or in my case sub-subdisciplines. They are basically prospectors who have found rich new mines for all of us to work at mining out. This sort of entrepreneurial effort is really needed to make our science move forward—just like entrepreneurs are needed keep the economy moving forward and to provide new jobs.

In my career I've done a lot of mining, but I've also done some prospecting and I've even stumbled upon a few new intellectual mineral deposits. So let's ask ourselves, "What will increase my odds of stumbling upon a new subdiscipline?" That's worth thinking about. I actually think that Harry Hess's strategy of assigning every graduate student a random quadrangle to map is OK, but I don't think it's the best way to increase your odds of discovery. Let me share a few research strategies that have been fruitful in my career.

The first one sounds crazy. It goes like this. When you are starting out in what is for you a new area of research, don't read the literature. Avoid reading the literature as much as you possibly can. Often new graduate students want to carefully read all the relevant papers before they start their research. That can poison your mind because you will very likely end up falling into intellectual ruts. It keeps you from coming up with fresh perspectives. But once you come up with some ideas, then you need to get in and wrestle with the literature.

My next advice is this. Consider being somewhat contrarian, in the investment sense of the word. That is, try working on some research projects in areas that aren't popular, that other people aren't working on. For example when I was an undergraduate we were all taught the uniformaterian slogan, "The present is the key to the past." If I had been really smart as a young man I would have immediately gone out and studied the present, but I didn't—essentially nobody was studying active tectonics in those days, even in southern California where I was a student. People thought of orogeny as something in the past. For example, they thought the Transverse Ranges behind Los Angeles formed back in the Pleistocene in what Hans Stille called the Pasadenan orogeny. But today we realize that the Pasadenan orogeny is going on full force, and we can study it with a diverse set of tools. Similarly when I first came to the Taiwan in the mid-70s people thought it had formed in the Plio-Pleistocene Penglai orogeny—but now it's obvious that the Penglai orogeny is going on full force today and that it's an incredibly fruitful thing to study. It was in Taiwan that I started to be somewhat contrarian, working more and more on things that weren't popular, like active tectonics. Being a little contrarian is actually a lot of fun and it makes it fairly easy to stumble onto new discoveries.

The most important ingredient of discovery is probably rich unstudied data. Ground-breaking discovery often requires rich data and new technology—the astronomers understand this very well. I've often been attracted to rich unstudied data. When I started to realize that petroleum companies had excellent data that academic structural geologists weren't working on, it was fairly easy to stumble onto new insights. This is what fueled the discoveries in fault-related folding, growth strata and borehole stresses. And I've recently moved back to Taiwan in part because it has become one of the best-instrumented mountain belts in the world. One kind of data I'm really excited about right now is new very high-resolution crustal and upper mantle tomography under Taiwan produced by my colleague Yih-Min Wu—this is giving us an amazingly detailed 3D image of what's happening under Taiwan. For example, you see ribbons of crust extending down into the mantle under Taiwan. We are probably seeing ultra-high pressure metamorphism taking place today. And it really takes experienced tectonicists to understand such data, people who understand outcrop geology, who think about processes, and who think palinspastically and historically.

Finally, it's often useful to think of new research interfaces. Try looking for separate disciplines or subdisciplines that can be fruitfully brought together. For example, I've been interested in the interface between crustal earthquake seismology and structural geology. This is a very natural marriage of fields in principle because upper-crustal deformation is dominated by slip in earthquakes. This is a field that is really starting to move in a number of fruitful directions. Similarly when I was Chair at Princeton I became convinced that research at the interface between low-temperature geochemistry, microbiology and molecular biology was really ripe for progress. So we started to hire faculty in this area and it has been enormously fruitful.

I should wrap this up by saying that thinking about what makes our science successful at moving into new fields is very important. That's what ultimately leads to new subdisciplines and new excitement. It provides exciting research opportunities and indeed fruitful employment for ourselves, our students and our colleagues.

Finally, I would like to thank all those, like John Shaw and all my former students, postdocs and collaborators, and my fellow structural geologists like Eric Erslev, who have shared this with me. It's a fun career with a lot of great people. Take a look at our new web pages at the National Taiwan University to see many of my current and former students and friends and what's going on in Taiwan (http://suppelab.gl.ntu.edu.tw/). We have a growing international research group and Taipei is a fun city with great food. And finally I want to sincerely thank all of you in the Structural Geology and Tectonics Division of the GSA. And sincere thanks to Eric Erslev and John Platt, and to John Shaw and others who nominated me for this award.