Geological Society of America
Structural Geology & Tectonics Division

2006
Career Contribution Award

Presented to John F. Dewey

Citation by Celal Sengor

Today, we are here to celebrate the career one of the greatest geologists of our times, who, during the last three decades of the twentieth century, has put his stamp on the tectonic interpretation of Earth's behaviour. How fortunate it is for geology that he is still active and, by all appearances, is likely to remain so for years to come. It is an immense honour for me as a fellow geologist but, even more so, as his pupil, to present Professor John Frederick Dewey to you as the career awardee of the Structural Geology and Tectonics Division of the Geological Society of America this year.

Fortune favours the prepared mind. John's career as a structural geologist was about a decade old when plate tectonic theory burst upon the geological science in the late sixties. He read geology at Queen Mary College and obtained his doctorate in Imperial College with a thesis on the Ordovician and Silurian rocks of western Ireland. Although not his thesis advisors, Dewey has always regarded John Ramsay and Janet Watson as his most important teachers and mentors.

In the late sixties, few geologists grasped the significance of plate tectonics because a broad view of the geological behaviour of our planet was the first necessity. In the sixties, there were a number of such geologists with an encyclopaedic knowledge of global geology, yet not one of them became a John Dewey, because they lacked the other, in my view the more critical, component of a broad world-view of geology: A critical rational approach, i.e., to dare to ask the question: What ought it to be like? Such a question had long been anathema in twentieth century geology because of the prevalent silly Baconianism. As Tuzo Wilson wrote, 'more geological mapping was both the method and the aim of geology' in those days.

Indeed, when I started my geological education, I was instructed to learn 'the basic principles' first and then be ready to question the data. However, my previous reading in the history of geology had taught me that those very 'principles' that I was advised to learn (implicitly, without questioning) were the mistakes of tomorrow. When I met John Dewey, after my first year in geology through a short course he was giving together with Walter Pitman, I instantly recognised a teacher who not only allowed, but actively encouraged, questioning even the most basic 'principles.' On the day we met (10th June 1975), I told him, as a freshman, that I thought his model for the tectonics of Turkey as published in the classical 1973 Dewey et al. Alpine System paper (GSA Bulletin) was wrong. John said "I know, but tell me how we can correct it." This reaction, pronounced with a genuine interest and smile during lunch, led to much scribbling on several paper napkins and, during that conversation, John conceived the idea that the Aegean grabens might have been created by east-west shortening and offered to write a paper with me, which appeared in 1979 in the GSA Bulletin. What really impressed me was John's incredibly quick and inquisitive mind, his genuine love for and determination to seek the truth and his generosity and kindness towards a freshman. I decided that I must continue my studies with him.

That decision turned out to be the most important and the luckiest I have made in my life. To this day, I have been a constant beneficiary of John's kindness and generosity towards me. I probably owe him almost as much as I owe my own parents. His generosity and kindness to all his students and colleagues have been no less.

When the kinematic theory of plate tectonics was almost complete in 1969, very few geologists dared to reinterpret geological data from an entire mountain belt in terms of it. I know of five papers that came out in 1969 on this topic: John Dewey's on the Appalachian/Caledonian System and on the conversion of Atlantic-type continental margins to Pacific-type continental margins, Warren Hamilton's on Mesozoic California, Hans Laubscher's on mountain-building (but essentially on the Alps) and Mitchell and Reading's on geosynclines in terms of plate tectonics. Of these only Dewey's and Hamilton's papers dealt with the motion of the plates not only to explain why the mountains were where they were, but also got into the bowels of the orogens to show us what the single lines geophysicists were drawing along convergent boundaries in reality were and how they worked to create the real geological record. Recently, I had to remind, in a book review on the Lake District in northern England, that Dewey's 1969 paper had explained the individual Lakes elements already in his 1969 Nature paper! I could have done the same for parts of Ireland, Newfoundland, and the northern Appalachians!

Once these initial papers were written, John's research forked: he continued to explore the theoretical implications of plate tectonics and he got into the field to test his and others' models. Therein we see how his critical rationalism was working. Dewey not only falsified many models by others, but also some of his own (including those dating from pre-plate tectonics days from the British Isles). Initially, for example, he thought ophiolites could glide down as gravity nappes. After work in Newfoundland with his students and visits to many ophiolites in the world, he changed his mind. In fact, his team's ophiolite research created such a sturdy edifice, that much of what is going on now on ophiolites is icing on its cake.

John spent the early seventies exploring plate tectonics in many mountain belts and, together with his colleague and life-long friend Kevin Burke (another awardee of this section), in rift valleys, along continental margins and on continental plateaus. These years saw the birth of the still-used models of uplift-generated triple-junctions on plume heads, of Tibetan-type plateaus in continental evolution, of cracking continental plates along complex zones of deformation. Through these studies, John reached a conclusion that horrified both him and those who read it and tried to come to grips with it. He documented, in an ingenious paper in the John Rodgers (another awardee of this section) volume of the American Journal of Science in 1975, that plate tectonics must destroy geological evidence on such a scale as to render unique reconstructions of the past impossible! Anybody who understood the reality of subduction would have guessed that, but John showed, on hypothetical worlds masterfully draughted on Wulff-nets how a continuously-evolving network of plate boundaries must behave and which kind of evidence would get destroyed in what sequence and at what stage of plate boundary evolution. This kind of rigorous analysis, while forcefully driving home to geologists that they cannot hope at the end of the day to be all-knowing, rescued them from despondency by showing them what systematic clues they can hope to find to fill the gaps, albeit hypothetically, that open up during plate boundary evolution. John has repeatedly emphasised the chance aspect in geological evolution. Anybody who has not read John's 1975 Am. Jour. Sci. paper and its offspring his 1976

Tectonophysics paper is at a serious disadvantage in interpreting geological history in terms of plate tectonics. John showed that, while much evidence is lost, history may still be testable as much as physics is, and that geologists must strive to erect testable hypotheses to reconstruct the past.

In the middle and the later part of the seventies we see John, with his colleagues, getting into the Precambrian (which he had already touched in 1969, with Kevin Burke, in a paper on the reinterpretation of the Pan-African 'tectono-thermal' event of Kennedy, which first appeared in 1972). They showed that the naive interpretation of the greenstone belts as little deformed synclines was hopelessly wrong and resulted from not appreciating how the structures of the Phanerozoic orogenic belts had been unraveled by a judicious combination of detailed biostratigraphy and structural geology. In the Pre-Cambrian, the lack of biostratigraphy had crippled structural interpretations much more than most Precambrian geologists seemed to have recognised. John took a position akin to that adopted by Eduard Suess a century earlier; he was willing to be actualistic but without losing sight of the fact that the terrestrial globe had an irreversible history. Today, Precambrian, especially Archaean, tectonic research rises on the pillars that John and Kevin erected.

In the eighties, John returned to the more detailed structural evolution of the orogenic belts and considered arcs, collapsing orogens, and "terranes." About terranes he initially had a most tolerant approach, adopting graciously the terminology of those who reinvented what already Tuzo Wilson and he had clearly said in the late sixties and the seventies. My fellow students from our Albany days will recognise that those papers fundamentally say nothing that we had not been hearing in the mid-seventies in John's lectures. When terranology became an end in itself, he revolted. The papers I wrote on that subject and those that we co-authored were all written in close communication with John. Later, his interests became concentrated around complex strain histories and they culminated, in 2002, in his masterly analysis of transtension. Here we see one of the best examples of John's method of approach to geological problems. He first lays out all the theoretically possible aspects of a problem, then takes individual geological objects, such as hand samples, outcrops, entire orogens, and tests the models using observations. Observations inspire further generalisations, correct errors, and lead to further questions. Then, he returns to the drawing board and tries to answer the questions first theoretically, laying out the basis for the next field-checks by modifying the original model, the iterative, networking, approach.

Most recently, his research has centered on 3-5 Ma transtension along the eastern side of the Sierra Nevada and the pre-Carboniferous history of the US Cordillera west of the "706" line, where he takes the superexotic view that all terranes with pre-end Devonian deformation originated in the Appalachians. He has also been mapping and describing mega-boulder deposits generated by freak waves and tsunamis, especially in New Zealand and western Ireland.

He has never been seduced by the deceptive numerical pseudo-precision of simplistic physical models derived from the application of elementary engineering concepts to geology. He has long warned against the bogus air of precision that one may obtain by ignorant application of ideal models, developed on unreal objects and for unreal circumstances, to real geological objects and processes evolving in inscrutable complexity in the abyss of deep time. He has been rightly intolerant of those producing numbers from either computers not tied to field reality or samples collected in the absence of a carefully-constructed geological map. While we were his students, he allowed none of us to obtain a degree without making a detailed geological map. Later, he allowed those with physical handicaps or of a more geophysical bent to do so but, even then, he made sure that they studied and understood geological maps und used them in their work. For John, geology is the ultimate natural science and unforgivable that a geologist should adopt the methods and theories of only one of its hand-maidens such as physics, chemistry or engineering.

I could go on and on about John the geologist, but time fails us. He is far greater than the limits of a single citation could possibly read. Without him, the geology of the latter half of the twentieth century would have been very much poorer.

Of the man John Dewey, I wish to say much, but I am deeply biased, as he and his wife Molly have always treated me as an older son and my affection, respect and indebtedness to them both are boundless. However, as no son should be barred from speaking about his father, so no grateful student should be prohibited from talking about his mentor. In John Dewey, all his students have found a wonderful, concerned and engaged teacher. His ability as a teacher and as a lecturer is legendary. His readiness to drop everything to answer a question has always impressed me. One day in Albany, when I asked him why he used a certain size Rotring pen while draughting a certain line, he dismounted the entire figure from the light table, walked across campus with me to the only reducing xerox machine we had on campus just to show me what it would look like when reduced! This reminds me what a superb draughtsman John is. He draughts all his own figures, now in Adobe Illustrator, and has always insisted that, when writing a paper, one should always first draw the figures: Of geological objects and processes, he was fond of saying 'If you cannot draw it, it does not happen!')

There is no more affectionate and considerate friend. A model family man, he invited me, after I had met him in Maryland in the Summer of 1975, to stay with him and his family during the coming Christmas. While the presents were being unpacked, he noticed that I had no present. He walked up to his bookshelf, picked up a rare 19th century geology book from his collection (T. Mellard Raede's, The Origin of Mountain Ranges) and handed it to me saying 'And this is your present!' I shall never forget that gesture. John has been a great teacher and a mentor to all his students.

I have often written that top scientists very seldom make good teachers. Dewey is one of those rarities. Not only is he a superb lecturer, a great discussion partner, and an inexhaustible well of information, but he has that great knack of making his students discover things for themselves. One evening in the mid-seventies, I remember his telling Gary White, who had just arrived in Albany to become one of his graduate students, that he did not inflict help upon his students. He has always refused to spoon-feed us. As a graduate student, one had to come up with one's own research topic and to make it acceptable to John. This was tough. Even tougher was the complete freedom one enjoyed as his graduate student. John laid down no guidelines in research, although he was always available with advice if asked. However, he encouraged his graduate students to talk to each other (we do, to this day!) If one was able to stand all that, one became an independent researcher in one's own right and not a Dewey clone. John has had 56 graduate students who, except for two who have sadly died, are now distinguished geologists across the globe.

Colleagues, I present to you, with much pride and immense satisfaction, this year's Career Contribution awardee of the Structural Geology and Tectonics Division of our society, Professor John Frederick Dewey.

Response by John F. Dewey

I am deeply honored and touched by this award. Celal has been most generous, but I am approaching 70 and "retirement" is looming in the not-too-distant-future. The career contribution award suggests thank you and goodbye, but I promise that I will keep on doing field-based geology in structure and tectonics, but I do have some new interests in the deposits of tsunamis and freak waves mainly in New Zealand, Ireland, Aruba, and Cyprus.

First, the negative. I will outline some of my profound concerns about the present state of Geology and academic life. I may be considered a grumpy old man but I can say whatever I choose at my age because I am seeking neither a job nor NSF funding.

EarthScope, IRIS, and programs like them are expensive boondoggles. Random data collection, of course, is always useful as would be a proposal to map the whole of Africa at the 1:10,000- scale, but it is not the way to do science. Actually, any surface random data collection like Quadrangle Mapping is much more useful than EarthScope because of the small filter size and direct access to rocks. It is peculiar how large-scale geophysical random data collection, which homogenizes at a very large pixel scale is considered to be superior to geological mapping at a small resolved pixel scale where one can actually see the rocks. Large expensive programs, driven by program managers and geopolitical activists, are, generally, appalling and costly nonsense. At an early stage, we needed less sycophancy from the geological community, such as "how can we adapt and use EarthScope to our geologic advantage" and more straight talk such as "please stop this nonsense and put all that cash into the responsive mode where all funds should reside." It is not too late, but important that geologists now inject some science into the program.

What started out as a new and vibrant marine geophysics that completely changed our geo-world in the 1960's has been used to denigrate and diminish the central and critical role of geology in the Earth Sciences. Geology is becoming like a puffball; the core of the science is rotting out inside a thin hard shell of the avant-garde and fringe. There is more than one Professor of Geology who has never made a geologic map, looked down a microscope, or studied rocks, minerals, fossils, or seismic sections, or logged a core. Classic field-based observational geology is being squeezed out.

Microscopy and optical mineralogy are being phased out and students are not taught to map properly and make field observations. How can one do serious petrology without optical mineralogy? The Universal Stage is a powerful tool in petrography yet is now scarcely taught or used. Whole departments are ignoring the fundamentals, and undergraduates who want to study geology are being cheated. The future of geology is now at serious risk because the young are not being properly trained in the basics, especially in the field. Francis Pettijohn said "The field is where the truth resides; rocks do not lie, and there is nothing as sobering as an outcrop." Field geology can be intellectually and physically very demanding, sometimes hot and sweaty or freezing and wet but without it, a resulting map, and observations of rocks, minerals and fossils, nothing much useful can be done.

Another problem is the seductive pseudo-precision and accuracy of numbers that come out of machines. Simple basic geology 101 tells us that the Sierra Nevada went up in the late Cenozoic, yet new and untested geochemical arguments are used to counter this. The established stratigraphic position and order of Ordovician sediments in western Ireland are challenged by zircon numbers with no micro-mineralogy or serious discussion of lead loss/gain. We have been seduced by and begun to believe implicitly in the sometimes bogus results of some of these methods. Rb/Sr was once considered the "bee's knees" of geochronology but is now realized to be almost worthless. Quantitative mensuration methods are important but have to be weighed as a component of all the evidence rather than considered to be the definitive truth. Numerical modeling is important in constraining ideas but is not the touchstone of veracity.

I am not suggesting that only hard rock field-based geology is worth studying. To understand the Earth, its processes and evolution, we need everything from all kinds of observation, experiment, numerical and analog model building, analysis, synthesis, and lateral thinking. My complaint is that the techniques of the core of geology are being progressively reduced and eliminated in favor of trendy and probably ephemeral topics. Environmental geology is a buzzword that conceals a lot of shallow and poor science; as Kevin Burke once remarked "I am an expert in this area, I have lived in the environment for seventy years." The ultimate piece of nonsense is astrobiology/exobiology, the only subject that I know that has no observational base and no material. Its rationale seems to be an excuse to study the Archean and the origin of life (why find an excuse?) and plenty of NASA money.

I am deeply concerned about the modern university obsession with accountability, assessment and review, but only of course of academics not administrators. The intellectual tradition of scholarship is decaying as the corporate business mode takes over with all its attendant money- based decision-making. The faculty, who perform the basic and essential university mission of teaching and research are paid substantially less than administrators. The recent history of some major universities involves devious and secret actions in wasting public money at the highest administrative levels; partners hired and given newly-defined and highly-paid jobs, massive funds spent on upgrading housing, expensive sabbaticals taken just before retiring, being fired, or relocating, exit golden handshakes, and secret unaccountable housing loans given to un-named individuals. The wasted money of "hands in the till and noses in the trough" is of less concern than the arrogance of putative importance and entitlement shrouded in secrecy. This kind of stuff, of course, is not available to academics and has to be stamped out. Faculty need to take over universities again; administrators should obey their instructions. We are in trouble when Chancellors, Vice-Chancellors, Provosts, and Presidents think of themselves as top dogs and CEO of their institution in the corporate business mode.

The overhead is a drug to which administrators have become addicted; it gives Chancellors and Presidents slush funds. Grants and overheads are corrupting serious scholarship. The overhead pours in, the faculty who generate it are rated by their ability to obtain it, while administrators, who do not and cannot generate it, cream it off to spend it in unaccountable ways. They are even beginning to tax research gifts to department and individuals.

The NSF funding system, especially in Structure and Tectonics, is moving away from the field base. It is no different internationally; the system is run largely by people who do not go into the field and have no sense of or interest in the field base of geological reality. Funding goes to safe research that has already been done, and to the ongoing support of large laboratory systems in which the funding agencies have an investment and a vested interest. The risky, innovative and clever is doomed to grades of good and very good, the kiss of death. Funding commonly goes to research that has already been done. I have thought for some time, and for many reasons, that the geo-funding activities of NSF should be transferred to the NAS, where they would be awarded to the best and most original research. The NSF Program Manager position, in its present form, is a position that should be disbanded in that managers have too much power to influence and steer, if not direct, the kind of science that that they regard as important. Program Managers do not have a fatidical and exclusive knowledge and understanding of what is and may become important. Everything is important. The proper way to proceed is for all the money to be in the responsive mode, for the panels to consist of the best people, and for the panels to make decisions, not recommendations, that the Program Managers administer.

To the young, I say "take up the challenge to preserve geology and our universities if you care about them." You have the power through your faculty senate, to take charge. Don't get sucked into the system; remain uncorrupted but remember "the ruling clique in the funding system and administrations may try to get you through funding, tenure, and promotion. Your university does not care about scholarship and what research you are doing; they are concerned mainly with the overhead, the number of papers that you have published in refereed journals, and external recognition through medals, awards, and prizes. Your promotion and tenure depend upon these factors while only scant regard is paid to university service, teaching, and serious scholarship. Universities should be about scholarship, a semi-forgotten word that means academic achievement and learning at a high level, exemplified par excellence by my citationist and many of my students. There have been great scholars who have spent many years developing a fundamental piece of research while publishing little or nothing but teaching superbly at the highest intellectual level. Such people would not have a chance in today's universities. Vertebrate paleontology, for example, is a field that demands an immense amount of work before something sensible can be published. Universities have to change the way in which faculty are assessed for tenure and promotion to promote scholarship rather than the present slavish dependence on an absurd algorithm.

Another problem is the scant attention paid to the literature and history of geology. The vulgar modern trend is to search and refer only to the digitally available literature of the last five years. Consequently, there is much "rediscovery of the wheel," commonly in elliptical or hexagonal form.

Now the positive. I am excited by some of my new research interests and students in UC Davis. Dave Benner and Tatia Taylor, top-class field geologists, have worked with me and taught me a lot about neotectonics and transtension in the East California Shear Zone along the eastern flank of the Sierra Nevada, especially in the Coso geothermal field. Frank Monastero, Director of the US Navy Coso Geothermal Program has been a tower of strength and knowledge in supporting our research, while Jeff Unruh of Lettis Corporation has generously shared his ideas and data, and Don Turcotte is a dependable intelligent counsel and pillar of strength in quantifying the

difficult in elegant and simple ways. This Coso transtensional research will be published soon and will change the way in which we think about vertical axis block rotation in both plane and non-plane strain regimes. There are so many problems in structure and tectonics world-wide. All involve an eclectic range of techniques from the thin section to the solar system but most involve field work, laboratory measurement, experiment and modeling.

The USA has been central in my life as the best place in the world to do geology. My ten years in Albany during the 1970's and the last six years in Davis have been wonderful with a small but excellent faculty and top-class carefully-selected and excellent graduate students. Both periods in the USA have generated an intellectual rejuvenation in me. I advise the young of the world to come to America to forge, at least the early stages of, their career.

Hans Laubscher, Greg Davis, Jan Tullis, Tanya Atwater, and Kevin Burke are all hard acts to follow as recipients of this award and illustrate the great range of ideas and techniques in our science. There are many Kevin stories but the simplest and most persistent is the best. If you tell Kevin something that you think is original, he will respond with "there's a lot of it about."

There have been many great people who have been important in my career for whose friendship, influence, and guidance I am deeply grateful: Janet Watson, Robert Shackleton, Chuck Drake, Teddy Bullard, Jim Gilluly, Warren Hamilton, Bill Dickinson, Bill Kidd, Kevin Burke, Hank Williams, all my 56 graduate students, who I will not list. I have been very lucky and am very grateful for receiving lots of research funding for forty six years, from many companies, trusts, and funding agencies.

I see the embers of a fire in the rise of a new generation of brilliant young field-based geologists such as David Chew, Paul Karabinos, and Alex Kisters, supported by the middle-aged generation such as Mike Brown, Peter Cawood, and Alan Glazner, and the older generation such as Art Snoke and Carl Anhaeusser, to name but a few. I have learned so much from many geologists, especially from Maria Mange, who showed me the power of high-resolution-heavy-mineral- analysis in tectonics, from Paul Ryan who is world-class at combining field-based geology with numerical modeling, and from my citationist who has demonstrated what can be done with a phenomenal memory, a keen kinematic sense, a fine analytic and synthetic ability, and a complete knowledge of the geology of the world, its history, and its literature.

The Career Contribution award suggests a career coming to its close. I have been teaching for 46 years (about 9,000 lectures, 6,000 hours of practicals/labs, 5,000 hours of field courses, a total of some 20,000 hours or 2.283 years of instruction) in Manchester, Cambridge, Albany, Columbia, Durham, Oxford, and Davis. It does not sound like a lot but try standing on your feet teaching continuously for 2.283 years. I now feel the need to give up full-time teaching. I may be coming to the end of my full-time teaching career but not of my research career. I hope to spend the rest of my life doing lots of geology around the globe in the field, skiing, cricket, watercolour painting, playing the piano, model railroading, walking, consorting and drinking fine wines with old friends, gourmet cooking, and listening to British and Irish classical music. My geology will be mainly in western Ireland, Newfoundland, Norway, California, South Africa, and New Zealand, and the topics will be arc-continent collision, mélanges, transtension, and tsunamites and freak wave deposits but, who knows, I may be seduced into any kind of geology that takes

my fancy, the only truly fundamental and the very best science. Thank you all so much for having been my friends for so many years and for being here tonight.