CHAPTER 17

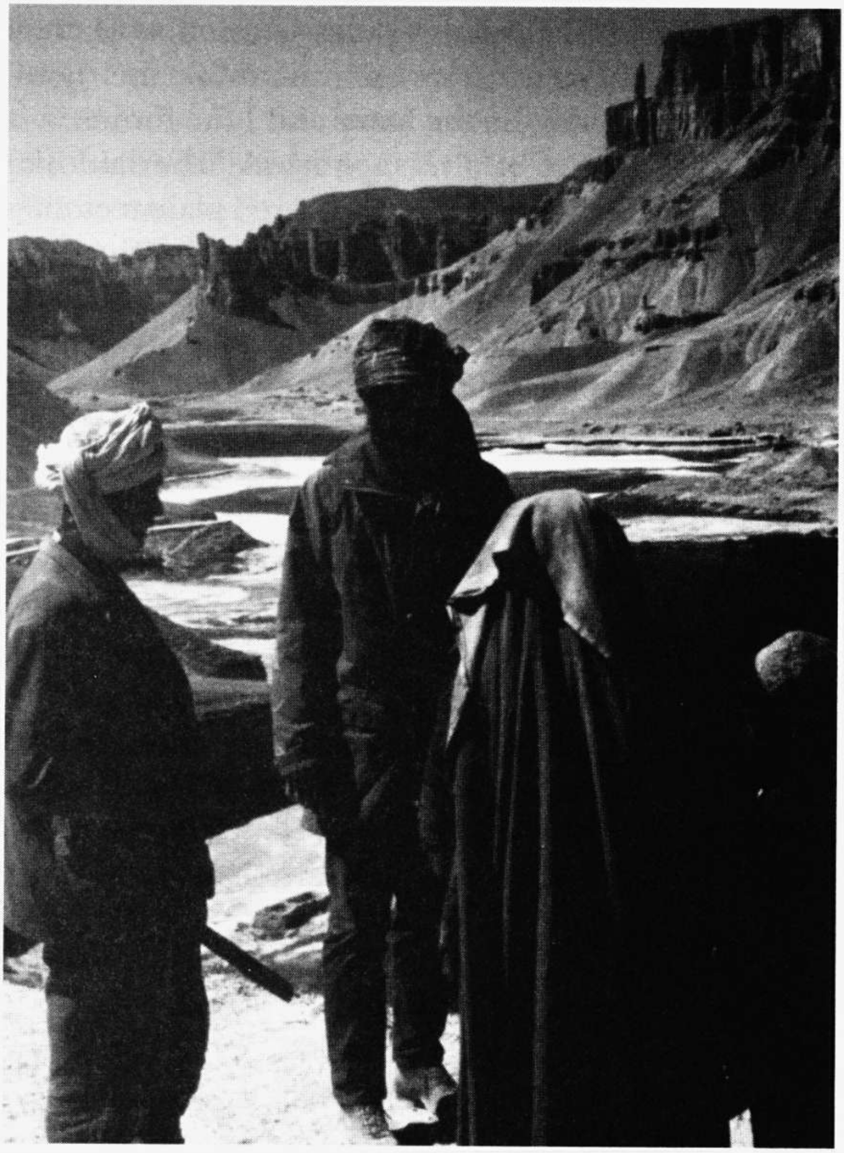
FROM PLATE TECTONICS TO CONTINENTAL TECTONICS

An Evolving Perspective of Important Research, from a Graduate Student to an Evolving Curmudgeon

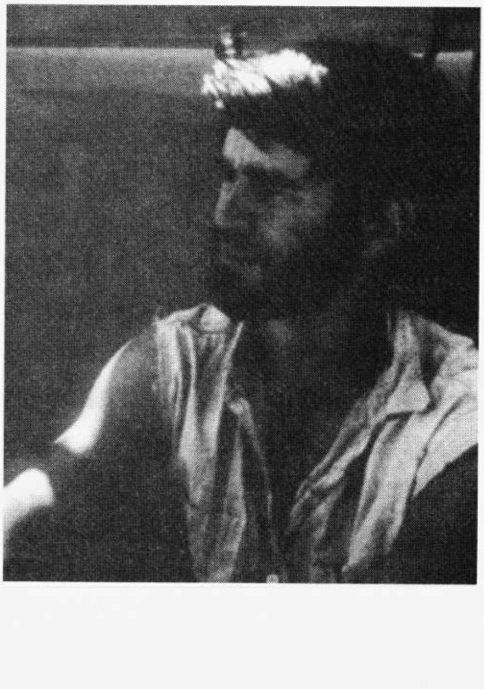
Peter Molnar

I WAS A LUCKY GUY, A GRADUATE STUDENT AT COLUMBIA UNIVERSITY from 1965 to 1970 and able to write a thesis consisting of three papers on different aspects of plate tectonics, one each with Bryan Isacks, Jack Oliver, and Lynn Sykes. Introduced to the forefront of the earth sciences as a student, how could I fail? With an excess of ambition and confidence, and armed with Jack's repeated prodding to think big and to pursue "the next, most important problem," I dismissed plate tectonics as dead in 1970 and sought a new direction. The slow recognition of failure returned me to plate tectonics as a tool (plate reconstructions), as a philosophical approach (to look first at a large scale), and as a battery of methods (mostly seismological in my case) for the study of the continental tectonics of Asia.

Luck struck again, when Paul Tapponnier introduced me to the Landsat imagery and analyzed those from Asia. If plate tectonics were a revolution, and the French one the metaphor, a reign of terror in the early 1970s made many geologists stop saying, "Plate tectonics is fine, but it doesn't work in my area," to avoid the intellectual guillotine. In 1975, Paul and I reached two conclusions that gained us some notoriety: (1) the deformation of Asia seemed to be dominated by strike-slip faulting, which suggested to us that India's penetration was absorbed by eastward extrusion of crust, and (2) Asia, and hence continents in general, behaved like



Peter Molnar, Afghanistan (Photo courtesy of Peter Molnar.)



Peter Molnar, East Africa Rift Basin. (Photo courtesy of Peter Molnar.)

a deformable medium, not like one or a few rigid plates, which rendered plate tectonics, at least in its strictest sense, a poor description of continental tectonics. Ironically, Paul and I now each minimize the significance of one of those two inferences: he the latter and I the former.

Our work might be seen as part of a metaphorical "Thermidorian Reaction"; no wonder field geologists had not discovered plate tectonics, for diffuse deformation and widespread strain makes recognizing rigid plates within continents difficult. By the late 1970s, plate tectonics seemed to have passed into earth science departments as a synonym for ophiolites and paired metamorphic belts, instead of rigid plates and angular velocities. In what may be seen as a counter-revolution, structural geologists defined a new discipline, structure/tectonics, which to a large extent disguised itself as historical geology with a new jargon. As structure/tectonics became the biggest section of the Geological Society of America (GSA), students, when asked how much displacement had occurred on a transform fault, puzzled over piercing points. So, what, after all, were the revolutionary changes brought by plate tectonics? It seems to me that plate tectonics accelerated a transition in the earth sciences from a 19th-century natural science that treated the history of the earth as an end in itself, to a 20th-century physical science focusing on a quantitative understanding of the processes that have shaped the earth.

Plate Tectonics (1965–1975)

Timing, as in a good joke, is a vital part of luck, and my experience during the recognition and development of plate tectonics illustrates well the importance of lucky timing. Having studied physics at a college (Oberlin) where teachers were paid to teach and took pride in doing it well, and where students studied more to learn than to get good grades, I was better prepared than most of my fellow students when I entered Columbia University in 1965. I was also lucky to be too young to be intimidated by, let alone aware of, how little I knew.

In 1965, options seemed limited for a physics major like me, physically fit but inept in the laboratory, insecure with mathematics and quantum mechanics, and illiterate compared with his friends. Getting drafted and going to Vietnam was much less attractive than using my training in physics to solve simple problems in the mountains, which had beckoned since my parents introduced me to the Rockies when I was only 9 years old. When I sought advice, however, my physics teachers asked, "But, what is geophysics, anyhow?" to which I responded, "I don't know, but I think it is applying physics to the earth."

The power-forward on the Oberlin faculty's intramural basketball team, Jim Powell, who also taught me a semester of geology while I sought an end-run of chemistry, advised me further, but with the admission that he did not know geophysics well. My father, an experimental physicist/administrator at Bell Labs, gave me the best advice, and on this rare occasion, I took it. His participation on the Berkner panel on nuclear testing had given him the narrow view that geophysics was little more than seismology and had introduced him to two outstanding young seismologists: Frank Press, then at the California Institute of Technology (Caltech), and Jack Oliver, then at Columbia. So I applied to both, hoping to work with one of them. Then, Jim Fisk, my father's boss and a member of the board of directors at Massachusetts Institute of Technology (MIT), leaked to him that Frank Press was moving to MIT to become the department head. Had my father not been a devout atheist, he might have quoted Huckleberry Finn, "I guess I'll go to hell," before breaking the rules of confidentiality he lived by and then confiding to me why I should go to Columbia. Although Caltech, arguably, has maintained the outstanding Earth Sciences Department in the United States since before plate tectonics, during plate tectonics, and subsequently, I consider my father's breach of his ethics, the only example in my experience, to have been another lucky break for me. Despite its pre-eminence, Caltech's direct contributions to plate tectonics lie somewhere between modest and invisible.

When I arrived at Columbia, I was also tired and in need of a rest, as so many students who have worked hard as undergraduates experience when they start graduate school. Before Fred Vine and Drummond Matthews and their hypothesis for how magnetic anomalies over the ocean floor formed were taken seriously, and before many people realized that one of Tuzo Wilson's speculative ideas (transform faulting) was actually right, I audited a course in postimpressionist painting and attended as many concerts and off-off-Broadway theatrical performances as possible, while taking what was reputed to be a full course load. Boredom gave way to more courses in the spring of 1966, but it was the fall that opened the intellectual doors of the earth sciences to me. If in 1965 Lynn Sykes had shown Tuzo to be right and if Walter Pitman and Jim Heirtzler realized earlier that Vine and Matthews had the only sensible explanation for the magnetic anomalies measured by the research vessel *Eltanin* while crossing the South Pacific Rise, I might have missed the fun.

In the fall of 1966, having gleaned more from geophysics than I had dreamed, a summer camping in the mountains of Alaska, I was finally ready to take my field of seismology seriously. Fellow student Bob Liebermann had suggested that deep earthquakes be the subject of the fall semester's two-credit course, Seismology Seminar, led by Jack Oliver and Bryan Isacks. Surely what we discussed was important to Jack and Bryan's recognition of subduction of oceanic lithosphere, although for me, unable to recognize a significant scientific problem, the mere discussion of scientific questions rather than homework problems was most important.[1] One of Jack's tricks was to get us to discuss, among the classics, some decidedly inferior papers, whose shortcomings we were to recognize before he told us what would happen if we had written them.

The seismology group at Lamont Geological Observatory of Columbia University ran another seminar, every Monday night, which was not for credit and not expressly for students, although we all learned quickly that we were expected to know what was discussed. One Monday that fall, Tom Fitch, a fellow student, told me I should stick around, because Jim Heirtzler was going to present evidence supporting "sea floor spreading." Lost, trying to conjure a sensible image of the sea floor framed in the metaphor of an expanding waistline, I stayed and learned more than just what those words meant. It was, however, Sykes' demonstration of transform faulting using seismicity and fault-plane solutions of earthquakes, presented at the Monday night seminar a few weeks later, that made me realize that continents drifted and that something exciting was happening.

When Sykes heard Wilson present his idea of transform faulting, he dropped what he was doing to test the idea; it took me three months to realize that I had better things to do than concentrate on courses. Art McGarr, an advanced student at Lamont, assured me that second-year graduate students could choose their own research problems. After another month, during which Jack Oliver told me daily that my perusal of seismograms (a seemingly aimless search for something interesting) was the best way I could spend my time, I buried my tail between my legs and sought advice from Lynn. He pointed me to a problem that eventually led to our determining the motions of the Caribbean and Cocos plates with respect to each other and to North and South America, just as Jack pointed me toward another, the mapping of lateral heterogeneity in the mantle and hence defining the lateral extent of lithospheric plates.[2] Suddenly I was studying what I wanted, not what teachers in classes expected, as my grades soon reflected. It wasn't long before Jack had to rein me in by pointing out that I seemed to be the kind who could not finish anything. I was a bit afraid of him; after all he was 20 years older than I, and therefore already in his 40s.

Lynn organized a special one-day session at the American Geophysical Union's (AGU) annual meeting in the spring of 1967 to discuss all of the recent developments that related to sea floor spreading, transform faulting, and underthrusting of lithosphere at trenches. Perhaps no single one-day session made the ongoing developments more obvious to the uninitiated than that one, and I felt a special advantage in being already familiar with some of what was presented. Experience had taught us students, however, that when the AGU's morning sessions ended, finding affordable lunch could be hard. To beat the crowds at the sandwich shops, many of us skipped the last talk in the morning. It was to be given by someone who had written a pair of papers interpreting gravity over the Puerto Rico region in terms of dynamic processes in the mantle, and those papers had elicited a critical Letter to the Editor to the *Journal of Geophysical Research* from the big guns at Lamont.[3] I had not heard of the author and remember only by vague innuendo that he didn't know what he was doing. Several years later I read these two gems by Princeton's Jason Morgan, profound and well ahead of their time.[4] By skipping out to minimize time in the queue at the sandwich shop, I missed Jason's presentation of plate tectonics, which was not discussed in his abstract. My impression is that among those who heard his presentation, only Xavier Le Pichon grasped the significance of what he said.

In the summer of 1967, Jason's paper on plate tectonics arrived at Lamont in pre-print form, and Lynn circulated it to all of us who might be interested. Suddenly the jigsaw puzzle fit together. By recognizing the rigid-body movement of lithospheric plates (which he called "crustal blocks"), Morgan gave us the missing glue that united sea floor spreading, transform faulting, and subduction (a word not yet used for what was clearly occurring at island arcs) into "plate tectonics" (more words not used yet).[5] A turning point had obviously been reached, if where next to turn was less obvious to me.

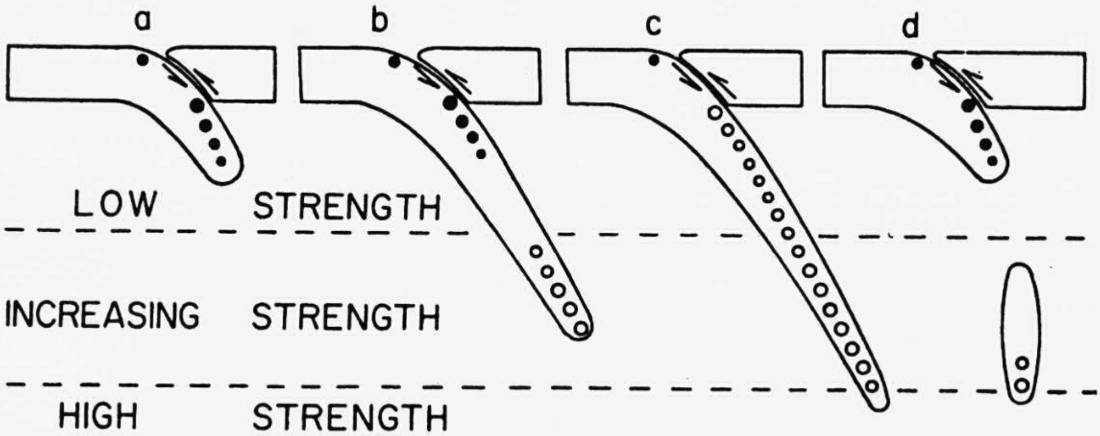
By the fall of 1967, Isacks, Oliver, and Sykes had started meeting regularly to prepare their encyclopedic summary, "Seismology and the New Global Tectonics."[6] With courses still to take and other obstacles to overcome before I could pursue exciting research full-time, I was envious. Poor Jack would have stimulating discussions with Bryan and Lynn, and then I would enter his office with the paper that he and I were writing. At Oberlin, except for a one-semester course in physical geology, I never earned better than a C+ in courses that required writing essays or term papers: English Composition, English Literature, Religion, Modern Painting, Philosophy, and European History. My non-scientist friends called me a "mere technician" and belittled my illiteracy at every opportunity. When done with a page of what would be my first major paper, Jack sometimes would hand it to Judy Healy, his secretary and my English compositional savior, to retype, because the editing had rearranged, if not replaced, most words.

By the time this paper and that with Sykes were approaching submission, what was missing was not the satisfaction of writing clear expository prose; that, like wisdom or humility today, seemed hopelessly out of reach. I sought something more immediately satisfying – my own original idea.

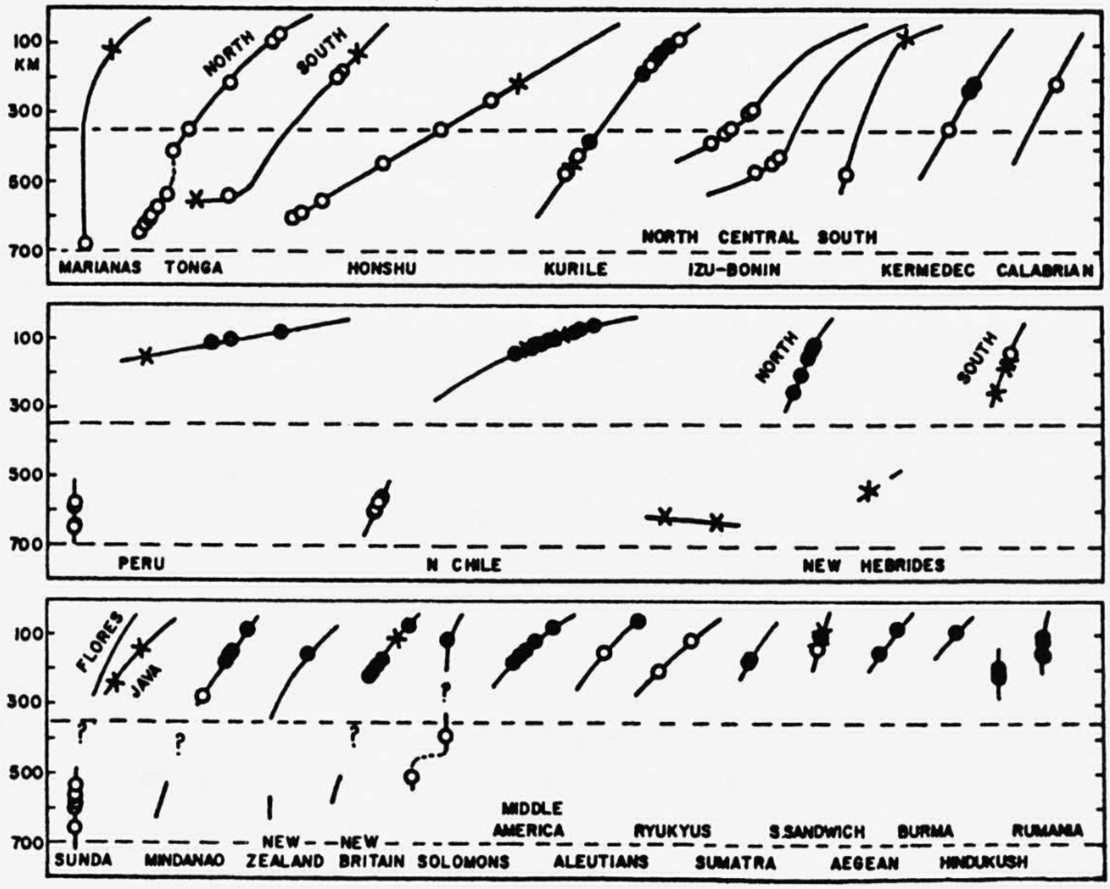
For as long as I can remember, my father, J. P. Molnar, had urged me to think for myself. He also tried repeatedly to impart some of his wisdom to me. Most of what stuck did not do so until after his death in 1973, but when Bryan, Jack, or Lynn reinforced his insight, I took it seriously. Jack could not end a seminar course (or a thesis defense) without asking something like, "What is the next, most important problem to pursue?" Those words tormented me for years, even when I thought I could answer them. Perhaps, if I had understood what made a problem "important," I would have wasted less nervous energy. In any case, whether articulated concisely or merely pervading an atmosphere, the repeated asking of such a question must surely separate institutions that make advances from those that mop up loose ends behind the forefront.

Good ideas at Lamont did not seem to be concepts that related facts and theories, and therefore the kind that often instill possessiveness in myopic egos; rather they focused on topics worth pursuing – like deep earthquake zones or transform faulting.[7] Jack once told me something like, "Yeah. Of course, I want students who can solve problems, but more important, I want students who can choose good problems." Simultaneously, Lynn was constantly suggesting that we students could find ways to test ideas, such as what drove plate motion. This two-pronged attack, of one urging a seemingly blind grope toward a good problem and of the other urging a more orderly analysis of hypotheses followed by tests, kept me alert to "important problems," whatever those words meant. Jack's book, *The Incomplete Guide to the Art of Discovery,* articulates his case more clearly than I had understood and offers advice on how to make such choices.[8] Anyhow, when I got my first good idea, to use fault-plane solutions of intermediate and deep-focus earthquakes to study the "driving mechanism of plate tectonics," I promptly learned that Bryan had already been pursuing this. A kind man, he allowed me to share the study with him, and I think I did contribute to it, although not as much as he did. Twenty-twenty hindsight suggests that Jack and Lynn may have let us share it alone, in part in deference to my palpable ambition.

We showed that fault-plane solutions of intermediate and deep-focus earthquakes defined a simple pattern.[9] First, neither of the possible fault planes determined in a fault-plane solution is oriented parallel to the inclined, deep seismic zone at an island arc. The relatively planar zone of seismicity, therefore, does not define a mega-thrust fault plunging into the mantle, as Caltech's Hugo Benioff had contended.[10] Instead, the approximate orientations of principal stresses causing the earthquakes imply that the earthquakes occur within a strong tabular body, the down-going slab of lithosphere, and result from the stressing of that slab.[11] In areas where no deep earthquakes occur (at depths greater than 200 miles or 300 km), the down-going slabs are stretched, presumably by gravity acting on the excess mass within them to pull the slabs downward in much the same way that a spring hung from the ceiling stretches due to its weight. Essentially all deep-focus earthquakes, however, suggest that the slab encounters resistance (as if the bottom of the spring rested on the floor). We assumed that resistance to reflect increased strength at a depth of 370 to 450 miles (600 to 720 kilometers). In many areas, a gap in earthquakes separates intermediate-depth seismicity from that below 200 miles (300 kilometers), which seems to mark a zone of low stress between down-dip extension above and down-dip compression below, although in some



Cartoons illustrating simple interpretations of fault-plane solutions of earthquakes of intermediate and deep-focus earthquakes. Closed and open circles signify earthquakes with down-dip T-axes and P-axes, respectively. (a) Where no deep earthquakes occur, T-axes are down-dip, because gravity acting on the excess mass of the slab pulls the slab down. (b) Where there is a gap in seismicity at depths near 200 miles (300 kilometers), T-axes again are down-dip at intermediate depths (shallower than 200 miles (300 kilometers), and P-axes are down-dip at greater depths, because the slab encounters resistance of some kind. Here, the gap in seismicity marks a zone where stress is low. (c) Where seismicity is continuous to depths of 350 to 450 miles (600 to 700 kilometers), P-axes are down-dip for both intermediate and deep earthquakes, because a rapidly subducting slab has encountered resistance at depth. (d) In some areas, a gap in seismicity might mark a gap in the down-going slab. T-axes are down-dip in the shallower part of the slab, because gravity pulls the slab down, and P-axes are down-dip at great depth because the slab encounters resistance there. Isacks, B. and P. Molnar, Distribution of stresses in the descending lithosphere from a global survey of focal-mechanism solutions of mantle earthquakes, *Rev. Geophys. Space Phys.*, 9, 103–174, 1971.



Summary of fault-plane solutions of earthquakes of intermediate and deep-focus earthquakes. Symbols as in Figure 1, with each symbol representing an earthquake that we studied, and with *Xs* showing fault-plane solutions with neither down-dip P- nor down-dip T-axes. A line through symbols denotes the cross-sectional shape and depth range of the seismic zone. Isacks, B. and P. Molnar, Distribution of stresses in the descending lithosphere from a global survey of focal-mechanism solutions of mantle earthquakes, *Rev. Geophys. Space Phys.*, 9, 103–174, 1971.

areas, the slab itself might be discontinuous. The fault-plane solutions, thus, lent support to the idea that gravity acting on the cold, dense mass of the down-going slab played a crucial role in driving plate motion, and also seemed to suggest (perhaps incorrectly) that the slab did not penetrate deeper than approximately 450 miles (720 kilometers).

By 1969, the first of our papers was published, and I felt proud of having done good work. After a presentation of this work in 1969, Seiya Uyeda from the Earthquake Research Institute of Tokyo University gave me what has impressed me as the highest form of compliment one can pay a scientist. He said, "I have to change my ideas."

Workaholism may be hereditary, but still young and untrained in the art of working all the time, I needed a rest. In 1969, I took a seven-month sabbatical to Africa to study earthquakes and intracontinental rifting there, and I failed, as I would time and again. Our results did not enlighten the process of rifting. The Tuesday following a Monday night seismology seminar in which I described my work in Africa, Muawia Barazangi, a fellow student at Lamont, asked me with that ingenuous frankness that has always endeared me to him, "What happened last night? That talk was no good." Although he said nothing after my next seminar, when I discussed what might have become my thesis, he could have said the same. So, with Jack's help in writing a three-page preface, I packaged reprints of three papers published the year before together as a thesis, and in December 1970, I defended a pamphlet whose bound cover was thicker than its pages. Although I felt smug knowing they couldn't fail me with a thesis consisting of papers with each of Isacks, Oliver, and Sykes, something was missing. In 1970, I passed from being a bright young graduate student to just another guy with a Ph.D., a manifest let-down, amplified by the concern that I could not identify "the next, most important problem." Jack had asked during my defense, as I knew he would, but my prepared answer was obviously unsatisfactory. I dropped that topic a year later.

In the late 1960s, the unsolved problem discussed most often surely was "the driving mechanism of plate tectonics." The force moving the plates is obviously gravity. If plate tectonics was a part of thermal convection, as most believed, the energy source must almost surely be radioactivity. No one doubted that subducted oceanic lithosphere played a key role, for the weight of the down-going slab was clearly huge by the standards of likely mass anomalies in the earth. Moreover, the dynamics of flow in a thermally convecting fluid must be expressed in terms of a balance between gradients in stress and *horizontal* gradients of temperature. The large horizontal temperature gradients at subduction zones again pointed to the down-going slab's role in the "driving mechanism." The question seemed and still today seems to me to be not, "What drives plate tectonics?" but "What processes and properties dictate the range of dimensions that plates and the underlying convection take?" In short, "How does mantle convection work?" which comprises more than one well-posed question. In any case, from my point of view in about 1970, the question, "What is the driving mechanism of plate tectonics?" was either already solved or too difficult for me to pursue. I sought a direction that would challenge me to learn something new, but that did not exceed my abilities.

My father had said repeatedly that nine out of ten scientific experiments failed; one had to persevere to get anywhere. I got a post-doc with Jim Brune at the Scripps Institution of Oceanography; Jim is one of those rare individuals who always seemed to find a different way to look at a problem. While reciting conventional views to Jim, and failing to appreciate his approach, I promptly learned that nine out ten ideas of what to pursue might lead to publishable papers, but they need not be important. Jack never defined precisely what "important" meant when applied to the next problem. What I was looking for, however, had gradually become Lynn Sykes' experience with transform faulting, a problem so important that it would cause me to drop what I was doing to pursue it with vigor (almost a love affair with a scientific problem).[12] Lynn did not drop everything when he first read Tuzo Wilson's paper on transform faulting; as he told me, it was not until he heard Wilson, who was renowned for exciting talks, present the idea in a talk given months after its publication.[13] Knowing that I had to be patient did not, however, prevent frustration.

Jason Morgan came to Lamont to give a talk in the summer of 1971, to present his thoughts on "plumes" before he published them.[14] We all went to listen to what was a memorable seminar. He opened with, "Consider plumes rising from the core-mantle boundary."[15] Before uttering the next sentence, of what may have been a rehearsed opening gambit, Xavier Le Pichon blurted out loudly from the front row, "Why?" Somewhat flustered, Jason said, "Well. (pause) Just consider it," but Xavier insisted that Jason give a reason for considering what seemed so preposterous an idea at the time. I sympathized with Xavier and was not inclined to drop everything to pursue this idea. That inclination came a year later, however, when evidence had mounted to suggest first, that "hotspots" – isolated centers of volcanism like Yellowstone or the islands of Hawaii or Iceland, which are the surface manifestations of Jason's plumes – might define a fixed reference frame, and second, that localized anomalies in seismic wave propagation at the core mantle boundary might lie directly beneath some hotspots.

I was delighted to have a problem that might allow me to drop everything, but my efforts to detect anything special at the core-mantle boundary failed. My father would have been happy, for I was learning an important lesson: "most experiments fail." Later, Tanya Atwater and I, then working at Scripps, concluded that the hotspots were not fixed.[16] That conclusion might not have been justified by the data in 1973, but it still stands, in my opinion, with much better data and a more rigorous treatment of uncertainties.[17] If Jason Morgan's paper on plate tectonics had seemed like pieces of a jigsaw puzzle falling out of the sky and into place,[18] resolving plumes and understanding hotspots seemed like assembling a jigsaw puzzle of fluid pieces; they could always be made to fit, but they did not always hang together.

More than once when I groped for a good problem and floundered, Jack Oliver told me, "The worst thing you can do when you can't think of anything to do is to do nothing." After recurring failures to identify a problem that would make me drop everything I was doing to pursue it, I took Jack's advice and returned to what I knew how to do, to determine fault-plane solutions of earthquakes. By the early 1970s, we students and staff at Lamont had studied nearly all regions where fault-plane solutions of earthquakes could resolve plate motions, but what had been ignored were the continents. Dan McKenzie had recognized this omission a couple of years earlier, and during a visit to Lamont in 1968 he began working on the Mediterranean region before returning to Cambridge, England, to complete this study.[19] I chose Asia and promptly learned that Tom Fitch, who had gone to the Australian National University in Canberra for a post-doc, and Francis Wu, of the State University of New York in Binghamton, had also begun work on the same area. When we recognized this, we joined efforts.

My study of Asian tectonics began as a back-burner project and remained so while I was at Scripps, still hoping to find something so important that I could drop everything else. My growing self-doubts about finding such a problem were strengthened when Dan read our paper.[20] We argued that, in contrast to plate boundaries, the consistent parameters in Asia were the approximate orientations of the principal stresses acting in the regions of the earthquakes, or more precisely the axes of principal strains,[21] not the orientations of the two perpendicular planes, which had been so useful in determining the orientation of relative movement at plate boundaries. Having used slip vectors to demonstrate plate tectonics, McKenzie asked how we could regress to old ideas about stress, after plate tectonics had shown the slip vector to be the important parameter.[22] One always takes seriously what Dan says, and he had valid points, which I will not discuss here, but the data showed a regionally coherent strain field (although we foolishly used the word *stress,* not *strain*).

In 1972, John Sclater left Scripps and went to MIT as a professor, in a step to strengthen MIT's program in what we now call geodynamics. As at Scripps, excellent work had been done at MIT in plate tectonics, but as much by graduate students, such as Don Forsyth, Norman Sleep, and Sean Solomon, as by the staff. John persuaded Frank Press that it needed another marine geophysicist, and the outstanding candidate was Tanya Atwater, my partner at the time. In effect, they had to hire me too. (Protestations that I had actually been hired on my own merit were far too emphatic to be believed.) Before going to MIT in January 1974, I wrote two proposals to the National Science Foundation (NSF), one to study large earthquakes in Asia and the other to refine plate reconstructions and develop a method for determining uncertainties. The need to estimate uncertainties was beaten into me unremittingly by my father, and then after a brief respite by Tanya, although she should not be held responsible for the fervor with which I have carried this flag. Anyhow, neither proposal addressed the next important problem, but I reckoned that both would keep me busy for a long time, and they did. Then, Tanya and I went to the Soviet Union for four months.

I had decided, not altogether incorrectly as others showed, that earthquake prediction offered important problems: not only did earthquake prediction seem possible, but precursory processes also could be understood.[23] The Russians were at the forefront, having demonstrated that the ratio of the two waves radiated by earthquakes and passing through the earth, P and S waves, varied over time, but no one had examined their data in any depth. My father would have been proud again, because this experiment failed, too. I could not find the change in the ratio of P and S wave speeds that the Soviet seismologists had reported, that made their work the center of attention in earthquake prediction, and that was subsequently found by others in the United States.[24] During periods mostly lost struggling with and waiting for the Soviet bureaucracy, I resurrected enough of my college Russian to read about the major earthquakes and geology of Soviet Asia and Mongolia and made a few lifelong friends who sympathized with my frustration. Shortly after arriving at MIT in January 1974, I abandoned almost completely earthquake prediction as a direction, with no subsequent regrets.

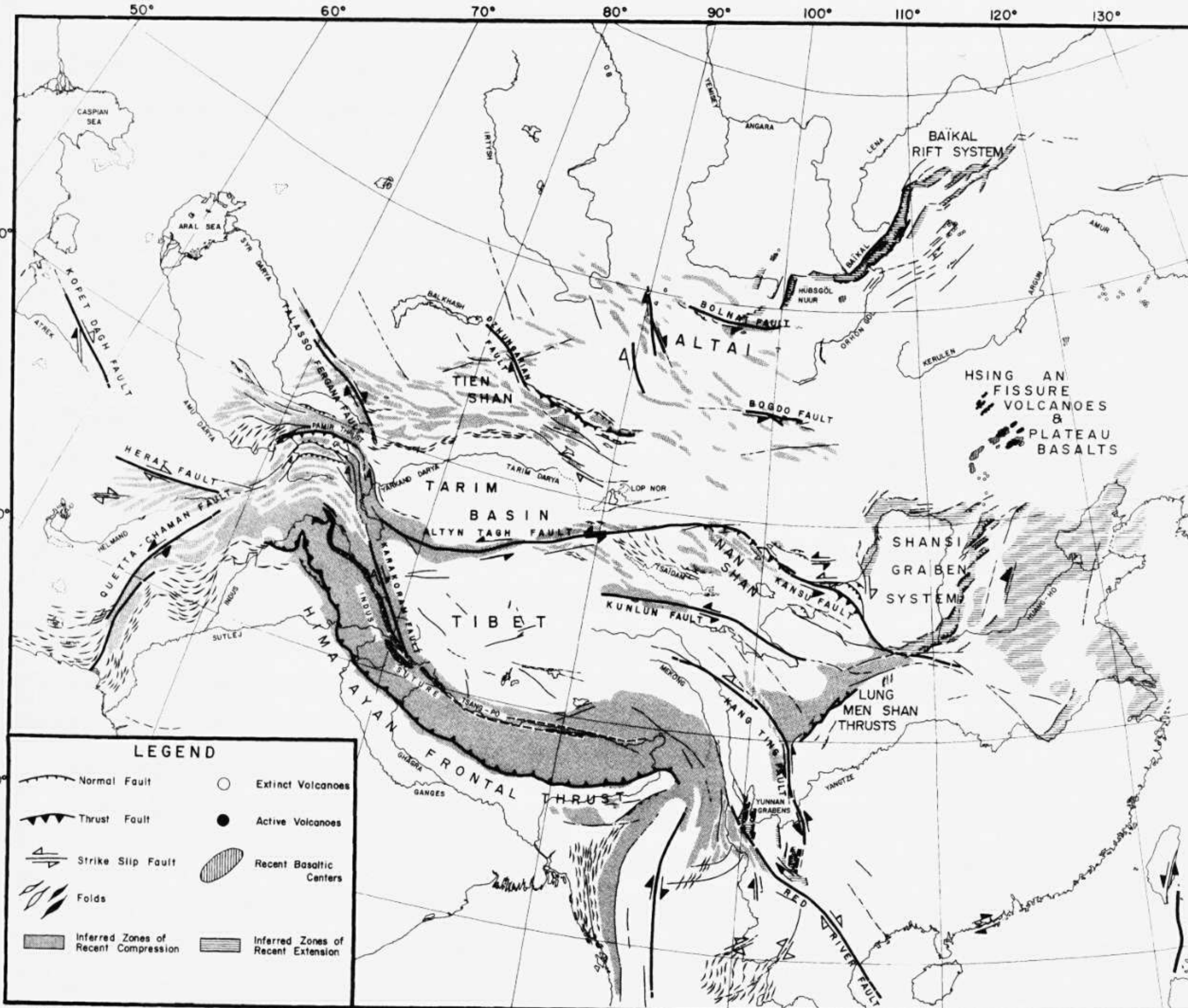
Satellite Imagery and Large-Scale Asian Tectonics

The intersection of timing and luck greeted me again shortly after starting at MIT. A special student from France, trained in structural geology but with the sound quantitative background typical of French students, had come to MIT to learn rock mechanics from Bill Brace. Already 26 years old, Paul Tapponnier planned to spend one year at MIT and return to France to begin his own laboratory work, based on what he learned from Brace. Anticipating an interest in earthquake prediction, I had asked that my office be on the same floor as Brace's. In the following weeks, the daily ebb and flow between courses and research brought Paul and me together, at first with no thoughts of collaborating. Paul's unusual intellect was soon obvious, and Brace and I thought it would be unfortunate if he left MIT after only one year. We discussed how to make it obvious to Paul that staying at MIT for a Ph.D. would be in his best interest, and Bill said unhesitatingly, "We should find him good research problems." He set Paul going on a careful study of brittle cracks in rocks, and I asked him if he would be interested in looking at the satellite imagery of Asia. The idea was less original than it might seem, because I had never heard of the Earth Resources Technology Satellite (ERTS) imagery, as the Landsat imagery was originally called, before Paul showed me some imagery from Iceland.

By September 1974, Paul had assembled mosaics of images and recognized the prevalence of large strike-slip faults in eastern Tibet. By late January 1975, we had a coherent interpretation of his analysis of the satellite imagery and my India-Eurasia plate reconstructions, fault-plane solutions of earthquakes, and reading of the Soviet geological literature.[25] (He then returned to France, having chosen not to pursue an MIT Ph.D., not live like a student for several more years, and not to pay for a large fraction of the parking tickets issued that year in Boston.) When we presented this work at MIT in January 1975, I could feel Frank Press' relief that the hiring of Tanya Atwater's partner might not have been a mistake after all. More important, I realized not only that Jack had been right about doing something when you couldn't think of anything to do, but also that satisfaction did not require dropping everything else.

Freed (briefly) of self-doubts, my definition of important problems evolved to become those that change the direction that science follows, and therefore the way scientists perceive their fields. A paper could be important if it merely addressed an important problem, without solving it. In 1973, Cambridge's (and Scripps' part-time) Teddy Bullard described to me how his study of gravity anomalies across the East African Rift demonstrated how geophysical methods could attack a geologic problem, like the dynamics of rifting.[26] He was not troubled that his conclusion, that rifting resulted from horizontal shortening of crust, was nonsense.

Paul and I showed that the deformation field over much of Asia bore a coherency that implied, in a loose sense at least, that all of this deformation was part of the same large-scale phenomenon, the collision and subsequent penetration of India into the rest of Eurasia.[27] A dominant feature of this deformation field was strike-slip faulting, in part right-lateral on northwest-southeast planes, but more significantly left-lateral on east-west planes (see figure on next page), from which we inferred that a substantial fraction of India's penetration was absorbed by extrusion of Eurasian crust eastward out of India's northward path. Moreover, active deformation seemed to occur by slip on so many faults, that it was better described as continuous deformation than by the relative movement of plates or blocks. Paul had been taking courses at MIT in plasticity and deformation processing from mechanical engineers and materials scientists and recognized immediately the analogy between faulting in Asia and slip-lines in a deforming plastic solid.[28] In short, although plate tectonics provided boundary conditions on deformation in Asia, the rules of plate tectonics provided little help in understanding, or even describing, continental deformation.



Map of eastern Asia showing large-scale structure inferred from mapped geology, the interpretation of Landsat imagery, published studies of surface faulting associated with great earthquakes, and fault-plane solutions of more moderate earthquakes. Note the prevalence of large strike-slip faults, right-lateral on faults trending NW to NNW-SSE and left-lateral on faults trending E-W. Tapponier, P. and P. Molnar, Active faulting and tectonics of China, *J. Geophys. Res.*, 82, 2905–2930, 1977.

Plate Tectonics and Continental Tectonics

Plate tectonics had an enormous effect on the earth sciences, even though only a small fraction of the field actually worked with the relevant data (magnetic anomalies and seismicity *sensu lato*). It is my own belief, however, that its main effect in the 1960s and 1970s was merely to persuade earth scientists that continental drift had occurred, and not to change how most of them carried out their science in an immediate and profound way. To paraphrase the speech-writer of a famous American president, too many geologists seemed to ask, "What can plate tectonics do for my patch of ground?" instead of "What can my discipline do to enlighten our understanding of tectonics?" Counter-examples of what were lacking do exist. For instance, geologic studies of ophiolites, slices of oceanic crust and upper mantle thrust onto continents, helped bring understanding of how the ocean floor forms.[29] Later, geologic studies of low-angle normal faulting, faults that are nearly horizontal, provided insights into how the earth's crust extends in regions like the Basin and Range Province of the western United States or the Aegean.

In the 1970s, however, a revisionist program developed to reinterpret the geologic history of regions in terms of leitmotifs ("plate-tectonics corollaries"). "Plate tectonics models" that assembled the appropriate leitmotifs for different regions were published frequently for many years. The exception that proves the rule that such studies were merely revisionist is Tanya Atwater's masterpiece that showed how plate tectonics could be used to place quantitative constraints on the history of western North America.[30] As an example of how to use plate tectonics to gain understanding of geological processes on land, her paper went unsurpassed until she revised it almost 20 years later.[32]

Insofar as plate tectonics can be seen as a revolution, its wide acceptance also brought what might be called a reign of terror. Those who quietly eschewed descriptions of the geologic history in terms of plate tectonics corollaries were treated as out of date; those more outspoken were publicly ridiculed. With belated embarrassment, I remember treating with some disdain an eminent geologist, Roye Rutland, who had worked in the Andes and later became the director of the Bureau of Mineral Resources in Australia. At the time, he seemed to know but not appreciate that the Andes formed at an ocean-continent convergent plate boundary. Only with years of hindsight did I realize that he was trying to go beyond such simple classifications to understand the processes that had built the Andes. We all often heard, "Plate tectonics is fine, but it does not work in my area," but only slowly did we realize that marine geologists, not continental geologists, were the only geologists who not only could, but should, see plate tectonics in their data.

Unlike the beheading of Robespierre in 1794 and the Thermidorean Reaction that claimed an end to the Reign of Terror in France, the swing back to treating continental tectonics as something different from plate tectonics was slow, at least from the point of view of a youth 32 years old. Moreover, for me it began with the fiercest of opposition. Paul and I had sent pre-prints out to many people, including Dan McKenzie at Cambridge University. Already a friend for several years, he seemed genuinely pleased that I had done something good, when he visited me in 1975. Yet, while walking from my apartment in Boston to MIT, as we reached the end of the Longfellow Bridge on the Cambridge side of the

Charles River, Dan said with an affirmative nod in reference to Paul's and my work, "But it is still plate tectonics." He had insisted that the active tectonics of the Mediterranean region could be described well with plate tectonics.[32] For at least the next three years, Dan argued with me that what Paul and I were saying about continuous deformation could not be right, because of a bunch of problems, some of which he himself later solved. The following year, 1976, he took advantage of an invitation to write a paper, "Can plate tectonics describe continental deformation?"[33] He seemed to argue Paul's and my case well, but concluded, "Yes, but not in detail." Imagine my surprise in 1980, when a visiting Chinese scientist said over lunch in Cambridge, United Kingdom, "How can we understand the plate tectonics of China?" and Dan replied, "You have to understand that Peter and I do not believe that plate tectonics works in continents." Although it was not long before we again had plenty to disagree about, it seemed like a chapter was finally closed.

But, no. Although no cause-and-effect applies, Dan and Paul rarely agreed, and Paul and I gradually diverged in our views, with each of us abandoning one of the two main inferences of our initial collaborative work. Paul no longer believes that Asia, or any large continental region, is best treated as a continuum; for him thick crust blurs plate tectonics in the uppermost mantle below.[34] Until 1989, I shared his belief that rapid eastward extrusion of China occurs, but no longer. My collaboration with Philip England of Oxford University beginning in 1988, overprinted on latent images articulated by Peter Cobbold and Philippe Davy of the Université de Rennes both orally and in a paper that I had reviewed and promptly forgot, made me realize that South China might move only slowly (less than 1/2 inch or 10 mm/yr) eastward with respect to the rest of Eurasia.[35] To some extent, the same questions that Paul and I addressed in the mid-1970s remain unresolved, and if our papers were "important," it cannot be because we, like Bullard and rifting, had *solved* an important problem.

Continental Tectonics Since the Mid-1970s

In the early 1970s Scripps' Bill Menard told me that one key to making progress in science is to choose a subject about which little had been written. Such a choice would obviate the scholarly ethic that requires one to read a pile of literature to get on top of the field. If nothing had been written, you could just write all the papers in the subject and automatically keep on top.[36] Studying continental tectonics was not so easy;

the literature was vast even before plate tectonics was recognized. Fortunately, by the mid-1970s, however, most of it either was irrelevant or had been assimilated into the boilerplate of common knowledge. What seemed weakly developed were a focus on dynamic processes responsible for the tectonics that we could observe and techniques for attacking these processes. Plate tectonics imparted the impetus for the former and offered tools for the latter. (Readers should be warned also that the literature on continental tectonics has grown enormously since the mid-1970s, and the review given here, from my perspective, suffers from both biases and my inability to keep up.)

The beauty of plate tectonics lies, in large part, in the simplicity with which the kinematics can be described – as rigid plates. The relative movements of vast areas can be specified using estimates of relative velocities at only a few points along the plate boundaries. Insofar as continental tectonics is best described by continuous deformation, that simplicity is lost. To describe completely the spatially varying deformation, or velocity field, within continents requires the specification of deformation or velocity at many more points, plus more complex rules than those of rigid-body motion. To appreciate the difficulty here, consider the difference between a boat moving through the ocean and the motion of the ocean itself. If we know how the front of the boat moves over the sea floor, we know quite well how the rest of the boat moves, as we do with one lithospheric plate moving with respect to another. If we know how the water in one part of the ocean moves with respect to the sea floor, however, we must exploit a relatively complicated theory to predict how adjacent water masses move; this difficulty also plagues descriptions of deforming continental regions. Dan McKenzie argued repeatedly in the late 1960s and early 1970s that plate tectonics was easy to accept because the kinematics could be treated separately from the dynamics. For oceanographers or meteorologists, separating the circulation of water or air from the dynamic processes governing that circulation is impossible, except in the simplest of situations or the broadest of generalizations. To a large extent, although many tectonic geologists might not realize it, the "Holy Grail" of continental tectonics has become understanding its governing dynamic processes.

Continental and oceanic lithosphere differ most obviously in the thickness of (low-density) crust atop each: only 4 to 5 miles (6 to 7 kilometers) beneath oceans, compared with 20 to 25 miles (35 to 40 kilometers) of continental crust. Although widely cited as the reason for continental tectonics being so different from plate tectonics, the buoyancy of continental crust may be overrated. Indeed, the negative buoyancy of oceanic lithosphere is more than adequate to carry its thin layer of crust into the asthenosphere once it has been subducted to a depth of approximately 60 miles (100 kilometers), but thick continental crust buoys continental lithosphere up. Even the coldest mantle lithosphere cannot drag with it 20 miles (35 kilometers) of crust into the asthenosphere, although if the upper crust could be scraped off the top of the lithosphere, the mantle part might carry lower crust into the asthenosphere.[37] Being inconsistent with plate tectonics dogma, subduction of continental crust had been dismissed as impossible by many, until Christian Chopin of Ecole Normale Supérieure in Paris found (by looking in his microscope, not with sophisticated instruments) the mineral coesite among what had seemed like garden-variety crustal rock in the Alps.[38] With coesite, a mineral that forms by very high-pressure metamorphism of quartz, Chopin showed that pieces of continental crust had been carried to a depth of approximately 60 miles (100 kilometers). The question of how continental crust could be subducted promptly inspired the next question, how does such crust return to the surface?

Brace–Goetze Strength Profile

In my opinion, the most significant study paving the way to understanding how continental lithosphere and oceanic lithosphere deform differently grew from laboratory measurements of how rocks and minerals deform.[39] At relatively low temperatures, slip occurs on discontinuities (faults) when friction is overcome, and therefore usually during earthquakes.[40] Such resistance increases as confining pressure (depth in the earth) increases.[41] When temperatures are high enough, however, crystals deform by the movement of dislocations within them, and strength (or viscosity) of the crystals, and hence the rock, decreases with increasing temperature. By the mid-1970s, measurements of the temperature dependence of strength (or more precisely, of the dependence of the rate of straining in the crystal on the stress applied to it) of olivine and quartz had shown that quartz deforms much more readily than olivine at the same temperature.[42] Working among scientists at MIT (such as Bill Brace, Jim Byerlee, and David Kohlstedt) who studied thumb-to-fingernail-sized specimens but also posed questions on a larger scale, Chris Goetze recognized that these different temperature dependencies of strength implied that a weak, quartz-rich lower crust underlies the stronger, brittle upper crust and overlies a strong, olivine-rich uppermost mantle.[43] This profile has often been likened to a "jelly-sandwich" (or jam-sandwich in Britain), with the attendant images of the slices of bread sliding with respect to one another and even with jelly being squeezed out between the two stronger bread layers. Where the crust is thin, as in oceanic regions, the jelly layer is absent, and the strong, cold olivine of the mantle makes plates strong enough to behave rigidly. Beneath continents, however, the crust is weak at the same depths where the mantle beneath oceans is strongest.

Goetze, who died of a brain tumor in 1978 at the age of only 38 years, had taught three classes of students at MIT this key to understanding why continental and oceanic lithosphere behaved so differently. Goetze's simple profile called attention to two aspects of the lithosphere that affect how we understand continental deformation at two very different scales. On a large scale, deformation within the mantle should occur as if it were a viscous fluid. On a relatively small scale, the upper crust might deform differently from, and even independently of, the upper mantle.

The rigid-body movement of plates of lithosphere that maintain their integrity as they move over a weak asthenosphere reminds many of us of sheets of ice floating on a lake. The Brace–Goetze strength profile for continental lithosphere, for which the strongest part resides in the mantle but where deformation can occur by plastic flow, suggests a quite different homely analogy: crumbs of bread overlying warm butter, which in turn overlies viscous honey. On a small scale, the crumbs at the top can move past one another as small particles, but on a larger scale their relative movement will be dictated by the flow of the honey below, if decoupled somewhat by the butter beneath them. (Perhaps more poignant, if politically less correct, is Ric Sibson's quote, copied from a stall in the lavatory at the Mayfair station in the London Underground: "The upper crust is just a bunch of crumbs sticking together.")[44] The widespread deformation of continents can be visualized as relative movements of weak bodies, crumbs of upper crust, carried by a stronger, deforming substratum.

The conversion of Goetze's simple image into understanding, replete with quantitative predictions and tests, required a breakthrough that, as usual, came with a simple approximation. What plate tectonics taught us was not to wallow in the complexity of the earth, but to reduce complex interactions to simple processes that could be understood, *before* they were all modeled together in detail. Describing the spatially varying velocity field within continents requires knowing not only the velocity at many more points, but also more complex rules than are needed to describe rigid-body motion. Moreover, continental crust, and presumably the entire continental lithosphere, can thicken or thin when compressed horizontally. Crust thicker than normal stores excess potential energy, and therefore crustal thickening, which requires work to be done against gravity, resists convergence between crustal blocks. Correspondingly, the understanding of continental deformation, in the sense of being able to predict that deformation from basic principles, becomes an integral part of the description of the kinematics.

The breakthrough in understanding of *large-scale* deformation of continents came with the recognition that a thin viscous sheet provides a simple model for continental lithosphere.[45] Only the vertical average of the sheet's strength, and by analogy the lithosphere's, need be considered. A thin viscous sheet deforms as an extremely viscous fluid (the lithosphere is perhaps a sextillion times more viscous than honey), with both viscosity and gravity resisting deformation. Numerical experiments quantify how different proportions of viscous resistance and gravity acting simultaneously affect the distribution and style of deformation on a scale that is large compared both to the thickness of the sheet and to dimensions of crustal blocks separated by faults.

The more important of the two (dimensionless) free parameters dictating the spatial distribution of deformation of such a sheet was officially dubbed the *Argand number* by England and McKenzie in deference to an early giant in large-scale tectonics,[46] Emile Argand, but many of us privately called it the "feta-brie" number. The Argand number scales the relative importance of gravity and strength (both brittle and ductile) in resisting deformation. A large Argand number, corresponding to crust behaving like ripe brie (or, for more Norman tastes, camembert cheese) signifies a weak lithosphere, for which gravity resists deformation most. Conversely, with a small Argand number, the crust behaves more like feta cheese and hence resists deformation with little crustal thickening or thinning and forms rigid blocks. Oceanic lithosphere is much more like feta than brie. Thickened continental crust, however, needs strong surroundings, analogous to the box that contains ripe camembert, with imposed stress to keep it from flowing apart. Such widespread stretching and thinning occurs today in Tibet and the Basin and Range Province of the western United States.

Whereas the thin viscous sheet, like ripe brie, provides a useful analog for deformation of large regions, such as eastern Asia or the western United States, most geologists must work at a smaller scale. As Cambridge's James Jackson and Oxford's Philip England often said, the area that a graduate student maps for a Ph.D. thesis is no larger than that affected by an earthquake with a magnitude of only 5.5 or so. (More such earthquakes occur each year than do students earn Ph.D. theses in field geology!) At such a scale, a weak lower crust allows the upper crust and the upper mantle to deform somewhat differently from one another, and in some regions almost independently.

A weak lower crust provides insight into a number of phenomena previously considered puzzling, if not worthy of much spilled bile. For instance, in regions of widespread horizontal extension, such as the Basin and Range Province of the western United States or the Aegean Sea, inactive normal faults mapped at the earth's surface dip at gentle angles of only a few degrees and separate deeper rock metamorphosed at high temperature from (in some cases) sedimentary rock that has undergone no metamorphism and is barely consolidated into rock.[47] James Jackson and Nicky White, however, showed that faulting associated with moderate-size earthquakes (magnitude between 5.5 and 7) in such settings occurs only on steeply dipping faults, raising the question of how slip occurred to produce the gently dipping inactive faults.[48] Flow in the lower crust, below the layer where earthquakes occur, provides the obvious solution. Many, if not most, of what are mapped as gently dipping, inactive faults at the surface may mark isolated surfaces that evolved from being steeply dipping, active faults to gently dipping, inactive ones. Although they currently separate metamorphic rock that underwent prolonged, extensive deformation in the lower crust, brittle deformation, only in a last gasp, juxtaposed that rock against the unmetamorphosed surface rocks. In many cases, subsequent flow or deformation in the lower crust of the region rotated the material on both sides of the fault, and the fault surface itself making the fault nearly horizontal. (I cite no one here for explicitly stating what the last three sentences say, because my impression is that nearly everyone who has written on this subject now shares this image, but interested readers might consult Block and Royden, Buck, Hamilton, Jackson and McKenzie, Kruse et al., Spencer, and Wernicke and Axen.)[49]

On a somewhat larger scale, a weak lower crust beneath eastern California and Nevada seems to separate very different styles of deformation. Crustal extension in the Death Valley region has thinned the upper crust and created a network of deep valleys, the surface of one of which lies about 250 feet (80 meters) below sea level. To the west, the Sierra Nevada defines a relatively high range (at least by Californian standards). The crust beneath the range, however, is not as thick as we might expect from isostasy.[50] Instead, as the University of Colorado's Craig Jones and colleagues have shown, as the crust beneath the Death Valley region has thinned, the corresponding thinning of the mantle lithosphere apparently occurred in the adjacent region beneath the Sierra Nevada, rather than directly below the area of crustal thinning.[51] The intrusion of hot, low-density asthenosphere into the space where colder, denser mantle lithosphere once lay buoys up the Sierra Nevada.

I have mentioned aspects of continental tectonics not only that have evolved since plate tectonics but also that either addressed processes or provided tools to attack them. Most of us in this field, however, spend our time searching for data to test ideas. As we are now many, our papers have made this field mature, at least as far as Bill Menard would use the word. I suspect that if able to restart again as an earth scientist, he would find the volume of literature for this subject too daunting to merit his attention. Conversely, we have learned a great deal in the past 25 years.

Most tests of ideas for how continents deform have taken one of two approaches: quantification of kinematics of deformation, and imaging of the present-day deep structure of mountain belts. I turn attention to them.

Kinematics of Deformation

Long ago Isaac Newton established a precedent for exploiting kinematic observations to constrain dynamic processes. The apocryphal story of his flash of brilliance when he noticed an apple falling might suggest as much. Less known, however, is his role in discovering Kepler's three laws of orbital motion of planets and satellites. Kepler himself had put forth several "laws," most of which we would today consider New Age fantasy; it took Newton to extract Kepler's brilliant insight from his otherwise muddled thinking.[52] Post-plate tectonics continental tectonics has also relied heavily on advances in measuring the kinematics of continental deformation.

At a small conference in 1976, I tried to call attention to an elegant cross-section of the Canadian Rockies, some 100 miles (160 kilometers) long and richly detailed, by complimenting it as a "beautiful sketch." Bert Bally, then at Shell Oil, now at Rice University, and still a friend, boomed out in an annoyed tone of voice that what I had disrespected as a "sketch" was in fact "high-quality data." Bally, indeed, had had access to excellent seismic reflection profiling across the Rockies, but what his work really showed was how to make a cross-section that was accurate.[53] Although such methods were used in the oil industry, subsequent developments, particularly by John Suppe at Princeton, have transformed the process from one in which each geologist could sketch a different cross-section from the same data to one in which nearly all geologists draw the same one given the same geological data.[54] With balanced cross-sections, total amounts of shortening across (at least some) mountain ranges can be measured.

Concurrently, methods for quantifying rates of active deformation have grown steadily. In the mid-1960s, the rate of slip on the San Andreas

Fault was, at best, poorly known. Today, slip rates for virtually all major faults have been measured by one means or another (although not always with agreement when more than one method has been used). One boon to this enterprise has been the growth of Quaternary geology, once an esoteric field of study, but now a mainstream discipline. Most geologists at one time or another took a course in field geology, where they mapped an area 5 to 10 square miles or more in dimension. Today, Kerry Sieh, one of the leaders in Quaternary faulting, has no qualms about taking his Caltech students into the field to map a region the size of a basketball court, where the uninitiated, as well as the cynical, would describe most of the rock that his students examine as just layers of dirt. The combination of applying classical geological techniques to unconsolidated sediment in small areas with the development of techniques for dating that sediment has enabled geologists to determine rates of slip on faults, rates of recurrence of major earthquakes, and growth of folds. In one of his early studies, Kerry's only fossil was a bottle, although he relied heavily on radiocarbon dates.[55]

In the 19th century much of what we call geophysics was geodesy, or in more mundane terms, surveying. The geodesist's task was to measure the shape of the earth, with the underlying hope that the shape would not change, for that would require redoing the work. Geodesy did not die in the early 20th century, when many earth scientists were unwilling to believe that the earth changed shape and when techniques changed little.The 21st century, however, is witnessing a renaissance in geodesy because of the development of new techniques in the past 30 years: laser ranging to measure distances between benchmarks, Very Long Baseline Interferometry (VLBI) and Global Positioning System (GPS) to measure longer distances, including intercontinental distances, and radar interferometry to map strain at the surface. Measurements with these techniques have determined the slip rates on faults like the San Andreas, demonstrated agreement of rates of plate motion averaged over the past 2 million years with those estimated geodetically for only ten years; and mapped the complete deformation field associated with earthquakes, not just the slip along the surface rupture. It is easy to imagine that in a few years, following the weather forecast, a geodesist will appear on television in front of a map of California in order to summarize the week's strain for the interested public: "We have a had a bit of strain accumulation along the San Andreas Fault east of Los Angeles, which raises the probability of an earthquake of magnitude 6.5 in this region to *n* percent during the six months, but that patch of accumulation we described last week just north of Los Angeles has relaxed, and the risk of an earthquake there has diminished to *m* percent."

Except for radar interferometry, both geological and geodetic measurements of deformation can be made only at points, and interpolating between such localities need not be straightforward, but in a paper overlooked for many years by nearly everyone except a couple of other New Zealanders, John Haines, at what is now called the Institute for Geological and Nuclear Sciences, solved this problem.[56] When he and Bill Holt, of the State University of New York in Stony Brook, generalized the solution to include measurements of different kinds, they put the determination of the complete strain-rate field of a large area, such as eastern Asia, within reach.[57] The essential constraint, obvious for more than a century (at least, when applied in other contexts), is that strain within adjoining regions must be compatible in the sense that if one sums the deformation along any line connecting two points, one obtains the same relative displacement of the endpoints ("Saint-Venant's compatibility," to the aficionados). For example, if measurable slip occurs on a fault and that fault dies in a region of folding, straining within the folds must accommodate the slip. Even if we cannot measure the rates of deformation everywhere, by knowing the rates in many areas, we can infer it in others.[58]

The ultimate goal of determining the strain-rate field of large regions is to understand the dynamic processes. In my opinion, this goal is within reach, but still not yet attained.[59] Ironically, one of geophysics' venerable techniques, paleomagnetism, has provided a kinematic clue to the underlying dynamics of continental deformation. The uncertainties of several degrees in paleomagnetic measurements, if small enough to allow definitive tests of continental drift, are too large to provide useful bounds on intracontinental deformation across most mountain belts. Paleomagnetism, however, can constrain rotations about vertical axes, and such rotations of material with deforming continental regions can be very large, in some cases greater than 90 degrees.[60] Moreover, the amount of rotation depends on how it is imparted. Consider two situations: (1) a block rubs against its neighbors, which slide past one another so that the block behaves like a ball bearing between the adjacent blocks, and (2) a block floats in a fluid layer undergoing shear (like a twig caught in an eddy in a stream). The rate of rotation of the first is twice that of the second, if all other aspects of the kinematics are the same.[61] Although data may not yet be definitive, they seem to favor the latter mechanism for imparting rotations, as if crustal blocks are carried by continuously deforming substratum.[62]

Imaging Subsurface Structure

Most of us think that the engine that drives continental tectonics lies in the mantle. Inferences about the mechanics of that engine can be made and hypotheses tested using observations of the surface, such as relative movement among plates or the kinematics of deformation in continents. Yet, it is a rare auto mechanic who would not look under the hood of a car to learn why its engine behaved the way it did, and most earth scientists feel the same about the earth's geodynamic engine.

The engine runs by generating lateral density differences so that gravity can move them around with respect to one another, carrying other, arguably more interesting, bits of crust and mantle along. Geophysics has a long tradition of measuring the strength of gravitational acceleration – gravity for short – and then inferring a density structure from the measurements. Because an infinite number of plausible, but very different, density structures can fit the same data exactly, such an approach is doomed to failure. This fact has in no way made measurements of gravity useless, but rather it forces researchers to pose more interesting questions than "What is the structure?" – a question perhaps of interest to geophysical stamp collectors, but only rarely asked, at least without qualification, at the forefront of science.

Were the earth an inviscid fluid, lateral variations in density could not exist, for heavy blobs would sink straight down until they reached a level where the density was the same. Some mechanical process must support heterogeneities. The strength of material can support density heterogeneities, as the walls of a building support its roof. One example is the flexure of the lithosphere due to loads, like mountain ranges, placed atop it. A long tradition of using gravity to constrain properties of an effectively elastic lithosphere preceded the recognition of plate tectonics by decades, but has been developed further as data became more complete. In addition, the flow of a viscous substance requires stress to maintain flow, and that same stress field will support lateral variations in density. The earth's gravity field provides a major constraint on convection in the mantle, particularly in oceanic regions where lateral heterogeneity of the crust is small. Beneath continents, however, the gravity field is blurred by heterogeneity resulting from billions of years of evolution and supported by strength of the lithosphere. To my knowledge, gravity has placed no tight constraint on the dynamic processes of mountain-building since confirming that to a good approximation the crust is in isostatic equilibrium. Depending upon one's point of view, that demonstration occurred in the first third of the 20th century, or was already clear in the mid 19th century when the archdeacon of Calcutta, John Henry Pratt, showed with 45 pages of tedious calculation that mass must be missing beneath Tibet, and George Airy, the Astronomer Royal in Greenwich, England, infuriated Pratt by showing, in four pages of simple argument, that such a state was required by the low strength of rock.[63]

Most techniques for imaging the earth's interior are seismological, but using these images to infer variations in density is risky because seismic wave speeds and density are not uniquely related. For such images to be useful, again the right question must be asked.

In the early 1970s, Jack Oliver and colleagues launched a major program to study the earth's middle and lower crust using seismic reflection techniques, which had been developed in the oil industry to study layered, but sometimes deformed, sedimentary rock. The success of this program, which resolved controversies about dips and depth of faults in the Rocky Mountains and Appalachians,[64] stimulated similar programs abroad. One particularly successful program was the British Institutions Reflection Profiling Syndicate (BIRPS), which took advantage of the ocean surrounding Britain to shoot and record from a homogeneous surface of constant elevation. BIRPS's data are renowned for being especially clear.

Data used to address questions at the forefront of science, however, are only rarely clear (if the data were clear, the question would be answered and would instantly move back from the forefront.) Geoffrey King's 12-year-old daughter Sophia highlighted this difficulty, when she visited her father's office at the Bullard Labs of Cambridge University, where a large display of BIRPS data had been hung on the walls in the seminar room. Transparent mylar sheets were hung in front of the seismic data, lines drawn with colored magic-markers to show the inferred horizons between layers and the inferred faults offsetting them. After a tour around the room, Sophia returned to her father to announce, "Daddy, it's lucky they've drawn the faults. Otherwise you'd never be able to see where they are."

It is my opinion that the most important result that data of this kind have shown us is not the presence of some feature, but rather the absence of one. Profiles that obtain reflected waves from the Moho, the boundary between crust and mantle beneath continents, show, with only one exception to my knowledge, that the Moho is not cut by faults. This continuity of the Moho without faults cutting it may be seen as a test of the hypothesis that the mantle deforms by continuous deformation, not by faulting as occurs at plate boundaries in oceanic regions.

The most popular technique for imaging the upper mantle is seismic tomography, which was pioneered by the University of Southern California's Keiiti Aki, while at MIT, Anders Christoffersson of the University of Uppsala, Sweden, and Eystein Husebye of the Norwegian Seismic Array, but named later for the procedure used for CAT scans of the brain in the medical profession.[65] With elegant color plots, this technique has evolved to assume imperialistic tendencies in seismology and attracted many of its best and brightest young people. The lunar program did the same in the 1960s, by attracting many of the luminaries of the earth sciences at the time to study a tectonically dead moon, allowing others to discover plate tectonics on the tectonically active earth. In my opinion, seismic tomography has hired seismic reflection profiling's tailor, and now dons similar new clothes. Of course, by no means is all tomography devoid of content, but to my knowledge, it has not solved an important problem since Rensselaer Polytechnic Institute's Steve Roecker, when a graduate student at MIT, showed that low-speed material (presumably crust) underlies (and presumably was subducted beneath) the Hindu Kush (mountains) in Afghanistan to a depth of at least 60 miles (100 kilometers).[66] (He did this before Chopin found coesite in the Alps and confirmed subduction to such a depth.)[67]

In my opinion, seismology's most promising techniques for studying dynamic processes occurring within the earth will exploit the anisotropic properties of deformed material in the mantle, as revealed by the difference in speeds of seismic waves propagating in different directions. P and S waves passing through olivine crystals differ by more than 10 percent, depending upon the orientation of the olivine crystal through which the waves propagate. Because olivine also deforms anisotropically, with one set of crystallographic axes being much weaker than the others, the deformation of olivine crystals in mantle rock causes them to align with one another in such a way that measurements of seismic anisotropy can be used to infer finite strain within the mantle.

The existence of seismic anisotropy has been known for a long time. Harry Hess, who described sea floor spreading before most were willing to consider it, was one of the first to demonstrate its existence in the earth.[68] From my perspective, however, anisotropy lingered somewhere between being a nuisance and an interesting, but useless, curiosity until the late 1980s, when papers by Paul Silver of the Carnegie Institute of Washington and Winston Chan of Teledyne Geotech, and by Lev Vinnik of the Institute of Physics of the Earth in Moscow and his French colleagues, convinced me both that it could be measured easily and that it could address important scientific questions.[69] (Many others were convinced of both aspects long before I was.) In terms of understanding continental tectonics, the large magnitude of anisotropy with consistent orientations of the faster of the two quasi-S waves nearly parallel to strikes of major strike-slip faults, recorded at stations as far as 125 miles (200 kilometers) from the faults, suggests (to many, but not to everyone) that the litho-sphere deforms over a broad zone instead of being cut by a fault through its entire thickness.[70] Again, the idea that the lithosphere deforms as a continuous medium passes a test.

Summary

The Chinese commonly look back on the Tang dynasty (seventh to tenth centuries) as the pinnacle of Chinese civilization. They also divide it into four periods, Early, High, Middle, and Late, with an obvious rapid rise and slow decline. I see continental tectonics developing in a similar fashion; following a barbarian period in which students had to study geosynclines and similar woolly thinking, the Early period began in the 1970s. Many of the major unanswered questions were posed. Techniques previously not used to address geologic questions could be borrowed and applied, without (yet) the need for much development. Our field still suffered from the division into subdisciplines, like geophysics, geochemistry, structural geology, stratigraphy, and so on, whose members rarely collaborated or showed interest in the questions of other subdisciplines. To be trained as a geophysicist but to work on geologic problems was an opportunity exploited by only a few and encouraged only rarely. A switch toward a more quantitative study of problems was in progress, but the word *geosyncline* was still often heard and presumably taught.

The High point in the study of continental tectonics came in the late 1970s and early 1980s. The thin, viscous sheet entered geodynamics of continental regions. John Suppe showed how to balance cross-sections with simple rules.[71] New seismological techniques for studying earthquakes, developed a few years before, were put to use.[72] Most good research universities developed programs in Quaternary faulting and earthquake geology. Sedimentary basins became a topic of quantitative analysis. For me, there was a stimulating interaction with geologists, increasingly with Clark Burchfiel and a group of outstanding students working with him at MIT. More important, the training of students in the earth sciences began to focus on studying processes occurring in the earth, not only such that basic mathematics, physics, and chemistry became an integral part of the curriculum, but also with the result that geophysicists and geochemists learned the basics of geology. Many departments renamed themselves Departments of Earth Sciences to eliminate the distinctions between subdisciplines. When I was a student in the late 1960s, the prevailing view was that it was easier to teach physicists geology than geologists physics and mathematics. In the summer of 1996, Dan McKenzie told me that for him times had changed; it had become easier to teach geologists math and physics than the opposite.

Since the early 1990s, what might be called the Middle period of continental tectonics seems to have begun. New techniques and new approaches have been developed steadily, and elegant work has been done, but it seems to me that problems solved have been less significant than those in the earlier periods. More careful analysis with more sophisticated methods must be brought to bear on problems in order to nibble away smaller pieces of them. Some may see this as cynicism, but it seems to me that many of the same old controversies dominate research programs. For instance, to what extent do the rules of plate tectonics apply to continental deformation? Few of us have changed our minds about such issues over the past ten years. Many of us just keep designing studies to prove that what we said ten or 20 years ago is right; few studies cause us to change our minds about fundamental processes. What I have written here illustrates this; I continue to defend what I thought ten years ago. Reader, beware! The examples that I have selected illustrate what I think, not what the many who disagree with me think. Perhaps, continental tectonics will never enter a Late stage analogous to the Late Tang period; some of these controversies will be resolved, but when remains to be seen.

Lessons from Plate Tectonics and Its Aftermath

The recognition of plate tectonics and the subsequent development of continental tectonics illustrate some patterns in the development of science that seem worth noting.

The Importance of a Fresh Point of View

All three of my advisors in graduate school, Bryan Isacks, Jack Oliver, and Lynn Sykes, changed the direction of their research to make their contributions to plate tectonics. When they began their study of deep-focus earthquakes in the Fiji–Tonga region in 1964, Isacks had just finished a thesis in high-frequency instrumentation, Oliver was an established expert in surface-wave seismology, and Sykes had recently written a thesis on short-period surface waves. Pursuing the same problems for too long can cap an open mind.

Many of the key scientists of plate tectonics, not just my advisors, changed directions again shortly after plate tectonics was recognized. Sykes remains a full-time seismologist, but his research has focused on earthquake prediction and the discrimination of underground nuclear explosions from earthquakes. Oliver launched the Consortium for Continental Reflection Profiling (COCORP) to use techniques developed by the oil industry to probe the lower crust. Isacks has become a geomorphologist. Similarly, Dan McKenzie has changed several times, and for 15 years his main focus has been igneous petrology. Much of Jason Morgan's research shifted to mantle plumes. Walter Pitman dropped magnetic anomalies in the early 1970s to pursue sea level changes and, more recently, the geological evidence for a great flood responsible for the legend of Noah's flood. John Sclater virtually abandoned heat flow and turned his attention to the development of continental margins.

Communication at the Forefront

I noted five events that set me forward in the direction of plate tectonics: a seminar course on deep-focus earthquakes, two Monday Night Seismology Seminars at Lamont by Jim Heirtzler and by Lynn Sykes, an American Geophysical Union meeting in 1967, and the arrival of Jason Morgan's pre-print. The AGU meeting was the only truly public affair, and what makes it notable in my experience is not what I learned, but what I missed (Morgan's presentation of plate tectonics). It seems to me that the least blocked channels for communication at the forefront of science ignore the public platform. By the time important developments reach the major scientific meetings, their offspring are well beyond the womb. By the time the funding agencies can respond to them, such developments are somewhere between maturity and senility. Funding agencies should focus less on supporting topics perceived as exciting and more on finding ways to allow individual scientists to create new, exciting topics.

The important developments in the early 1960s involved a small number of people who communicated directly with each other. While a student at Cambridge, Fred Vine benefited immensely from a seminar by Harry Hess and subsequent interactions.[73] Hess' now widely cited paper, however, had no impact; by the time people read it, they already knew its essence.[74] Vine seemed to give direction to Bullard, Matthews, and Wilson, whose open minds were receptive, but Vine and Matthews' paper seemed to have little immediate effect, except perhaps on Tuzo Wilson.[75] By the time Walter Pitman and Jim Heirtzler had recognized the significance of that paper, Vine had already completed his synthesis, in which he not only tied together magnetic anomalies from virtually all oceans, but also corrected the omission of the Jaramillo event from his earlier work.[76] Vine learned of the Jaramillo event from Brent Dalrymple before it was published, an event whose significance historian Bill Glen recounted clearly and insightfully.[77] Moreover, Vine moved to Princeton in 1965, and though he claims no credit, surely he influenced Jason Morgan, his office-mate at that time.[78]

The Earth Sciences as a Modern, Quantitative Physical Science

The beauty of plate tectonics radiated from the ease with which it could be tested quantitatively. The simple description of rigid-body motion on a sphere allowed plate tectonics to exploit magnetic anomalies, orientations of fracture zones, and fault plane solutions of earthquakes in some areas to make predictions of those in others. At first, such data confirmed predictions, and therefore plate tectonics passed these tests. Then with refinements, systematic errors in the data, due for instance to inter-arc spreading or simply to deformation with island arcs, did not refute plate tectonics, but allowed further understanding of processes within the Earth, such as the partitioning of slip into thrust and strike slip at island arcs.[79] Although the testing of hypotheses occurred in the earth sciences before plate tectonics, the development of quantitative analysis grew rapidly afterward.[80]

Continental drift, in its strictest sense, seems to have had little impact on the earth sciences before plate tectonics. "Most geologists could proceed with their research interests without much concern over whether drift theory was right or wrong."[81] My impression is that the interpretation of most geological observations would have been unaffected by confirmation of continental drift.[82] Oreskes has argued quite persuasively that geologists in North America rejected continental drift in part because geophysicists there said it was impossible.[83] Her evidence for the opinions of geophysicists is quite convincing, but it seems to me that her explanation as a whole implicitly requires believing something I do not: that geologists lacked either insight or the ability to think critically. No good scientist accepts uncritically an argument that he or she does not understand, but that is critical to his or her research. Thus, if geologists rejected continental drift because others told them it was nonsense, then either it was not important to their research or they were not good scientists.

Unlike continental drift, plate tectonics, largely through its quantitative implications, affected most subdisciplines of the earth sciences. It seems to me that whereas continental drift offered few solutions to questions asked by sedimentologists and stratigraphers, they now can understand many of their observations in terms of subsidence induced by a cooling lithospheric plate or by flexure of an effectively elastic plate. This analysis can be carried further to estimate maturation of organic material and the potential for petroleum production, because of the simple physical understanding provided by, among other processes, a cooling lithosphere. "Basin analysis" was legitimately born with traditional geology and geophysics already married.

Similarly, continental drift offered little insight into the processes by which igneous rock forms, especially since the vast majority of igneous rock forms at mid-ocean ridges; most of the rest forms at subduction zones, another region largely ignored in continental drift. Subsequent to the discovery of plate tectonics, petrologists recognized that not only the thickness of oceanic crust, but also the composition of magmatic rock could be understood. Essential to plate tectonics, sea floor spreading calls for hot rock in the asthenosphere to rise beneath mid-ocean ridges, but to cool only slightly as it decompresses, for the same reason that air becomes cooler at higher elevation. As pressure decreases, rock can melt at lower temperatures, just as water at high altitude boils at a lower temperature than at sea level. Thus, as hot rock rises beneath a mid-ocean ridge, although it cools slightly, it melts when it reaches sufficiently low pressure without an additional source of heat. Continental drift provided no clue that such processes occur beneath mid-ocean ridges and create most of the igneous rock on the planet.

Measurements of the earth's gravity field provided one source of data used to argue against continental drift.[84] At the time, such analyses treated the earth as static. As no dynamics were considered in most treatments of continental drift, except perhaps those attempting to refute the idea, the scope of problems addressed with gravity anomalies remained limited. With plate tectonics, however, the analysis of the earth's gravity field ceased to be an exercise in choosing one among an infinity of nonunique and very different structures, and became formalized into procedures for constraining processes, such as convective flow within the mantle, which affect density within the earth.[85]

Even profound insights into mountain-building lay fallow. Emile Argand in Neuchatel, Switzerland, recognized that much of Asian deformation could be ascribed to India's penetration into the rest of Eurasia, and on a smaller scale he recognized large folds in crystalline basement, but his analysis was qualitative and his ideas not easily tested.[86] As profound as it was, this work went largely ignored by North Americans for decades; I learned of it only after I had rediscovered in my own data much of what he had described. With balanced cross-sections, however, structural geologists began to quantify amounts of deformation in mountain belts. Similarly, Quaternary geologists appropriated techniques traditionally used to look at vast, ancient sedimentary deposits and applied them to thin layers of historical sediment to place constraints on rates of deformation and even earthquake recurrence. Geosynclines, sketches drawn without even a scale and based on qualitative description, gave way to cross-sections drawn without vertical exaggeration.

The common theme of these post-plate tectonic studies has been quantitative analysis with the goal of understanding. Here "understanding" implies the ability to predict from basic principles, and "quantitative" includes the concept of uncertainty. (Much numerical modeling and seismic tomography remains qualitative.) Although few of the techniques or measurements were invented after plate tectonics was recognized, and many earth scientists had taken a quantitative approach to the earth, plate tectonics accelerated the transformation of the earth sciences from a focus on descriptive classification of phenomena to understanding processes quantitatively.[87]

In an effort to inspire earth scientists, Tuzo Wilson, in what now seems almost clairvoyant, predicted that the days of geology as primarily a historical science were numbered and a new era was dawning.[88] Earth scientists would study processes by trying to develop physical laws that make testable predictions. How Wilson could see this when he did baffles me, for much time elapsed between the recognition of plate tectonics and the permeation of quantitative hypothesizing and testing into the various subdisciplines of the earth sciences.

Although computers have opened new dimensions for experimental science, all too often, numerical modeling seems more to be numerical masturbation, the ejaculation of color plots summarizing simulations of complicated, "realistic" models, perhaps best likened to geophysical Barbie-dolls. My father taught me that "the most beautiful sight to an experimental scientist is a straight line of data points," for data that fit a straight line virtually assure a simple understanding. Although the computer has created a new laboratory for experimentation in all fields, only rarely does numerical modeling follow the tradition of G. I. Taylor, the eminent fluid dynamicist at Cambridge University, whose goal was scaling laws: straight lines of measured data points plotted versus quantities controlled in experiments and scaled to reveal simple relationships.

Sometimes, I think that Wilson will turn out to be wrong, but for the wrong reason.

Plate Tectonics as Revolution, or as the Trigger for Rapid Evolution?

The history of continental drift, as an idea, makes good copy for historians with a journalistic bent, and for a public both sensitive to "revolutions" and sympathetic to underdogs. Le Grand captured this perspective: "The folk-tale of Drift is the stuff of myth and legend in which Cinderella, after years of abuse from her vain step-sisters, is visited by her Fairy Geophysicist, is touched by the Magnetic Wand, goes to the Ball and marries the Prince."[89] No drama is lost by a Greenland winter martyring drift's long-belittled champion, Alfred Wegener.

More important, many who espouse the view that a revolution in the earth sciences occurred concurrently with the recognition of plate tectonics argue that the revolutionary change was the acceptance of continental drift; few seem to see plate tectonics as little more than a version of continental drift.[90] Glen wrote of the "emergent, more complete theory, renamed plate tectonics."[91] In what I consider to be the most insightful historical analysis of changes in the earth sciences brought on by plate tectonics, Le Grand repeatedly refers to the "plate tectonics version of Drift," as well as the earlier "seafloor-spreading version of Drift" and other versions.[92] Wilson wrote that "[t]he acceptance of continental drift has transformed the earth sciences . . . into a unified science," although he had recognized that more than just continental drift was at stake.[93] Menard obviously saw plate tectonics as more than continental drift, but the subject of his book ends in 1968 and looks backward more than forward in his assessment of the impact.[94] Too many observers saw the "revolution" as merely a demonstration that continents had drifted. It seems to me, as I have tried to argue here, that plate tectonics brought a profound change in the way the majority of earth scientists viewed or approached the topic of their study. (To be fair to Homer Le Grand, he recognized that plate tectonics marked a change in how the earth sciences were carried out.)

According to historian of science Thomas Kuhn, scientific revolutions share features that characterize political revolutions.[95] Although no two political revolutions are alike, one can argue that their differences are small compared with their similarities.[96] Among the parallels between continental drift and the French Revolution, one might call the paleomagnetic work of the 1950s an emerging period of enlightenment that prepared the minds of earth scientists, without motivating most of them to pursue continental rift. The "Bastille" fell in the early 1960s, largely with developments in marine geophysics, but it was Vine's verification of the Vine–Matthews hypothesis and Sykes' demonstration of Wilson's transform faulting, by requiring both continental drift and Hess' sea floor spreading, that summarily beheaded fixist ideas (in which positions of continents were assumed forever fixed) of the "establishment."[97] Historical geology, the subject pursued by most earth scientists, was rewritten, as papers presenting "plate tectonics models" of the geologic history of various patches attracted hundreds of reprint requests. A "reign of terror" followed, for those who did not accept plate tectonics were ridiculed as old-fashioned and out-of-date. What was found wanting in such "models," however, rarely included an appreciation for the features of plate tectonics that distinguished it from continental drift. Although no Robespierre lost his head in a Thermidorean Reaction that restored moderation to the earth sciences, it became clear that those "old-fashioned" geologists neither could, nor should, see continental drift, let alone plate tectonics, in their data. Similarly, no geological Napoleon proclaimed an end to revolution and then united earth scientists by giving them an alternative simile to nationalism. Nevertheless, the rise of structure/tectonics as a dominant discipline in geology might be seen as counter-revolutionary. Despite numerous exceptions (two of which are mentioned above), structure/tectonics has focused as much on historical geology, albeit couched in a new jargon, as on discovering new principles or bringing understanding to processes not easily studied with older techniques. Only in the 1990s did the structure/tectonics section of the National Science Foundation begin funding active tectonics, the branch most concerned with understanding the principles. Finally, and gradually, a new approach to the study of the earth is emerging.

Naomi Oreskes has expressed a different view of both hypothesis testing and quantification in the earth sciences.[98] She sees both as in place when plate tectonics was recognized, and she considers the recognition of plate tectonics more as the result of hypothesis testing and quantification than as a stimulus for their growth and development. Perhaps she is right, although if so a revolutionary simile for those earth scientists motivated by hypothesis testing and the need to quantify might be the Bolsheviks in Russia in 1917. As a clear *minority* among Russian revolutionary groups, the Bolsheviks named themselves using a word that meant *majority* and then took over the country. Naomi and I do agree that the earth sciences have become more quantitative since the 1950s, and that quantitative approaches are more common now than before.

These parallels with the French Revolution pose the question: who benefited from the "plate tectonics revolution"? "For most ordinary French subjects, . . . [the French Revolution] had made their lives infinitely more precarious."[99] Beneficiaries included the owners of land, the bourgeoisie, and the soldiers, but not those whose economic plight required the greatest change.[100] "Fat cats got fatter. . . . By contrast, the rural poor gained very little from the Revolution."[101] If the demonstration of continental drift was a revolution, then, similarly, traditional field geologists gained little, for continental drift offered little insight into the solutions to their problems.

One might ask, why do earth scientists tout plate tectonics as a revolution that unified their science? First, plate tectonics is beautifully simple, and most scientists treat simplicity as a prerequisite of scientific profundity. Although no scientific, or epistemological, law guarantees that simple ideas approach truth more closely than complicated ones, simplicity is an expedient, for more people will understand and therefore take interest in a simple idea than one comprehensible only to few. Second, it seems to me that accepting plate tectonics may have been a low hurdle for most scientists. No catharsis was required, or was experienced, by many earth scientists, because the immediate impact on their work was slight. For instance, "in 1978, leaders of the Ministry of Geology of the USSR instructed all field geologists to interpret the results of their 1:25,000 scale survey [i.e., their geologic maps] exclusively in terms of plate tectonics. Almost none of the field geologists was able to do this," not because none understood plate tectonics, but because the sizes of regions mapped could not reveal plate tectonics.[102] At the same time, however, lateral mobility of the crust implied by continental drift and plate tectonics gave mountain belts and other large-scale features a framework into which more detailed studies could be fitted, which surely united earth scientists working at all scales. As the local fans take pride in a winning home team, so perhaps many earth scientists saw plate tectonics as a victory for their team.

If plate tectonics was "revolutionary," how was it so? The significance of "revolution" in history is obscured in part by historians' traditional predilection for considering wars and battles, slaughters and assassinations, and political carnivals to be the grist for their mills, instead of spiritual, intellectual, and technological developments that really have altered the general human condition. Likewise, in part, the "plate tectonics revolution" is obscured by the failure of many historians of science to look beyond the Cinderella story of continental drift. Winston Churchill concluded a book on *The Age of Revolution* by noting that "the principles which had inspired [France in her revolution] lived on . . . to play a notable part in changing the shape of government in every European country."[103] Similarly, as Wilson so precociously recognized, plate tectonics spurred (or validated) a quantitative approach to the earth sciences.[104] If, in general, "history sooner or later takes back her gifts," she seems in this case to have passed them on to a new generation of geological beneficiaries.[105] Plate tectonics was a revolution less because it guillotined existing fixist ideas, and more because it affected the way earth scientists approach the study of the earth. Its impact has been both more gradual and more subtle than most active scientists realized at the time. Perhaps, it is too soon even now to see the impact, Schama wrote: "Asked what he thought was the significance of the French Revolution, the Chinese Premier Zhou En-lai is reported to have answered, 'It is too soon to tell.'"[106]

Looking back on the past 30 years, some of the techniques that have led to quantitative understanding of processes in the earth sciences were in place in the 1960s, but many have been developed subsequently and at a steady rate during those 30 years. For instance, with Stanford's Norman Sleep's analysis of the subsidence of the Atlantic margin (carried out while a graduate student at MIT), much of the formalism was in place to study thermally induced subsidence of sedimentary basins, but nearly 10 years elapsed before this approach became widely exploited.[107] The recognition that the thickness of oceanic crust resulted from a very simple phenomenon apparently was not published until 1988.[108] Although Bally constructed balanced cross-sections before plate tectonics was proposed, their heyday waited another 15 to 20 years.[109] Quantitative methods in metamorphic petrology developed largely in the 1970s and 1980s. In my field of continental tectonics, the most controversial topic 25 years ago was framed by, "Can plate tectonics describe continental tectonics?" and remains unresolved in the minds of many. My impression is that seismic anisotropy, a topic that has blossomed in the past ten years, but is hardly new, will resolve this question.

Revolution was a popular concept in the late 1960s. Presidents John Kennedy and Lyndon Johnson led us Americans into a war we did not want to fight. Anti-Soviet propaganda was too vehement to be believed, at least by idealists. Who could not be revolted by the recurring injustices to African-Americans and other minorities? Although hindsight now clearly exposes previously latent images of the fundamental roles played by established giants in the earth sciences before the 1960s, many of the founding fathers, and the mother, of plate tectonics were young, less than 35 years old, when they wrote their widely cited papers. To many of the youth in the 1960s, revolution seemed like a plausible solution to the world's ills. When Kuhn wrote a book about scientific revolutions, and others deemed plate tectonics an example, it felt good to be part of one, especially a nonviolent revolution.[110]

Harvard's physicist/historian of science Gerald Holton has shown how non-scientific beliefs strongly influence the approach scientists have taken in different eras, and revolution was a theme that pervaded thought in the 1960s.[111] Yet, after the recognition of plate tectonics, most earth scientists returned to what they knew well. Younger scientists, for the most part better trained in basic sciences and mathematics than their mentors, began to push their own subdisciplines forward with rigor inspired by plate tectonics. Meanwhile, however, the mood of the country shifted to the almighty dollar, Americans elected Ronald Reagan with enthusiasm, and political revolutions were associated with their villains, not heroes. No wonder contemporary discussions of "the revolution" employ the past tense. Nevertheless, young scientists should realize that not only do the ideals of the French Revolution remain unattained, but so do those of the plate tectonics revolution.

The Future

One often hears nostalgic discussions of what an exciting time the plate tectonics era was. Today's scientific problems (climate change, landscape evolution, mantle dynamics, etc.) are no less exciting than those 25 to 30 years ago were. What made that time exciting and the present less so is not the nature of the scientific questions, but the number and nature of obstacles that now lie in front of young scientists compared with the unfettered days of plate tectonics. Obviously, difficulties of obtaining funding engender recurring discouragement, compared with what seemed like unlimited freedom in the 1960s. We students at Lamont were encouraged to write proposals to NSF, not to fund our research, but as part of our education; ignorance of the source of money that paid us was bliss. At Columbia, at the beginning of every semester I was required to fill out a form that described my thesis topic and its progress, but no one seemed to notice that for ten consecutive semesters I wrote "Not known at present" on every line except the one containing my name. Freedom to pursue what we wanted seemed to characterize the life of students, post-docs, and anyone eager to seize it. I was a third-year graduate student before I learned what "tenure" meant. Science was fun; that was enough.

Have the pressures to achieve "success," and to feel good about it, supplanted the pleasure of doing scientific research? Has the corporate mentality taken over science? By "reengineering" (sic) students into clients, graduates into products, and imaginative research into oblivion, have universities made funding more important than creativity? (A "creative solution" in the banking world is one so ill conceived that bankers laugh at it.) Young scientists are often encouraged to work on projects that are fun, *only* if funded; the correlation between quality of work and level of funding is immeasurably small. What about the tenure system, once designed to protect academics from the McCarthyism and political correctness of the 1950s, but now the last, highest hurdle in an exhausting race? "Tenure" impedes the development of young scientists less because it maintains departments full of dead wood blocking the paths of a more imaginative youth, and more because it forces young scientists to jump through a sequence of narrowly defined hoops, which, in turn, keep them channeled in research directions that are thought to gain them sufficient fame and funding for promotion, without regard for the pleasure of pursuing what appeals to them. By the time such scientists can relax with a job for life, they may have become so narrow in both scope and self-confidence that most of the field, and its excitement, lies outside their perspective, which now consists of a close-up view of the walls of the rut into which they have dug themselves.

I urge young scientists to assert themselves and to say to their department heads what MIT's John Edmond did, "Leave me alone, Frank. I know what I am doing."[112] Even in the unlikely case that your department head has more vision than Frank Press, if you think you know what you are doing, do it. If you want vision, avoid the ruts. Don't rewrite your Ph.D. thesis. Don't pursue what others want you to do, unless you really want to. Fail, for if you don't fail sometimes, probably you are just polishing the terrestrial monopole. Don't be afraid to be wrong in what you think (although make sure your data are sound). Don't be afraid to flounder at the forefront, at least for a while; it too teaches a lesson. Ask, "What is the next, most important problem?" Change directions to find it, but "when you can't think of anything to do, don't do nothing." Fail again, but most important, find a problem that turns you on. Then, repeat the process, unless you really do want to get old fast.

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Three disclaimers. First, my father was not my only, nor my more influential, parent. Without encouragement from my mother, Margaret Molnar, in those few activities that I could actually do better than my father (e.g., sports), I might not have recognized the extent of his insight. Second, this essay is not a study in history of science. I am well aware of Heilbron's admonitions to scientists to take their research of history as seriously as their scientific research.[113] As requested by the editors, I simply offer my personal views (dubbed "unexpurgated" by one editor). Third, I have been aggressive with some general subdisciplines of the earth sciences, for instance, numerical modeling and structure/tectonics. Do not take these personally, as attacks on individuals, but as opinions about popular trends. "Some of my best friends" are modelers and structural geologists who study tectonics, although perhaps for those who know the joke of the 1960s the double entendre applies.