When the Plate Tectonic Revolution Met Western North America.[1]

Tanya Atwater, Department of Geological Sciences, Webb Hall 1006
University of California, Santa Barbara CA 93106
atwater@geol.ucsb.edu

Chapter 15 (p. 243-263) in ***Plate Tectonics, An Insider's History of the Modern Theory of the Earth****,*
Naomi Oreskes, Editor, pub. 2001 by Westview Press, 424 p.

Contents

Beginnings: the Lure of Science and the Oddness of being Female

I was in high school in 1957 when the Russians successfully launched the first man-made satellite, Sputnik. It is hard to explain to younger generations just what a profound event that was. To us, it was totally astonishing: that we humble humans could put an object into outer space. Until then I had planned to be an artist, but I thought "Wow! If scientists can do that, they can solve anything (ghettos, hunger, strife, …)". So began my checkered studies in science.

Various college recruiters came through my high school and I started asking questions about science. When the recruiter from the California Institute of Technology came, he told me straight out that Cal Tech didn't accept women because they viewed us as a waste of their time – we would just get married, quit, and waste our educations. This was especially ironic since the rhetoric at the time was that no man would marry a woman who was as smart and educated as he was. Luckily I had my brilliant botanist mom and my admiring engineer dad as role models. My brother was a senior at the Massachusetts Institute of Technology at the time, so I went to visit him and various eastern schools. At Harvard, they didn't accept women but they proudly told me they would allow me to do the whole Harvard science curriculum if I enrolled at Radcliffe. Meanwhile, they rejected me at Radcliffe because I hadn't studied Greek or Latin. Thank goodness for M I T, where they said "Sure, come along." They had been accepting a sprinkling of women almost since the Institute's inception.[2]

In my junior year at M I T, I was in my fifth major, electrical engineering, when I accidentally took a physical geology course. I was hooked immediately. When they announced summer field camp at the Indiana University camp in Montana, I was first in line. I loved field camp: the mandate to hike out every day and commune with mountains, discover their secrets. I have always loved hiking and landscapes and maps, and geometry was my favorite high school subject. The entanglement of geologic structures with land surfaces presented for me an ideal geometric mapping puzzle. I was in heaven.

But I was nervous, too. Everything in geology was so descriptive and detailed. When it came time to discuss the larger forces, we simply drew big arrows at the edges of our maps: the hands of a capricious god shortening or extending our landscapes, willy-nilly. I really didn't want to spend my life adding descriptive observations to the pile, and anyway, I wouldn't be very good at it since I have a terrible memory for isolated facts. The plate tectonic revolution came along just in time to rescue my geo-career.[3]

Once I found geology, I had to move west. The rocks in the east are old and tired and cooked and spend most of their time covered with green or white stuff. Also, three years in Boston had made me realize what an insufferable Californian I am. I transferred to the University of California at Berkeley and finished my undergraduate degree in geophysics.

During the time that I was studying at Berkeley, my siblings had been roaming around South America having adventures – without me. I needed to get to South America. Cinna Lomnitz, from the University of Chile, was a visitor at the Berkeley Seismology Lab and I asked him about jobs in Chile. He took a long look at my bare feet and beads and flowers – this was Berkeley, 1965. He laughed, and said "You'd be good for them." Chile was a very formal place at the time. He gave me addresses and recommendations.

The Mysteries of the Oceanic Realm and their Revolutionary Solution

For the summer of 1965, while I waited to hear from Chile, I applied for and got an internship at the Woods Hole Oceanographic Institute. I admit, I was drawn primarily by the romance of the sea and ships. I didn't know enough to realize that the marine scientists were about to unleash a revolution upon the geo-world. When I saw a number of the Woods Hole staff preparing for an Upper Mantle Committee meeting in Ottawa, Canada, I asked my mentor, Bracket Hersey, if I could go too. He said "Sure. Why not?" and found me travel funds. The meeting was concentrated on the geophysics of the oceans and the various mysteries therein. The list of sessions included all the right things: mid-ocean ridges and rifts, fracture zones, trenches and island arcs, magnetic stripes. They knew what needed explaining, just not quite how to do it. Most of the major players in this small field were there and I greatly enjoyed meeting them and putting their faces and their quirkinesses to their names. The whole meeting was exciting, but the presentation that made the biggest impression on me was the one by J. Tuzo Wilson, about transform faults.[4] Tuzo was a wonderful showman with a great twinkle in his eye. After he had explained his idea, he passed out paper diagrams with two mid-ocean ridges connected by a transform fault. It said “cut here”, “fold here”, “pull here”. We all laughed, and I felt embarrassed (kindergarten games at this august scientific meeting?), but I took the paper back to the privacy of my hotel room and cut and folded and pulled and, wow: the light bulbs really went on in my brain. The simple geometry of the transform faults with their fracture zones holds the key to the geometry of formation of all the ocean basins – right there in that little piece of paper. I’ve been handing out versions of that diagram to students ever since, and urging them, after they stop laughing, to cut, fold and pull.

The revolution caught up with me in Santiago, Chile in 1966. I was working as a technician in the Geophysics Institute of the University of Chile when I heard about an international Antarctic meeting to be held there. I did my job reading seismograms in the early mornings and evenings so that I could attend the meetings during the day. One morning session, I was dozing through a series of papers full of Latin names of diatoms and foraminifera (single-celled planktonic organisms) when they announced an extra paper. Jim Heirtzler was passing through from Lamont-Doherty Geological Observatory on the way to meet a ship in Valparaiso and he wanted to present some marine geophysical results. In his talk he put up the Eltanin 19 magnetic anomaly profile - still, to this day, the most clear, beautiful, symmetrical profile in the world - and made the case for sea floor spreading.[5] It was like a bolt of lightning had struck me. My hair stood on end.

My sisters still remember how crazy I was at dinner that night. I was crazy-excited: this was that big-picture key I had been dreaming of. And I was crazy-disappointed too: there was a revolution going on and I was missing it. I immediately applied to graduate school at the Scripps Institute of Oceanography, but, in the rush of youth, I was sure the excitement would be finished by the time I could get there, a whole six months later. In fact, I was only a few weeks late for the start.

I arrived at Scripps in January of 1967 to find the place in chaos. Fred Vine had been there in December 1966 and had presented a collection of magnetic anomaly profiles from various spreading centers around the globe (including the beautiful Eltanin 19 profile).[6] This was extremely compelling evidence for sea floor spreading in all the oceans. Apparently the whole institution attended the talk, most of the scientists going in as fixists, all coming out as continental drifters. In the first meeting of my first class, marine geology, professor Bill Menard forgot to tell us any of the usual class preliminaries, just launched into raptures about this "wonderful new idea", scribbling all over the blackboard. I also took a "Geosynclines" seminar that spring and we, in the arrogance of youth, smirked our way through all that literature with its convoluted, fixist explanations and elaborate naming systems (there was a fancy Latin name for every sub-variant within the array of geosynclines). Now we realized, they were just describing ancient continental margins in their various tectonic situations and combinations.

My Education in Plate Tectonics

Since I arrived at Scripps in mid-year, my initiation into graduate school was ad hoc. They sent me to talk to several research groups to try to find a project. My second interview was with John Mudie at the Deep Tow group. His group was developing an instrument package that could be towed very near the ocean floor in order to get a systematic, high-resolution look at various deep sea features. He was anxiously looking for a student to work up the data to be collected during an upcoming cruise to the Gorda Rift, offshore of northernmost California. It would be the first close-up look at a sea-floor spreading center and it was going that spring, just a few months hence. I couldn't believe my good luck. I signed up immediately and never looked back. I heard much later that my little decision set off a long battle over what to do with "the girl" on board the ship – women on ships are bad luck, don't you know? This first battle was fought by Mudie, who had to constantly assert my need to be aboard and, in especially bad moments, my right to be there and to sue if they wouldn't let me go. Apparently there was similar virulent discourse behind the doors each time I went to sea, though I remained happily ignorant of it all.[7]

The results of the cruise were wonderful, showing that most new basaltic sea floor is formed in the narrow rift valley floor of the spreading center and that the giant rift mountains that flank the valley are built not by volcanism but rather by uplifts of blocks along big normal faults. I wrote up the preliminary results that summer and fall, with lots of help, encouragement, and goading from Mudie, and it was published as a lead article in the journal *Science*.[8] At the time, I had no idea what an honor that was. I presented this work at the American Geophysical Union the next spring, my first professional talk ever, to a full house that came especially to hear me. (When I hear about the miserable first talks of many of my colleagues, I continually marvel at how spoiled I was.) Again, Mudie gave me lots of help and advice and insisted on several rehearsals, so that the presentation went very well.

After the meeting, I heard that some other students were going up to New York for a tour of Lamont Geophysical Observatory (now known as Lamont-Doherty Earth Observatory). Curious, I joined them. I remember two things from that tour. One is that I was invisible. In every lab we visited, they introduced all the young men and skipped me, every time. I guess our student guide assumed I was someone's tag-along girlfriend and, therefore, of no account. I introduced myself and tried to establish that I was a scientist, too, but my hints fell on deaf ears. The other thing I remember was the map of earthquake locations that student Muawia Barazangi, working with Jim Dorman, had plotted onto transparent mylar sheets and had overlain on a huge wall map. There they were, the plates of the world all outlined by the earthquakes. It was stunning, awesome, so simple and clear and full of details about the individual plates. It was oh-so-hard to pull myself away from that map.

In those first years we didn't speak about "plate tectonics", rather, the magic phrases were "sea floor spreading" and the "Vine-Matthews hypothesis". Subduction was a necessary adjunct concept, but one that was much harder to test with marine geophysical techniques. Observations from the field of seismology gave us mantle subduction zones and rigid plates. When the paper by Jack Oliver, Brian Isacks, and Lynn Sykes "Seismology and the New Global Tectonics" came out in 1968, we students all read it forward and backward and argued about all its points.[9] It was a seminal paper for me, filling in many vital gaps in my understanding and solidifying my commitment to the whole scenario. The other paper that set me on the rigid plate road was the 1968 paper by Jason Morgan.[10] In this paper, Morgan laid out the mathematical basis for quantifying the displacements of plates on a sphere (they are rotations around "Euler Poles") and applied it to the several well-known plate boundaries.

My Education in the Doing of Science

My graduate student years at Scripps were frenetic. All the data ever collected about the solid Earth was waiting to be re-interpreted. I got in the habit of dropping in at Bill Menard's lab. It was already known that the magnetic anomalies in the northeast Pacific were exceptionally clear, and that they were well lineated and offset across the fracture zones, but no one had compiled them for a look at the regional pattern.[11] Menard had his draftswoman, Isabel Taylor, transfer all the available magnetic profiles from their paper records to their ship tracks on a big map. She did it all by hand - this was before computer data processing became routine. The result was spectacular. The magnetic anomalies of the northeast Pacific are especially easy to read and the emerging pattern was full of information about sea floor spreading and transform faulting processes.[12] Thus, every session that we had over the map was full of discovery and excitement. Menard and I began seeking each other out first thing in the morning to share our middle-of-the-night thoughts. I often couldn't sleep at night, my head was so abuzz with geo-possibilities and implications. Apparently he was having the same problem, because I often arrived in the morning to find his ideas scribbled on my blackboard. "What about this…?"[13]

In Bill Menard, I found a soul mate, a fellow enthusiast for geometric patterns and their implications. He was constantly cutting up pieces of paper and moving them around – "What if such and such happened? How would that play out in the sea floor patterns?" He had a thorough knowledge of the oceanic data sets of the time; we would predict some geometric relationship with our paper cut outs and out of his mind would pop examples of the same patterns from the real world. Imagine my surprise when, after a few weeks of this, he presented me with a draft manuscript describing our conversations. I was just having fun, playing mind games, and it was actually serious science. Indeed, those playful sessions resulted in three early papers in prestigious journals, summarizing the magnetic anomaly isochron patterns in the northeast Pacific and generalizing them to examine the effects of changes in direction of sea floor spreading.[14]

I learned many things from Bill Menard, among them that a new object or phenomenon needs to have a name in order to hold a place in the human mind. For example, some direction changes cause transform faults to pull apart along their lengths, allowing magmas to seep up into the resulting rifts. He dubbed this phenomenon "leaky transform faulting." I was amazed how often "leaky" transforms appeared in the literature thereafter (although the usage was not always what I would have chosen). In another example, I worked with fellow graduate student John Grow on a paper about the oceanic plate that once lay north of the Pacific plate and that was entirely subducted northward beneath Alaska and the Aleutian Island arc. Walter Pitman and Dennis Hayes at Lamont had already pointed out the evidence for this plate, but they had described it and its neighbors as plates I, II, III and IV, not exactly names that stick in the mind.[15] Plates I, III, and IV were, in fact, the Pacific, North American, and Farallon plates. We needed a name for plate II, the one that had been entirely subducted. We described our need to Donna Hawkins, who had done social work with native American peoples in Alaska, and she dug out her dictionaries and came up with a possible list of names and their definitions. We chose "Kula", the Athabascan word meaning "all gone."[16] I still blush when I see our paper credited (or sometimes discredited) with the discovery of this plate.[17]

Menard was responsible for the naming of many of the fracture zones in the north Pacific. He was especially pleased with the fracture zones off Mexico, which had been named after Mexican artists Orozco, Tamayo, Siqueros, and Rivera. Following his lead, I named new fracture zones right and left as they emerged from our patterns, all unknowing that there are weighty rules and procedures concerning the official naming of geographical objects. Happily, he had neglected to teach me about those.

Another rule of Menard's was: when drawing on napkins during a discussion, each individual must have her or his own pencil. Many a joint conversation was put on hold after the first sentence while he went to fetch that second pencil. Our conversations were so geometric that the person without a pencil was rendered voiceless. I am often reminded of this rule when a colleague or student, looking at something I am drawing, starts snatching at my pencil or madly pointing and finger-sketching: Ah yes…, time to implement Menard's multi-pencil rule.

The Power of Triple Junctions

Scripps was frequented by visitors from all over the world and they added greatly to the liveliness and depth of this already exciting place. Dan McKenzie was there during the fall of 1967 and he was thinking hard about many aspects of the new theories. My advisor, John Mudie, had set up a monthly beer party at a local German dance hall to get people together for informal talk. I especially remember one of these sessions during which McKenzie and Bob Parker arrived, bubbling over about some project they were working on.[18] I couldn't figure out what they were talking about and could barely hear them over the loud accordion music, but during a lull I asked, rather timidly, what the fuss was about. Dan took a napkin and sketched out the San Andreas and Queen Charlotte fault systems and the Aleutian/Alaskan subduction zone. He showed me how all these features lay along the boundary between two large rigid plates, the Pacific and North American plates. "That's all very well, but what about the Mendocino fracture zone? That doesn't line up," I complained, trying to grab his pencil so I could add the offending feature to his tidy sketch. (They were acting so smug, I hoped I could trip them up.) "Easy." said Dan, and he drew a third plate, the Juan de Fuca/Gorda plate, meeting the other two at the Mendocino triple junction. Three plates! Of course. So elegantly simple and so powerful. I sat there, agog, my brain zooming around in all directions. Here is what I wrote about this moment a few years later.[19]

> "It is a wondrous thing to have the random facts in one's head suddenly fall into the slots of an orderly framework. It is like an explosion inside. That is what happened to me that night and that is what I often felt happen to me and to others as I was working out (and talking out) the geometry of the western U.S. I took my ideas to John Crowell [at the University of California at Santa Barbara] one Thanksgiving day. I crept in feeling very self-conscious and embarrassed that I was trying to tell him about land geology starting from ocean geology, using paper and scissors. He was very patient with my long bumbling, but near the end he got terribly excited and I could feel the explosion in his head. He suddenly stopped me and rushed into the other room to show me a map of when and where he had evidence of activity on the San Andreas system. The predicted pattern was all right there. We just stood and stared, stunned.
>
> "The best part of the plate business is that it has made us all start communicating. People who squeeze rocks and people who identify deep ocean nannofossils and people who map faults in Montana suddenly all care about each others' work. I think I spend half my time just talking and listening to people from many fields, searching together for how it might all fit together. And when something does fall into place, there is that mental explosion and the wondrous excitement. I think the human brain must love order."

After that evening in the beer hall, I became a McKenzie groupie, attending his seminars; dogging him with questions; making a big nuisance of myself, I'm sure. He was humorously generous and I learned a lot, about tectonics, about the scientific approach, and about tectonic passion and delight.

The magnetic anomaly patterns of the northeast Pacific are different from those in other oceans in that they are almost entirely one-sided. The eastern half of the expected symmetrical pattern was embedded in the Farallon plate and has been subducted beneath North America, along with the spreading center that separated it from the Pacific plate. Thus, only the western half, the half that is embedded in the Pacific plate, remains for us to observe. This one-sided configuration was a hindrance at first, because the lack of symmetry removed one of the most convincing arguments for sea floor spreading. However, once the concept of spreading was demonstrated elsewhere, the one-sidedness revealed a remarkable relationship. The Farallon plate had been completely subducted in the exact regions now occupied by the San Andreas fault and its relatives, so that the subduction of the Farallon plate and its spreading center hold the key to the origin of the San Andreas system. Dan MacKenzie and Jason Morgan first described this geometric relationship (and named the Farallon plate) in their 1969 paper about triple junctions.[20]

The relationship just described is very useful for establishing the timing of events in Western North America. Since the San Andreas fault system forms the boundary between the Pacific and North American plates, it could not have originated until those two plates came into contact. This contact, in turn, could not occur until after the complete subduction of the intervening Farallon plate. The offshore magnetic anomalies would constrain when and where that transition occurred, if only we could obtain reliable ages for the magnetic reversals that caused the anomalies. We could not make the next step until we had these dates.

Mysteries of the Continental Realm in the San Andreas Fault System

Meanwhile, I was learning about the San Andreas system. The main fault had been recognized as a major through-going structure ever since the 1906 San Francisco earthquake.[21] Mason Hill and Tom Dibblee really sharpened our awe of this feature in 1953 when they laid out evidence for at least 500 km (about 300 miles) of cumulative offset across the fault.[22] By the time I began to study it in the late 1960's, it was clear that the San Andreas was a profound break and that it was almost surely a major boundary in the global plate system, but the age of origin and rate of offset were not known. Hill and Dibblee's most convincing evidence for large offset was in the displacement of late Cretaceous granites of about 80 million year age – from the Tehachapi Mountains in southern California to Bodega Head or beyond along the northern California coast. Had the fault been moving since late Cretaceous? It seemed likely at the time. We were just realizing, thanks especially to some papers by Warren Hamilton of the U. S. Geological Survey, that the Sierra Nevadan granites were formed in the roots of subduction type volcanoes.[23] The Sierran magmatic system had been active during much of the Mesozoic Era, implying a major, long-lived subduction zone, but this magmatism had suddenly ceased in the late Cretaceous, about 75 million years ago. This seemed to be just what we were expecting: a cessation of the subduction plate boundary and its replacement by the San Andreas plate boundary. Furthermore, if the fault had been moving steadily since the late Cretaceous, the offset rate would have been less than one centimeter per year (about one-third of an inch per year), quite slow. It all seemed to be coming together, or so we thought.

Bill Dickinson hosted a 1967 meeting at Stanford to see if the community could solidify the timing and displacement rate along the San Andreas fault.[24] Some of us students attended this meeting, sitting up high in the back of the big lecture hall, watching with awe as the grand old men presented their works. Dickinson began the meeting by urging all the speakers to be wild, to describe any tentative geological correlations that might conceivably bear on the subject. That introduction made a big impression on me. Before that, I had thought all public presentation of science had to be formal and factual and serious; no speculations allowed. How fun to see that the big guys had lots of wild ideas, too.

This august bunch of Californian geologists laid out lots of possible correlations – datable rock bodies or features such as ancient shorelines that occur on one side of the fault and that seem to match with similar bodies or features that occur somewhere on the other side of the fault. If the paired objects started out side-by-side and were later offset, they would help us work out the displacement history. This is always a tricky business, since many different rock bodies are quite similar in their characteristics, and many features, especially shorelines, tend to follow along faults, rather than crossing them. Among the many tentative correlations presented, the majority seemed to favor the slow rate described above.

Not everyone agreed with the Cretaceous origin and slow rate, however. A group from U. C. Berkeley, in particular, had evidence for a much faster rate. They presented exceptionally strong evidence for a correlation between the Neenach volcanic rocks in the northwestern Mojave Desert (on the east side of the fault) with volcanic debris in rocks at Pinnacles State Park, near Salinas (on the west side). This match documents an offset of about 320 km (about 200 miles) sometime after the volcano erupted 23 million years ago. They presented this and related data implying a rate of several centimeters per year.[25] Thus, we were left with two conflicting scenarios for the San Andreas – a young, fast-slipping fault or an older, slower-moving one. I sat there aching, knowing that the offshore magnetic anomalies would bring an independent voice to this problem, if only they could be reliably dated.

Establishment of the magnetic reversal ages presented a big challenge (as does most geological age dating, in fact). During the 1950s and 1960s the paleomagnetic community had honed the ages of the reversals that occurred during the last few million years.[26] They did this using isotopic methods, dating normally and reversely magnetized lava flows on land. These ages were the ones used by Fred Vine in his 1966 compilation to date the youngest magnetic anomalies at each sea floor spreading center and to establish recent spreading rates, a hugely valuable contribution. However, for ages greater than a few million years, the dating errors were too large to distinguish one reversal event from another and so were useless. We really needed those older ages.

The Lamont Miracle – First Solutions for the World's Oceans and their Timing

Meanwhile, on the global scale, the marine geophysical group at Lamont was busy interpreting the world. For many years their ships had been traversing the global oceans under the somewhat dictatorial eye of Maurice Ewing, the founder and director of Lamont. Everywhere these ships sailed, whatever the immediate interests of the shipboard scientists, they collected a coherent set of geophysical and geological data. As part of the routine, they measured magnetic field profiles, even though no one could make sense of the resulting wiggly lines. It was a relatively easy measurement to make, so they made it. The other major U. S. ocean-going research institutions were much more democratic (anarchic?), each scientist following his own agenda and those of his close associates. During traverses between study sites, data collection was somewhat haphazard. When Fred Vine and Drum Matthews finally supplied the key ideas for reading the magnetic anomalies, the Lamont group was in the unique position to interpret the broad histories of most of the world's ocean basins. They presented these interpretations in a series of papers in the March 1968 issue of the *Journal of Geophysical Research*.[27] My copy of this issue is disgustingly grubby and tattered from my constant reference to these papers during the next decade.

The final paper in the March 1968 series was especially important.[28] The preceding papers had presented oceanic histories in terms of magnetic anomaly numbers, using an informal numbering system that Walter Pitman had invented for the purposes of communication within their group. He assigned the central anomaly the number 1 and, working outward, assigned 2 through 32 to distinct bumps in the rest of the known pattern. We still use this numbering system, slightly modified, referring to the numbers as "magnetic isochron numbers," or just "chrons." In that final paper, the Lamont group amassed and compared all their data concerning the distances from the spreading centers out to the various magnetic isochrons. In a series of innovative comparison tests, they concluded that the South Atlantic was the most likely of all the oceans to have spread at a steady rate over the long term. (Of course, no one had any idea if that was even possible.) They then made the leap and extrapolated from the South Atlantic spreading rate, known for the last 4 million years, out to 85 million years – a 20-fold extrapolation. With this audacious extrapolation, they were able to assign tentative dates to magnetic reversal chrons 1-32. The resulting timescale became known as the Heirtzler scale, after the first author, Jim Hiertzler. It has turned out to be surprisingly accurate, good to a few percent in most parts – one of those great strokes of genius or luck or both – but of course, at the time, no one knew if they were even close. Indeed, there was some evidence that the present spreading rates were only good back to about 10 million years (chron 5), and that there may have been a pause in spreading of unknown duration before that.

Meanwhile, back at Scripps I was stewing over our sea floor isochron patterns, yearning for some reliable dates. The one-sided magnetic anomalies nearest the California coast were easily identified as chrons 10-6. These had been formed by the Pacific-Farallon spreading center and, thus, had to have preceded the end of subduction and the start of the San Andreas plate boundary. In the Heirtzler scale, the extrapolated ages for chrons 10-6 were about 30-20 million years, implying a quite young San Andreas. But what if there had, in fact, been a spreading hiatus before chron 5? Then chrons 10-6 could have any older age – maybe even late Cretaceous, seemingly matching the preponderance of evidence from the land.

Young? Old? Young? Old? We needed direct dates for these older isochrons.

At first thought, this problem doesn't seem so difficult: just dredge some rocks from the different parts of the sea floor and date them. Unfortunately, all the sea floor is continually being buried in a snowfall of debris (mud and biological remains) so that all the older rocks are buried under a mantle of younger sediments. To get the basement rock ages, we would have to drill through this overlying sedimentary pile.

The Deep Sea Drilling Miracle – Direct Confirmation at Last

The Deep Sea Drilling Project came on line at just the right time to give us the gift of age dating that we needed so badly. Various grand schemes to drill through the entire oceanic crust and into the mantle had been around since the Mohole Project of the 1950s. By the mid 1960s, these efforts had consolidated into the more modest Deep Sea Drilling Project, whose aim was to drill many holes into and through the sea floor sedimentary cover. Quite by lucky chance, the drilling ship, the Glomar Challenger, was ready to begin its work just when the community was especially hungry to use it. The ship set sail in the fall of 1968 and after some trials, set out for the South Atlantic to test the symmetry of that ocean and to check the proposed constancy of its spreading rate. I have heard that many of the scientists on that expedition boarded the ship in Dakar with considerable skepticism for the whole idea of sea floor spreading. When they got off the ship in Brazil, two months later, they were all avid, noisy believers. They had drilled nine holes along a line across the mid-Atlantic ridge and westward toward South America. By identifying the fossils in the bottom-most sediments, the shipboard scientists had been able to determine the ages at the base of the sedimentary piles in seven of the holes. As each age was determined, they had plotted it on a graph versus its distance from the central ridge. The points formed a perfect straight line (within the errors of the data). To everyone's surprise (including that of its authors), that outrageous Heirtzler scale extrapolation was correct.[29]

Writing up the History of the San Andreas

With the validation of the Heirtzler time scale, the San Andreas history suddenly became tractable. I don't recall how I first heard about the South Atlantic dating results, but it disrupted my concentration on my thesis work. By summer 1969, I had dropped all pretense at the sea floor work and was struggling along with the San Andreas plate story. There followed the most intense work period of my life. It was almost like a trance that I would be in for many days at a stretch, hardly sleeping or eating.

This brings up one important factor in this story: the nature of funding in the 1950's and 1960s. It was much more general and flexible than the present grant system. Throughout my graduate years, I was funded by the Navy through the Marine Physical Laboratory. Officially, I was working up the Gorda Ridge Deep Tow surveys, but my major excursions into plate tectonics (nine straight months for the San Andreas paper) were accepted, indeed encouraged, by my advisor, John Mudie, and by our Navy funders. The Navy at the time tended to fund productive sea-going groups and individuals in their scientific endeavors, without being too particular about the details. Their view was that any information about the oceans was useful to their mission. Thus, we had a lot of freedom to be productive wherever our hearts led us. In later years the Navy funding became much more restrictive, so that the pure research community was shifted to the National Science Foundation. Funding from the latter is excellent in many respects but, since it awards money for specific projects, there is less flexibility for following up unanticipated avenues as they appear.

That fall (1969) Warren Hamilton came visiting to Scripps from the U.S. Geological Survey. He came to learn first hand about the revolution and to present a graduate seminar about continental tectonics. I spent many happy hours in his office, absorbing bits of his vast store of continental geological lore and sharing what I had been learning about marine geophysics. I was still hard at work honing the San Andreas story and loved the chance to try out many of the pieces on him. We had such fun sharing mind candy that I remember one middle of the night, about 2 AM, when I woke up to some mental explosion and just couldn't wait to try it out on him. I called him up and yakked away into his sleepy ear. When he finally managed to get a groggy word in, it was (with patiently humorous undertones) "What time is it, anyway?" I took the hint and let him hang up – promising to repeat it all first thing in the morning.[30] Warren infected me with his passion for big-picture geology – a view of geology that I hadn't really encountered much before.

That winter, Bill Dickinson organized another of his specialty meetings, this one a Penrose Conference at Asilimar, California. It was primarily a land geologists' meeting, but he expressly invited students, so a gang of Scripps students went. The meeting was full of good information about the plate tectonic interpretations of many geological phenomena and it solidified all these for me. It was also very empowering because we oceanography students found ourselves in the role of teachers - about ocean floor features, in particular, and about oceanic plate tectonics, in general. I presented my San Andreas story in a badly crafted talk - I went way over time - but when the moderator decided to cut me off, someone in the audience called out "Aw, let her go on. This is great stuff." (Bless you, whoever that was.)

Another special memory from that meeting is of a moment at the end of my presentation when someone asked whether he really had to accept such young dates for magnetic chrons 10-6 and, thus, a young age for the San Andreas. I was groping in my mind for a convincing description of the South Atlantic drilling results when a voice called out from the audience: "It's true! It's true! Believe it!" The speaker, Ken Hsu rose up and took over. He had been on that wonderful Deep Sea Drilling expedition and spoke with all the passion of the newly convinced. We all enjoyed his exciting and overwhelming recitation both of the results and of his own personal conversion.

About that time, I ran into Allan Cox from Stanford University at some meeting and told him that I was working on the history of the San Andreas fault. I could see his eyes rolling up in his head and his struggle to come up with something polite to say to this starry-eyed, impudent student. I asked him if he would read and critique my manuscript and his non-enthusiasm was palpable, though he graciously agreed. I sent it to him a few weeks later and it came back almost immediately with big letters on the front: "PUBLISH THIS IMMEDIATELY..." It was the impetus I needed. I was elated but also scared. This project was my first real solo writing effort. I hit up everyone I knew for reviews and got a lot of excellent advice, including some extremely helpful suggestions from my fellow students. Probably the most useful of all the reviews was from Warren Hamilton. He had a clear understanding of the importance of brevity and clarity and he didn't hesitate to go after me about it. My original manuscript was dense with "what ifs" and minor possible implications, and so he crossed out whole pages of mine with the simple remarks "FLUFF" and "STUFF". Of course he was also very excited and encouraging. The balance was perfect. The paper was published as the lead article in the December 1970 volume of *the Bulletin of the Geological Society of America*.[31]

After the Penrose conference, and especially after the San Andreas paper was published, speaking invitations poured in from all over the West. My synthesis was just what many land geologists had been waiting for. They had heard noisy rumblings from the ocean-going community, but it hadn't been clear how the revolution would affect continental work. The San Andreas history is quite unusual in that the oceanic and continental realms are so completely, intricately inter-tangled. You really can't understand one without the other: the oceanic geophysical record documents the demise of the spreading center while the continental geological record shows the development of the resulting new plate boundary. Although I was officially a marine geophysicist, my passion still held for the mountains and landscapes of the continents. Thus, I had a foot in each camp and became a kind of translator, telling each group about the findings of the other side. I suppose it had something to do with being female, too. I knew that a number of those in every audience were there to see the freak. (Indeed, many of the speaking invitations were prefaced with the rationale that their girl students needed to see a real-live female scientist.) I didn't mind. I knew that I had an irresistible tale to tell and was happy to present it for anyone who would listen.

Work on Plate Circuit Reconstructions

At the time of my 1970 paper, I was still missing one important piece of information. In order to work out the details of the plate interactions, I needed to know the long-term history of Pacific - North American relative motions. We had evidence that the Pacific plate is presently moving parallel to the San Andreas fault 5-6 cm/yr (about 2 in/yr) past North America. However, we couldn't be sure how long that had been the case and a number of lines of geologic evidence suggested that this motion had been slower in the past. In the 1970 paper, I presented two "end member" models: one in which the relative motion had been steady and a second with no relative motion before about 10 million years ago. If we wished to find out the actual history of Pacific - North America motion, we needed to make "plate circuit reconstructions" around the world for a number of past times.

The plate circuit that must be followed in order to calculate a past location of the Pacific plate with respect to North America is one that steps from the Pacific plate to Antarctica to Africa to North America, crossing a spreading center in each step. For example, a reconstruction for eleven million years ago, the time of magnetic chron 5, is based on the following steps. First we reconstruct the Pacific plate to the Antarctic plate using the chron 5 patterns in the sea floor of the south Pacific. Next we reconstruct both, together, to the African plate using chron 5 patterns in the southwest Indian Ocean, south of Africa.[32] Finally, we reconstruct those three, all together, to the North American plate using chron 5 patterns in the central-north Atlantic. If we could do similar reconstructions for a number of different chrons, we could work out the approximate track of the Pacific plate past North America through time.

This plate circuit (and every plate circuit that relates Pacific ocean plates to the continents) uses the step from Antarctica to the Pacific plate. This step is made using the spreading patterns on the Pacific-Antarctic ridge in the south Pacific. In 1970, this ocean was quite poorly known. Peter Molnar came from Lamont to Scripps at about that time on a post-Doctoral Fellowship. We set out, together with Scripps map maker Jaqueline Mammericx, to quantify the plate motions across this spreading center[33] Using the results of that study, we were then able to construct a track of past Pacific locations with respect to North America for four points in time. The uncertainties on the locations of the points in this first track were quite large, but they did generally support a long-term northwest drift of the Pacific past North America.[34]

Finished? (Really, just waiting.)

In the late 1970's and 1980's, the plate tectonics "revolution" took an interesting turn. It became old-hat for the land geologists. Whole geological meetings were conducted with hardly a mention of plate tectonics. Oceanic work continued apace, deepening and honing the theory, but on the continents, it seemed to have become irrelevant to most new work. The early revelations, of course, had given the community a huge leap forward in general understanding of earth processes, and they definitely set us free: it was suddenly not outrageous to think about terranes or whole continents travelling far distances across the globe. However, the quantitative aspects, so powerful for predicting patterns in the ocean floors, didn't seem helpful on the continents. In western North America, for example, our multi-step circuit reconstructions were generally too crude to help with specific geologic problems. (A geologist standing on a hillside outcrop isn't impressed by a prediction that has an uncertainty of hundreds of miles.) At the time, I thought maybe we were done with continental global tectonics, and I returned to my ocean floor studies with renewed vigor. It turns out that, rather than being finished, we had simply run through the collected store of relevant information and so had to wait a while for improvements in concepts, techniques, and data sets.

Measuring the present-day drifts and deformations of the plates.

Several technological advances changed the nature of plate tectonic studies in the 1980s and 1990s. One exciting aspect has been the development and honing of various new systems for measuring the locations of points on the Earth's surface. During the mid twentieth century, great progress had been made with surveying and laser ranging techniques for characterizing local deformations near and across selected active faults, especially those of the San Andreas system. These had shown us a complex history of on-going deformations near the plate boundaries and had given us some understanding of how energy accumulates near plate boundaries and then is released during earthquakes. The real test of plate motions, however, required that we measure the on-going motions of the plate interiors, far from any plate boundary complications.

Global scale measurements of relative positions on the Earth's surface became possible in the latter decades of last century through the "Very Long Baseline Interferometry" program, or VLBI. By comparing and timing signals coming to Earth from deep space radio stars, scientists in this project were able to find the locations of their observation points with an accuracy of a few centimeters. In order to measure the relative displacements of those points through time, they had to measure the locations, then wait years, then measure, then wait again. The results were definitely worth the waiting. Repeat measurements of locations over the decades have given us the wonderful (and reassuring) result that the movements of the rigid plate interiors during the last few decades have been the same as the motions over millions of years, i.e., motions that we had deduced from the magnetic anomalies!

An especially fun innovation has been the development and democratization of the Global Positioning System or GPS.[35] This is the satellite system (originally developed for military navigation) that now allows any citizen to locate herself (or her fancy car) on the earth's surface. A researcher can place a marker, then, with some patience and diligence, can use GPS to determine its position within a few centimeters. The uncertainty in these measurements is about the same as the displacement of plates in a year, so that one only needs to monitor the location of the marker over a few years time span to get a quite good estimate of its ongoing movement.[36] Furthermore, the equipment for making GPS measurements is relatively cheap so that many groups can make local and regional measurements. Thus, it has become feasible to measure deformations in broad plate boundary zones, both steady motions and the time dependent deformations around earthquakes and creeping faults. For example, a dense Japanese array of continuous GPS stations is already yielding wonderful images of the ongoing warpings of the land over that major subduction zone.[37] Likewise, the results from periodic measurements across western North America are full of new detail about the way the plate boundary deformation is presently partitioned across the West.[38] I find myself eagerly awaiting each new data set and its revelations.

Measuring Past Plate Displacements and Deformations

Studies of past plate motions have also greatly benefited from technological developments. Our primary data sets for reconstructing the histories of plate motions are oceanic magnetic anomalies and fracture zone trends. These have mostly been gathered aboard oceanographic ships lumbering slowly across the surfaces of the world's oceans. When I began going to sea, our biggest problem was figuring out our position. In the south Pacific and other remote regions, we were proud if we could locate the ship within a few miles twice a day (by measuring the stars at sunrise and sunset, but even then, only "if the weather be good").[39] Post-cruise data processing often involved Herculean efforts to adjust the navigation record so that the data sets were at least self-consistent, not to mention located well on the earth's surface. The advent of satellite navigation and, eventually, of GPS navigation has changed all this. With this system we can now routinely locate the ship to within a few meters every second. When we tell our students about the bad old days and our navigational labors, they look at us as the poor, deprived, primitive ancients.

Technology has also given us a wonderful gift of ocean topographic coverage with the laser altimetry satellites, Seasat, Geosat and ERS-1.[40] These satellites measure the height of the top of the water in the oceans with a precision of a few centimeters, somehow averaging out all the waves and tides. In turn, variations in the water surface height show us gravity variations caused by the topography of the ocean floor. Linear fracture zones show up as some of the most dramatic features on these records and maps, and this is a special boon for us, since our plate tectonic reconstructions of plate motions are based upon fracture zone trends. While the ship-generated sonar records of these features are more detailed and precise than the satellite altimetry records, they are very tedious to collect. In a few years of observations, the satellites filled in the fracture zones in vast regions of the more remote oceans, including those southern oceans so critical for our reconstructions around Antarctica. Combining these with new, well-located, magnetic anomaly data, we are finally able to make round-the-world circuit solutions that have some relevance for land geologic studies. For example, in a recent article Joann Stock at Cal Tech and I were able to reconstruct the Pacific plate track past North America with some location uncertainties as small as a few kilometers. This track, in turn, allowed us to formulate a quite precise "deformation budget" for Western North America. For example, we predicted that the continent must have extended at least 265 km (about 150 miles – that is a lot of extension!) and must have been sheared parallel to the coast as much as 870 km since about 20 million years ago.[41] These budget estimates were made from our chron 6 round-the-world oceanic reconstructions. If they are correct, they should match estimates made from summing the deformations observed across the land.

Estimating the magnitudes of past land deformations

Land deformations are much more difficult to quantify than those in the oceans, because all land deformations are superimposed upon older features. We are spoiled in the oceans where virtually all of the deformation is accommodated by the creation or destruction of crust. Fortunately, the late twentieth century also saw great progress in our ability to quantify continental tectonic deformations.

From the perspective of plate tectonic history, a crucial breakthrough has been the recognition, acceptance, description, and quantification of large magnitude extensional features known as core complexes, or low-angle extensional detachment faults. These deformation systems allow the crust to extend one hundred to several hundred percent in a very short time (perhaps less than one million years)..[42] These events often bring the ductile middle crust to the surface, laid bare or thinly strewn with fallen-over "dominoes" of the broken, brittle upper crust. The amount of extension represented by one of these features can often be estimated by re-erecting the dominoes to reassemble the original upper crust. The timing is often recorded in the lavas that tend to accompany the extensional events.

The Basin and Range Province of interior Western North America contains a large number of these extensional features. Many of them date from the Miocene and, thus, they overlap the San Andreas deformations in time and space. In the east-west corridor near Las Vegas, Wernicke and Snow of Cal Tech were able to add up all the extensions between the Colorado Plateau and the Sierra Nevada and thus to estimate for the first time the very large Basin and Range extensional budget.[43] With this piece, we can finally compare the oceanic and continental deformation estimates, and they agree. It took nearly thirty years, but the quantitative power of the plate tectonic theory is finally becoming relevant on the land.[44]

Better and better, so far at least.

From time to time, every scientist must step back and re-examine her assumptions. I have often done this in my life, sometimes of my own volition and sometimes when under the barrage of some doubter. In plate tectonics work, our most basic assumption is that the aseismic interiors of the plates are rigid, so that we can deduce the motion of every point on each plate using relatively few measurements along the plate edges. It is a pretty outrageous assumption, especially given the array of non-rigid structures that present themselves to the student of continental geology. We must suppose that all these structures were formed when each region lay near a plate boundary – but this supposition often has no independent confirmation.

As the years pass, I have regularly been pleased (and surprised, and relieved, I admit) to see the rigid plate assumption holding true and being reconfirmed with new techniques and data sets. With the passage of time, most scientific ideas are overturned or are greatly modified. Similarly, as the uncertainties in our data sets get smaller and smaller, I fully expect that we will start detecting the non-rigidity of the major plates, but this is yet to happen. So far, with just a few small adjustments, the assumption continues to work. As I tell my students: "Gol' dern! It must be true!"

Publication reference:

Tanya Atwater, "When Plate Tectonics met Western North America", Chapter 15

*(p. 243-263) in **Plate Tectonics, An Insider's History of the Modern Theory of the Earth**,*

Naomi Oreskes, Editor, pub. 2001 by Westview Press, 424 p.

Notes

[1] In general, I have a very poor memory. I request forgiveness in advance for events that I misremember and from colleagues I may have misrepresented or slighted.

[2] When I later asked one of my M I T professors why they bothered with us women, he said it was because a woman with an M I T education would raise great children (read "sons"). I didn't even blink, I guess I was used to it by then. To be fair, that was just one man's opinion.

[3] Ten years later I spent some geological field time in the Caucasus mountains with three Georgian geologists including a young woman, Manana Lordkipanze. Comparing notes, we found that she had had exactly the same experience, Soviet style. She, too, was about to give up geology when she went to an international meeting in Moscow, heard about plate tectonics, and found her calling.

[4] His presentation covered the material in Wilson, J. Tuzo, 1965, A new class of faults and their bearing on continental drift, *Nature*, v. 207, no. 4995, p 343-347.

[5] Pitman, W. C., III and Heirtzler, J. R., 1966. Magnetic anomalies over the Pacific-Antarctic Ridge, *Science*, v. 154, no. 3753, p. 1164-1166. A few years ago I spent a fall sabbatical at Lamont. I spent a while sorting through their voluminous data sets to plot out magnetic anomaly profiles from all the world's spreading centers. I was looking for good teaching examples to show the effects of latitude and spreading rate, but while I was at it I conducted a magnetic anomaly "beauty contest". After all these decades of ships collecting new data, that old Eltanin 19 crossing still won first prize. Its high latitude, medium fast spreading rate, and lack of noisy seamounts make it wonderfully clear and symmetric and exceptionally easy to read.

[6] Vine, F. J., 1966. Spreading of the ocean floor; new evidence, *Science*, v. 154, p. 1405-1415.

[7] Actually, they must have been glad they had me after the second year. About then Uncle Sam ran out of cannon fodder for the Vietnam War and cancelled most graduate student deferments. Many of the young men in my graduate class quit and joined the Coast Guard as a preferable alternative to being drafted. Some years later, Allan Cox asked me if I felt guilty about not being eligible for the draft while all my male peers were having such a hard time with it. "Guilty?" I said, shocked. "No way! Lucky? Yes, but not guilty." This was not my war; not a war that my generation could believe in.

[8] Atwater, Tanya M. and Mudie, John D., 1968, Block faulting on the Gorda Rise, *Science*, v.159, no. 3816, p. 729-731.

[9] Isacks, Bryan, Oliver, Jack, and Sykes, Lynn R., 1968. Seismology and the new global tectonics, *Journal of Geophysical Research*, v. 73, p. 5855-5899.

[10] Morgan, W. Jason, 1968. Rises, trenches, great faults, and crustal blocks, *Journal of Geophysical Research*, v. 73, p. 1959-1982.

[11] Vacquier, Victor, Raff, Arthur D., and Warren, Robert E., 1961. Horizontal displacements in the floor of the northeastern Pacific Ocean, *Geological Society of America Bulletin*, v. 72, p. 1251-1258.
Mason, Ronald G., and Raff, Arthur D., 1961. Magnetic survey off the west coast of North America, 32 degrees N. latitude to 42 degrees N. latitude, *Geological Society of America Bulletin*, v. 72, p.1259-1265.
Raff, Arthur D., and Mason, Ronald G., 1961. Magnetic survey off the west coast of North America, 40 degrees N. latitude to 52 degrees N. latitude, *Geological Society of America Bulletin*, v. 72, p. 1267-1270.
Peter, George, 1966, Magnetic anomalies and fracture pattern in the northeast Pacific Ocean, *Journal of Geophysical Research*, v. 71, no. 22, p 5365-5374.

[12] Indeed, these anomalies would have given the Eltanin 19 profile some serious competition in the "beauty contest" if only their symmetrical halves had not been subducted under North America.

[13] Long after I left Scripps, whenever Menard and I found each other at a meeting, we would make a lunch date, just for the pleasure of spending a couple of hours scribbling on napkins, sharing our latest "mind candy". Indeed, on my last visit with him, shortly before he died, I was pleased to see his blackboard filled with his latest ideas - possible correlations he was planning to track down.

[14] Menard, H. W., and Atwater, Tanya, 1968. Changes in direction of sea floor spreading, *Nature* (London), v. 219, no. 5153, p. 463-467.
Menard, H. W., and Atwater, Tanya, 1969. Origin of fracture zone topography, *Nature* (London), v. 222, no. 5198, p. 1037-1040.
Atwater, Tanya, and Menard, H. W., 1970. Magnetic lineations in the northeast Pacific, *Earth and Planetary Science Letters*, v. 7, p. 445-450.

[15] Pitman, Walter C., III ; Hayes, Dennis E., 1968. Sea-floor spreading in the Gulf of Alaska, *Journal of Geophysical Research*, v.73, p. 6571-6580.

[16] Grow, John A., and Atwater, Tanya, 1970. Mid-Tertiary tectonic transition in the Aleutian arc, *Geological Society of America Bulletin*, v. 81, p. 3715-3721.

[17] In 1968, Lonsdale gave a talk "Kula Plate not kula" in which he identified a patch of the Pacific plate which he believed had started out as a piece of the Kula plate and still is not "all gone". Lonsdale, Peter, and Debbie Smith, 1968. Kula Plate not kula, *Eos, Transactions, American Geophysical Union*, v.67, no. 44, p 1199.

[18] published a few months later in McKenzie, Dan P., and Parker, R. L., 1967. The north Pacific; an example of tectonics on a sphere, *Nature* (London), v. 216, no. 5122, p.1276-1280.

[19] Cox, Allan, 1973. *Plate Tectonics and Geomagnetic Reversals*, W. H. Freeman Co., San Francisco, pub, p. 535.

[20] McKenzie, D. P., and Morgan, W. J., 1969. Evolution of triple junctions, *Nature* (London), v. 224, no. 5215, p. 125-133.

[21] Lawson, Andrew Cowper, ed., 1908. The California earthquake of April 18, 1906, *Report of the State Earthquake Investigation Commission*, Carnegie Inst Wash, Pub 87, 451 p.

[22] Hill, Mason Lowell, and Dibblee, Thomas Wilson, Jr., 1953. San Andreas, Garlock, and Big Pine faults, California; a study of the character, history, and tectonic significance of their displacements, *Geological Society of America Bulletin*, v. 64, p. 443-458

[23] for example, Hamilton, Warren, 1969. Mesozoic California and the underflow of Pacific mantle, *Geological Society of America Bulletin*, v. 80, p 2409-2429.

[24] Dickinson, William R., and Grantz, Arthur, 1967. Indicated cumulative offsets along the San Andreas fault in the California coast ranges, *in* Dickinson, William R., and Grantz, Arthur, eds., *Proceedings of conference on geologic problems of San Andreas fault system*, Stanford University Publications. Geological Sciences, v. 11, p. 117-120.

[25] Huffman, O. F., 1972. Lateral Displacement of Upper Miocene Rocks and the Neogene History of Offset along the San Andreas Fault in Central California, *Geological Society of America Bulletin*, v. 83, p. 2913-2946.
Matthews, V., 1976. Correlation of Pinnacles and Neenach volcanic formations and their bearing on San Andreas Fault problems, *Bulletin of the American Association of Petroleum Geologists*, v. 60, p. 2128-2141.

[26] as summarized in Cox, Allan, and others, 1968. Radiometric time-scale for geomagnetic reversals, *Quarterly Journal of the Geological Society of London,* v.124, no. 493, p 53-66.

[27] Pitman, W. C., III, Herron, E. M., and Heirtzler, J. R., 1968. Magnetic anomalies in the Pacific and sea floor spreading, *Journal of Geophysical Research*, v. 73, p. 2069-2085.
Dickson, G. O., Pitman, W. C., III, and Heirtzler, James R., Magnetic anomalies in the south Atlantic and ocean floor spreading, *Journal of Geophysical Research*, v. 73, p. 2087-2100.
Le Pichon, Xavier and Heirtzler, James R., 1968. Magnetic anomalies in the Indian Ocean and sea-floor spreading, *Journal of Geophysical Research*, v. 73, p. 2101-2117.

[28] Heirtzler, James R., Dickson, G. O., Herron, Ellen M., Pitman, W. C., III, and Le Pichon, Xavier, 1968. Marine magnetic anomalies, geomagnetic field reversals, and motions of the ocean floor and continents, *Journal of Geophysical Research*, v. 73, p. 2119-2136

[29] Maxwell, Arthur E., Von Herzen, Richard P., Hsu, K. Jinghwa, Andrews, James E., Saito, Tsunemasa, Percival, Stephen F., Jr., Milow, E. Dean, and Boyce, Robert E., 1970. Deep sea drilling in the south Atlantic, *Science*, v. 168, no. 3935, p. 1047-1059.

[30] I have been surprised to discover how differently various colleagues feel about new discoveries. A colleague once told me that he loves to keep a new idea to himself for a while, savoring the feeling that he may be the only one in the world with that particular concept. In contrast, when something dawns for me, I can’t wait to tell someone. It is as if it doesn’t exist until I say it out loud.

[31] Atwater, Tanya, 1970. Implications of Plate Tectonics for the Cenozoic Tectonic Evolution of Western North America, *Geological Society of America Bulletin*, v. 81, p. 3513-3535.

[32] Actually, this ocean was too poorly known when we made the first circuit calculations so we had to insert yet another step, through Australia/India, to get from Antarctica to Africa.

[33] Molnar, Peter, Atwater, Tanya, Mammerickx, Jacqueline, and Smith, Stuart M., 1975. Magnetic Anomalies, Bathymetry and the Tectonic Evolution of the South Pacific since the Late Cretaceous, *The Geophysical Journal of the Royal Astronomical Society*, v. 40, no. 3, p. 383-420.

[34] Atwater, Tanya, and Molnar, Peter, 1973. Relative motion of the Pacific and North American plates deduced from sea-floor spreading in the Atlantic, Indian, and South Pacific oceans, *Conference on tectonic problems of the San Andreas Fault system, Proceedings*, Stanford University Publications, Geological Sciences, v. 13, p. 136-148.

[35] for information about GPS, visit the University NAVSTAR Consortium, 1998, UNAVCO brochure, www.unavco.ucar.edu/community/brochure/.

[36] I find it quite wonderful that a system developed to track missiles and other fast-moving man-made objects is equally useful for monitoring the slow, stately drift of the plates.

[37] Sagiya, T., S. Miyazaki and T. Tada, 2000. Continuous GPS array and present-day crustal deformation of Japan, PAGEOPH, 157, 2303-2322.

[38] Thatcher, Wayne; Foulger, G. R.; Julian, B. R.; Svarc, J.; Quilty, E.; Bawden, G. W., 1999. Present-day deformation across the Basin and Range Province, Western United States, *Science*, v. 283, no. 5408, p. 1714-1718.

[39] There is a wonderful refrain in an old folk song in which all sorts of outrageous things are being planned… “that is, if the weather be good.”

[40] Smith, Walter H. F.; Sandwell, David T., 1997. Global sea floor topography from satellite altimetry and ship depth soundings, *Science*, v. 277, no. 5334, p. 1956-1962.
Sandwell, David T.; Smith, Walter H. F., 1997. Marine gravity anomaly from Geosat and ERS 1 satellite altimetry, *Journal of Geophysical Research, B, Solid Earth and Planets*, v. 102, p. 10,039-10,054.

[41] Atwater, T., and Stock, J. M., 1998, Pacific-North America plate tectonics of the Neogene southwestern United States: an update: *International Geological Review*, v. 40, p. 375-402. (Reprinted, 1998, in Ernst, W. G., and Nelson, C.A., eds., *Integrated Earth and Environmental Evolution of the Southwestern United States: The Clarence A. Hall, Jr., Volume*, Bellwether Pub., Columbia, MD, p 393-420.).

[42] e.g. see Gans, P.B., et al, 2001. Rapid Eocene extension in the Robinson district, White Pine Co., Nevada: Constraints from 40AR/39Ar dating, *Geology*, v. 29, p. 475-478.

[43] Wernicke, B.P. and Snow, J.K., 1998, Cenozoic Tectonism in the central Basin and Range: Motion of the Sierran-Great Valley block, : *International Geological Review*, v. 40, p. 403-410. (Reprinted, 1998, in Ernst, W. G., and Nelson, C.A., eds., *Integrated Earth and Environmental Evolution of the Southwestern United States: The Clarence A. Hall, Jr., Volume*, Bellwether Pub., Columbia, MD, p 111-118..).

[44] To see this quantitative deformation in action, visit my web site: www.geol.ucsb.edu/~atwater/ and download my animations. Enjoy.