Before PBO: An Overview of Continuous Strain and Tilt Measurements in the United States

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Abstract

Unlike Japan, the United States has not had a large sustained program to make continuous measurements of crustal deformation—until the the Plate Boundary Observatory (PBO) initiative (Section 10) that began in 2003. Most earlier measurements instead were initiatives by individual scientists or particular agencies. In many cases early and promising results have been followed by demonstrations that apparent signals were in fact noise. Partly because of this, few programs have lasted more than a few years, two examples being measurements with shallow strainmeters in the 1960's and shallow tiltmeters in the 1970's. I use some of these historical examples, and concepts from studies of scientific experimentation, to suggest some general conclusions about this type of measurement.

1 Introduction

In the spirit of the other papers in this volume, I review here crustal deformation measurements in the United States that have used strainmeters and tiltmeters, with particular focus on measurements aiming to observe deformations at longer periods caused by tectonic or volcanic motions, as opposed to Earth tides and seismic waves. I do not discuss instrument designs because these are covered in Agnew (1986), and few new designs have been introduced in the two decades since. I also do not describe results in any detail, partly for reasons of space, partly because details are often not available except in abstracts and technical reports and partly because a large fraction of the data produced is now thought to be noise rather than signal. While reviews usually ignore such dead ends, I summarize them because they influenced what paths were and were not later followed; I usually describe how results were viewed at the time, not how we would evaluate them now.

I have tried to go beyond the usual review paper in two ways. I have outlined the funding available for U.S. earthquake studies at different times; Geschwind (2001) gives the best history, which can be supplemented by Bates *et al.* (1982) for funding related to nuclear test detection. Because crustal deformation measurements need large initial investments

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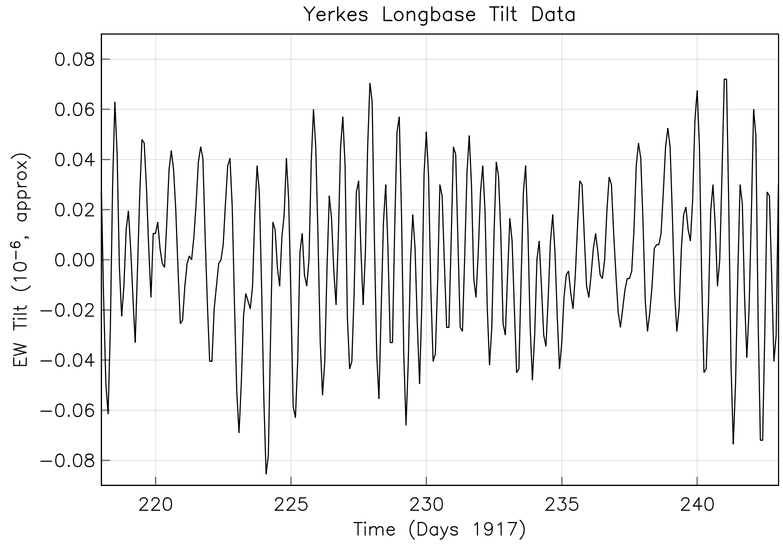

Figure 1: Tilts measured by the Michelson-Gale tiltmeter at Yerkes Observatory (42.571°N, 88.558°W). This tilt series is taken from a list of values of observed and theoretical tides preserved in the Michelson papers at the United States Naval Academy; no units or precise times are given, but comparing the theoretical tide to that computed by a modern program supports the times and units as shown.

followed by steady support for operations, funding patterns are very important. And, in the last two sections I have offered some conclusions from this history; inevitably, these reflect my own perspective on the field (as does, no doubt, the historical account); these conclusions should be viewed as tentative thoughts rather than definitive findings.

2 Beginnings: Michelson, Merritt, and Benioff

The idea that the ground might tilt in various ways, and also that these tilt variations might portend earthquakes, became a subject for scientific investigation in the nineteenth century (Darwin, 1881, 1882). The first meaningful measurements of tilts were however all in connection with Earth tides; the first in the United States was by A. A. Michelson and H. G. Gale between 1914 and 1917 (Michelson, 1914; Michelson and Gale, 1919). In response to a question by their geological colleague T. C. Chamberlain, they installed two 152-m pipes at Yerkes Observatory in Wisconsin, filled them halfway with water, and measured the water-level changes at the ends, first visually and later interferometrically. The results (Figure 1) were, even by today's standards, very good: an early illustration (though not the first) of the improvement in quality that could come from a long-base instrument. The resulting Earth-tide data, being not only of high quality but relatively unaffected by ocean loads, gave what was for a long time the best estimate of the Love-number combination for tilt; Jeffreys (1926) used this estimate as part of his demonstration that the Earth's core must be of very low rigidity.

The next attempt to measure tilts ignored this lesson of the value of length (whether intentionally or not is unknown). Starting in 1925, the government agency responsible

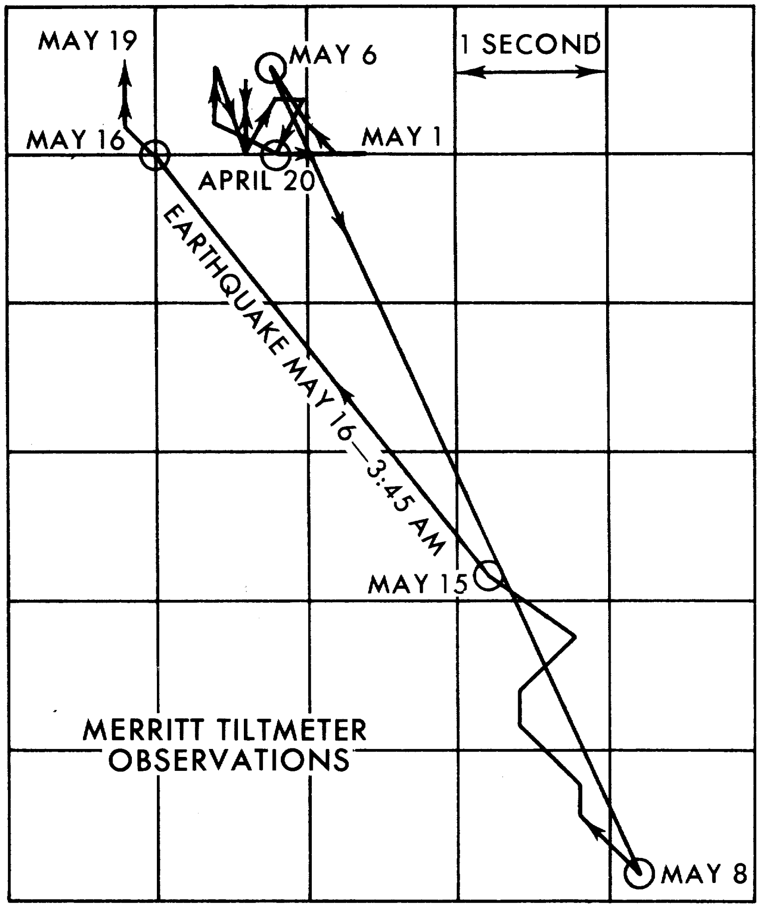

Figure 2: Vector tilt, from the Merritt tiltmeter at Berkeley, California, in April and May 1933, showing an apparent precursor to a magnitude 4.5 earthquake about 50 km away (Cloud, 1966). Note that 1 second of arc is 4.84×10^{-6}radian.

for seismology in the United States was the Coast and Geodetic Survey, which began an expanded program of measurements in 1932, even further expanded following the Long Beach (California) earthquake in March 1933. In 1931 K. Suyehiro had given a series of lectures in the United States (Suyehiro, 1932), describing possible precursory tilts observed in Japan with Ishimoto tiltmeters, though Suyehiro was careful to point out that "We are not sure, however, whether these tiltings were due to meteorological causes or to subterranean changes." In response to these reports, the Coast and Geodetic survey commissioned a new tiltmeter design from G. E. Merritt of the National Bureau of Standards (Heck *et al.*, 1936; Cloud, 1966): a Pyrex dish partly filled with oil, covered with an optical flat, and placed on a concrete slab at the bottom of a pit about 2 m deep. This was illuminated from above with monochromatic light to form an interference pattern, whose change as the dish tilted could be observed from above by a telescope. Four instruments were installed near the Hayward fault in Berkeley in 1933. In a pattern too often repeated, early results (Figure 2) appeared to show a precursory tilt.

Measurements over the next 25 years at these stations and others, gave no useful results;

as summarized by Richter (1958, p. 562):

> In California, failure seems definite; small, irregular tiltings, due probably to local causes are recorded but show no correlation with large shocks.

The final development of this early period was the invention of the strainmeter by Benioff (1935), who was at the only academic institution in the United States with expertise in seismic instrument design: the Seismological Laboratory founded by the Carnegie Institution of Washington (CIW), and later supported by the California Institute of Technology (Goodstein, 1984). Benioff applied his new variable-reluctance transducer to measure strain, with the idea of producing an instrument to measure very short period seismic waves more accurately than could be done with an inertial system. As more data accumulated, it became clear that one strength of this instrument was its ability to record waves with very long period, notably a possible free oscillation after the 1952 Kamchatka earthquake. In new instruments, Benioff therefore used fused quartz for the length standard (instead of iron pipe), added a transducer sensitive to longer periods, and installed the system deeper underground: in 1953 at Dalton, close to Los Angeles (Figure 3), and in 1956 at Isabella, to the north (Benioff, 1959). Both instruments showed large annual cycles, with long-term rates of 10^{-6}yr^{-1}.

3 Quartz-Bar Strainmeters: Bombs and Steps

The late 1950's and the 1960's brought huge changes to American seismology, largely through greatly increased funding; while Benioff's new instruments had been funded by private foundations and small grants from societies, this was the period in which the Federal government began to provide large sums for seismic methods for identifying nuclear explosions, through the VELA UNIFORM program. This program included basic research in many areas, including strain measurements, which also began to see funding from the National Science Foundation (NSF).

With these larger sums, and a strainmeter design available, several university groups built instruments in other locations. With support from the United States Department of Defense (DOD), Benioff's group put quartz strainmeters in Peru (as part of the International Geophysical Year) and in Hawaii (Benioff, 1965; Blayney and Gilman, 1965). In terms of later influence in U.S. strain measurement, perhaps the most important installation of a Benioff strainmeter was that made in a New Jersey mine (Ogdensburg, at 550 m depth) by Maurice W. Major at Lamont-Doherty Geological Observatory (Major *et al.*, 1964). In 1966 Major moved to the Colorado School of Mines, where he built a set of completely new strainmeters in purpose-built tunnels (in this case using private funds). With funding from the Coast Survey (now known as ESSA) he also designed a quartz-bar system with additional thermal compensation, which allowed it to be installed in a shallow trench, at much lower cost than a tunnel (Major, 1966).

At the same time as this apparent advance in instrument design, there was increasing interest in strainmeter data, for several reasons. Strainmeters had proven their value in recording the low-frequency signals from free oscillations, and in particular toroidal modes that could not be observed on vertical sensors. A natural extension was to look at the strain steps observed at the times of earthquakes. Press (1965), in a very influential paper, combined observations of such steps (largely on the Caltech strainmeters) with what was then the new idea of modeling coseismic deformations using elastic dislocation theory. Strain-step data, termed "zero-frequency seismology" (Press, 1969), seemed to give a window into the interaction of tectonic processes and sudden strain release; this interaction was of interest in the problem of identifying nuclear explosions because any tectonic strain released at the time of the explosion might produce shear that could cause the event to appear earthquake-like.

As a consequence of these developments, and of the beginnings of interest in earthquake prediction, the period from 1966 through 1972 saw a substantial increase in strainmeter development and deployment. This included not just quartz-bar systems but also laser strainmeters, which are discussed in more detail in Section 7. Inevitably, there was a competitive element present in terms of which type of system was "best"; some of this was captured in Sydenham (1974) and Levine (1975).

Much of the deployment of quartz-bar instruments centered around the nuclear test site in Nevada, where three different groups installed a few deep and many shallow strainmeters (Romig *et al.*, 1969; Shopland, 1970; Boucher *et al.*, 1971). A few underground tests, too large to be detonated in Nevada, were set off in the Aleutian Islands in Alaska, and the Colorado group installed numerous shallow strainmeters there (Romig *et al.*, 1972), which they used to detect not only strains caused by explosions, but other changes, including claims for large aseismic steps correlated over large distances (Butler and Brown, 1972). A network of shallow strainmeters installed in the Denver area (Major and Romig, 1969) also showed strain fluctuations, and secular rates as high as $10^{-5}\mathrm{yr}^{-1}$.

The Colorado group also observed strain steps from distant earthquakes on their original instruments, compiling their observations into a paper (Wideman and Major, 1967) that was widely cited. They claimed that the data showed much larger steps than could be explained by dislocation theory, and postulated some kind of propagating step, perhaps involving plastic deformation, traveling at about 3 km/s. Similar observations of propagating tilt steps (Berg and Pulpan, 1971; Berg and Lutschak, 1973) could be taken as confirmation of this; although the possibility of all these data being caused by instrument hysteresis was considered, these data suggested that strains and tilts from earthquakes involved some kind of novel physics.

Despite this activity, the use of quartz-bar strainmeters in the United States seems to have ended fairly abruptly in the early 1970's. Following Benioff's retirement in 1965 the Caltech instruments were kept running for a few years but eventually given lower priority than local seismic recording; the Colorado group seems to have ceased measurements in 1972, possibly because earthquake research was no longer being pursued by their funding agency.

4 Tiltmeters Again: Prediction, Creep, and Rain

Except for the Merritt tiltmeter, none of the measurements described so far were justified primarily (if at all) through their contributions to earthquake prediction, no doubt because few American seismologists thought this possible. But this began to change in the late 1960's and 1970's, as reports from other countries, especially China and the Soviet Union,

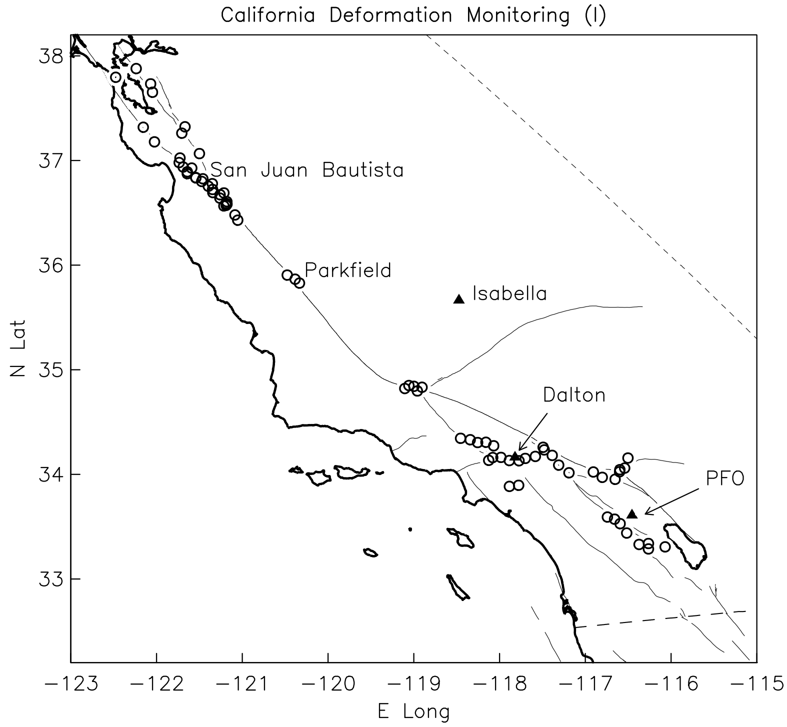

Figure 3: Deformation monitoring sites in California and Nevada, 1953-1980. The circles are locations of the USGS tiltmeter network as of 1976 (from Mortensen *et al.* (1977). The locations of the Benioff strainmeter sites, and of Piñon Flat Observatory (PFO), are shown by labeled triangles; other locations mentioned in the text are labeled.

suggested that successful prediction might be possible. In addition, having two destructive earthquakes in the United States (1964 Alaska and 1971 San Fernando) made enhanced funding of earthquake studies more important politically. Both of these earthquakes were followed by calls for large-scale research programs; while these were not put into practice, they did result in modest funding increases for two agencies: the Coast Survey (now called ESSA), and a new program established in 1965 at the United States Geological Survey (USGS); in 1973 the USGS program absorbed the ESSA effort. Additional scientific support for prediction came from the combination of apparently positive results for the dilatancy method of earthquake prediction (Scholz *et al.*, 1973) and the discovery of the "Palmdale bulge", showing apparent large-scale crustal motion (Castle *et al.*, 1976). By 1977 these results had led to a formal U.S. effort in earthquake studies, the National Earthquake Hazards Reduction Program, which greatly increased the funding available to scientists within the USGS and at universities.

USGS scientists had begun a modest program to measure tilt in 1970, using two mercury tiltmeters, and recording an apparent precursory tilt (Wood and Allen, 1971). Two factors combined to expand the program. One was the existence, for the first time, of a commercially available instrument: the bubble tilt system developed by Rockwell for aerospace measurements (Cooper, 1970), and sold for geophysical use by Kinemetrics, which superseded the USGS's own effort to design a borehole tiltmeter (Allen *et al.*, 1973). As with that

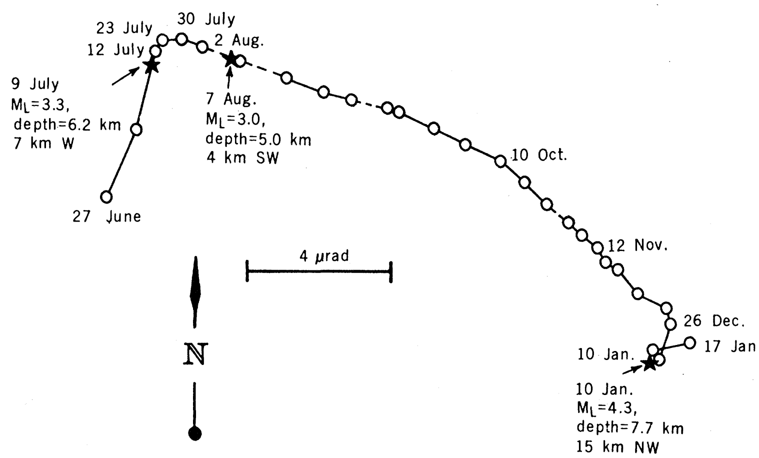

Figure 4: Vector tilt, from Johnston and Mortensen (1974), recorded at Nutting (36.824°N, 121.459°W), showing an apparent precursor to a magnitude 4.3 earthquake.

earlier design, the Kinemetrics system was installed in a shallow borehole, usually about 2 m deep.

This new system produced an immediate success by showing an apparent precursor to an earthquake near Hollister in 1973 (Figure 4 and again in late 1974 (Johnston and Mortensen, 1974; Mortensen and Johnston, 1976). The USGS tiltmeter network expanded rapidly over the next two years to include some 84 instruments at 77 different locations in California, with particular emphasis on the region of branching faults around San Juan Bautista, and around the Los Angeles region (Figure 3). Based on the data collected, claims began to be made of routine earthquake prediction using tilt precursors (Herriot *et al.*, 1976; Stuart *et al.*, 1976). The USGS also provided Kinemetrics tiltmeters to university investigators to use in other locations, including Alaska, the Caribbean, and the New Hebrides (Harrison *et al.*, 1979; Bilham *et al.*, 1979; Isacks *et al.*, 1979).

Unfortunately, these early positive results were not repeated. Indeed, it eventually became clear that they were almost certainly meaningless coincidences, though this conclusion was not reached easily. While the sensor itself was adequately stable (Johnston, 1976), increasing evidence was found that the tilts seen on shallow instruments were caused by soil motion driven by weather (Wood and King, 1977; Goulty *et al.*, 1979) and were much larger than any changes observed geodetically (Savage *et al.*, 1979) or by long-base sensors (Section 8). And, as described below, comparisons between closely-spaced instruments showed no agreement. All of these issues were discussed extensively at a two conferences. The first was the Sixth Symposium on Recent Crustal Movements, held at Stanford University in California in July 1977, at which Dr. G. C. P. King of Cambridge University, drawing on the historical review in Horsfall (1977), pointed out that the advantage of long-base systems had been repeatedly rediscovered–the case of Michelson and Merritt being in fact the third such instance. The second conference was a special meeting organized by the USGS in November 1978, on "Stress and Strain Measurements Related to Earthquake Prediction;" the summary of this (Evernden, 1979) illustrates the divergence of opinion, and the difficulty in deciding that what had appeared to be a quick and simple solution to the earthquake prediction problem was in fact not going to be. Several investigators continued to improve both the sensor electronics and installation methods (using deeper holes), but neither gave much better results (Morrissey and Stauder, 1979; Wyatt *et al.*, 1988). In late 1982 the USGS shallow tilt measurements were largely discontinued for earthquake studies (Mortensen, 1982), though continued in volcanic regions with much larger signals, such as Long Valley (Mortensen and Hopkins, 1987). Additional tilt monitoring was done at Mount St. Helens using a near-surface tiltmeter designed by Westphal *et al.* (1983).

5 Medium-Length Strainmeters

The other instrument design to appear around 1970 was the constant-tension wire strainmeter (Sydenham, 1969), which provided sensitivity and long-term stability comparable to the Benioff design, but was much easier to install. This instrument was further developed by the Cambridge University group in England (King and Bilham, 1976) and brought to the United States when two members of that group (R. Bilham and J. Beavan) moved to Lamont-Doherty in 1974 and 1976 respectively. Some of the original designs, using invar wire as a length standard, were installed near the San Andreas fault in the early 1970's and used to study the strains caused by creep events (Goulty and Gilman, 1978). While carbon fibers had been proposed earlier as a length standard, new types of these (Hauksson *et al.*, 1979) made this design more practical. Two of these instruments were first deployed near Cape Yakataga in Alaska (Bilham and Beavan, 1980), though noise and logistic difficulties meant that they were operated for only about two years. Starting in 1983 twelve additional instruments were installed at eight locations north of Los Angeles, mostly in mines and tunnels, including the Dalton site that had been used by Benioff (Leary and Malin, 1984); the instrument lengths were around 20 m. These installations gave useful records over short times, (see, for example, Ben-Zion and Leary (1986)), but showed longer-term variations driven by precipitation and annual thermal cycles; this network only operated for a few years, and was closed in 1986.

6 Deep Borehole Strain

The failure of the shallow tiltmeter network did not mean a loss of interest in looking for precursory signals, but by about 1980 had convinced most U.S. investigators that deformations could not be measured well unless the instruments were either made very long (discussed in the next section) or very deep. Unfortunately, either of these meant "more expensive", which was a problem given the trend of NEHRP funding over the years: steadily downwards in terms of real purchasing power, though with occasional temporary increases following damaging earthquakes. And, in 1992 the USGS program was divided into regions, making it less California-centered than before; most of the areas given new prominence were places where intensive strain or tilt monitoring was not so appropriate.

Though a number of designs for borehole tiltmeters had been developed, and some (notably the Askania instrument) brought to limited commercial production (Rosenbach and Jacoby, 1969), by 1980 no such instruments were readily available; in addition, the requirement that they be installed in cased boreholes made them somewhat problematic for use in measuring tectonic tilt. Fortunately, a borehole strainmeter was available, namely the dilatometer that had been developed by Sacks and Evertson at the Carnegie Institution of Washington (CIW) using internal funds. Sacks had been interested in obtaining high-quality nearfield records of earthquakes (Sacks, 1966), particularly of coseismic strain steps. In 1965 he proposed the idea of a hydraulic system, cemented into a borehole, to D. W. Evertson, an engineer at the University of Texas who was developing seismic applications for the "solion", a sensitive electrochemical pressure sensor (Hurd and Jordan, 1960) then enjoying a temporary vogue. Evertson and Sacks used this sensor in a dilatation-rate borehole strainmeter installed at CIW in 1968, with promising results that led to the development of a borehole dilatometer that used silicone oil for the hydraulic fluid, and different transducers. The first example was again installed at CIW, with the next three being put in a small array in Matsushiro, Japan, in 1971 (Evertson, 1977). Successful use of this instrument depended not only on the design of the strainmeter, but also on careful study of the type of expanding grout to be used in cementing it, and the installation procedures. Tests with small explosions nearby showed no signs of spurious strain steps (Sacks *et al.*, 1971).

On the basis of the Matsushiro results, the JMA (starting in 1976) installed borehole strainmeters in the Tokai area, which provided a precedent for their use in the United States. Between 1980 and 1983 the first twelve CIW instruments were installed in California: four in a profile in the Palmdale area, two in Parkfield, three at Piñon Flat Observatory as part of an intercomparison project (Wyatt *et al.*, 1983), two in San Juan Bautista, and one in Long Valley for volcano monitoring. A number of these early installations failed after a few years because of a poorly designed cable seal; subsequent installations have had lifetimes of (at least) 10 to 20 years. The next round of dilatometer installations was from 1984 through 1987, when six more instruments were emplaced as part of the Parkfield Prediction Experiment; in 1992 six dilatometers were put in the San Francisco region as part of the Hayward Fault Surveillance Project, which built upon the successful deployment at Parkfield. After the 1994 Northridge earthquake, a pair were installed in 1996 just north of Los Angeles, for a total of 27 installations in California; in 2000 four dilatometers were set up at Kilauea and Mauna Loa volcanos in Hawaii. Myren and Johnston (1989) and Myren *et al.* (2006) describe the procedures used for installing and operating these instruments.

The other borehole strainmeter available during this period was the multicomponent system developed at CSIRO in Australia by Gladwin (1984), originally for measuring strains induced during mining (Gladwin, 1977), with a transducer derived from the capacitive system of Stacey *et al.* (1969). Since this instrument could measure changes in all components of horizontal strain it became known as the Gladwin tensor strainmeter (GTSM). The first two systems were installed in 1984 at PFO and in San Juan Bautista; the same initiatives that led to the dilatometer installations brought three GTSM's to Parkfield in 1986-87, two to the San Francisco region in 1993, and one to southern California in 1996.

The final set of borehole strainmeter installations, though made before PBO, was in part done as a trial run for it: the "mini-PBO" was an NSF-funded project to add more strainmeters and continuous GPS to the San Francisco area, and GPS to Parkfield (Murray *et al.*, 2002). The strainmeters installed were built by CIW, but used the sectored-chamber system introduced by Sakata and Sato (1986) to measure shear strains as well as dilatations. Five such instruments were installed in 2002, bringing the total number of deep borehole systems installed over two decades to 40, of which 27 are currently operating (Figure 5).

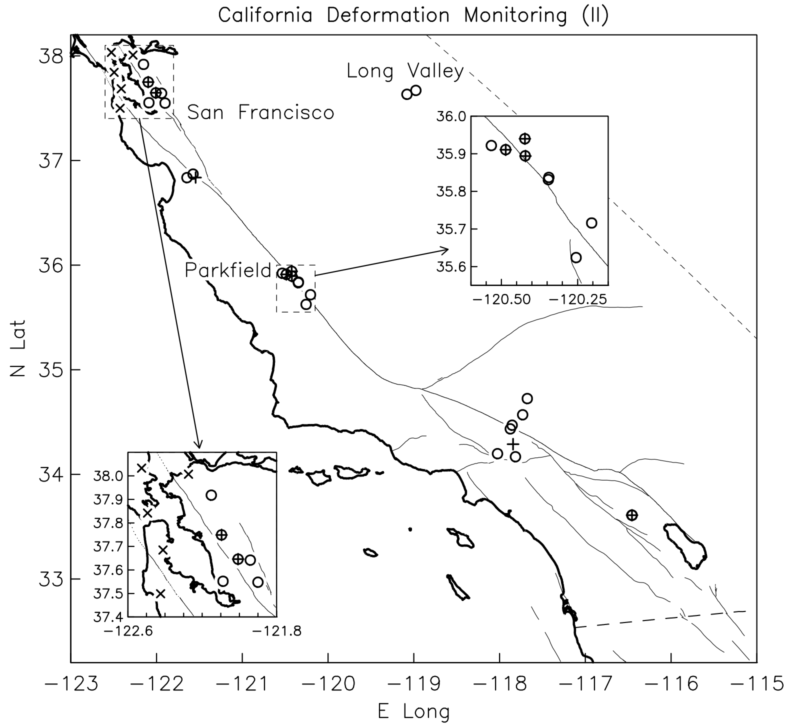

Figure 5: Borehole strainmeter sites in California, 1981 to the present. Open circles show where dilatometers were installed (not all are still operating); filled circles are locations of Gladwin tensor strainmeters (all operating but the one at PFO), and crosses are locations of the "mini-PBO" Sakata-type instruments installed in the San Francisco Bay area. Inset plots show the San Francisco and Parkfield regions in more detail.

Unfortunately, the larger earthquakes during this period were, with two exceptions, not in the areas in which these instruments were clustered. The first exception was the 1989 Loma Prieta earthquake, for which Gladwin *et al.* (1991) found a medium-term precursor in GTSM data, though later analysis (Roeloffs, 2006) suggests that this might simply be unmodeled drift. The second, of course, was the 2005 Parkfield earthquake for which no strain precursors of any kind were detected on any of the numerous systems installed (Johnston *et al.*, 2006). The Parkfield result extended the findings of Johnston *et al.* (1987), in showing an absence of any short-term strain changes before a number of moderate earthquakes; the one possibly positive result for longer-term precursors is from the 1984 Kettleman Hills earthquake (Roeloffs and Quilty, 1997). That precursory signals are rare may be a negative conclusion from the standpoint of prediction, but it is a positive one in terms of constraining models of fault failure.

Borehole strainmeters in California have also detected aseismic strain events in two locations: near San Juan Bautista, several events between 1992 and 1998 (Linde *et al.*, 1996; Uhrhammer *et al.*, 1999); and at Parkfield, an event between 1993 and 1998, first detected by a GTSM record (Gwyther *et al.*, 1996) but for which the most evidence comes from 2-color EDM data (Langbein *et al.*, 1999). Both these aseismic events are at locations where part of the fault motion is released as earthquakes, and part through creep: a pattern seen only for a small part of the San Andreas, and perhaps a few other faults.

7 Longbase Strain and Tilt: Anchoring

One of the puzzles of crustal-motion measurement is why Michelson's early design for a long-base tiltmeter has been so rarely repeated; what liquid tiltmeters were used in the United States tended to be variations on the mercury pot-and-tube system of Gile (1974), originated by Benioff, which can only be used in locations with stable temperatures; a commercial version of this, built by the Ideal Aerosmith company, has monitored Kilauea volcano in Hawaii since 1965. It seems to have remained for Bilham *et al.* (1979) to revive this system, making the point that for the tubes to be half-filled required only that they be level, but not necessarily straight. Before 2002 only three such instruments had been constructed in the United States. At PFO a two-component system with an EW length of 535 m (later extended to 651 m) and a NS length of 544 m was built in stages between 1980 and 1989; in 1980 a 535-m system was set up in parallel with the EW system for a test described in the next section. The third longbase tiltmeter is a two-component instrument in Long Valley that has been operating intermittently since 1987 for volcano monitoring. Starting in 2005 three more Michelson-type instruments have been installed in the Pacific Northwest to monitor tilts associated with the episodic slip events detected using GPS (Suszek *et al.*, 2004).

Strain measurements over a truly long baseline, unlike those of tilt, were not possible until the development of the laser. The first U.S. attempt at a laser-based strainmeter seems to have been that of Vali *et al.* (1964); this group installed a number of instruments between 1965 and 1970, culminating in a 1-km instrument in an abandoned railroad tunnel (Vali and Bostrom, 1968), though few of these installations seem to have produced any significant data. Much better results came from a 30-m system in Colorado (Levine and Hall, 1972), which combined a very sensitive interferometer (originally built to measure the velocity of light) with some of the first atomically-stabilized lasers. This instrument ran between 1971 and 1977, producing strain records with very low noise from subtidal to seismic frequencies (Berger and Levine, 1974; Levine, 1978).

A bolder approach was that of R. H. Lovberg in California. At the suggestion of W. H. Munk and J. F. Gilbert, and soon joined by J. Berger, he used support from the Coast Survey earthquake program to design a very long strainmeter using a Michelson interferometer. Initial tests of a 200-m system in 1967 allowed the extension of the instrument to 735 m in 1968, and the addition of improved laser and optical-path stabilization (Berger and Lovberg, 1969, 1970). The boldness lay in building this strainmeter at the Earth's surface: a surface installation, though not without its challenges, provides much more flexibility in siting than an instrument that must be installed in a tunnel. This design has been the only type of laser strainmeter to produce really long spans of data. The further development of this system, and some results from its installations, are described in some detail in Agnew and Wyatt (2003). The 735-m laser strainmeter (LSM) was a testbed installation near San Diego, which operated intermittently between 1968 and 1973. The next three LSM's, 720 m long, were installed at Piñon Flat Observatory between 1971 and 1973, and are still operating. These first installations were made with a mix of funding, including ESSA, NSF, DOD, the USGS, and NASA; as described below, studies of data from these instruments led to a series

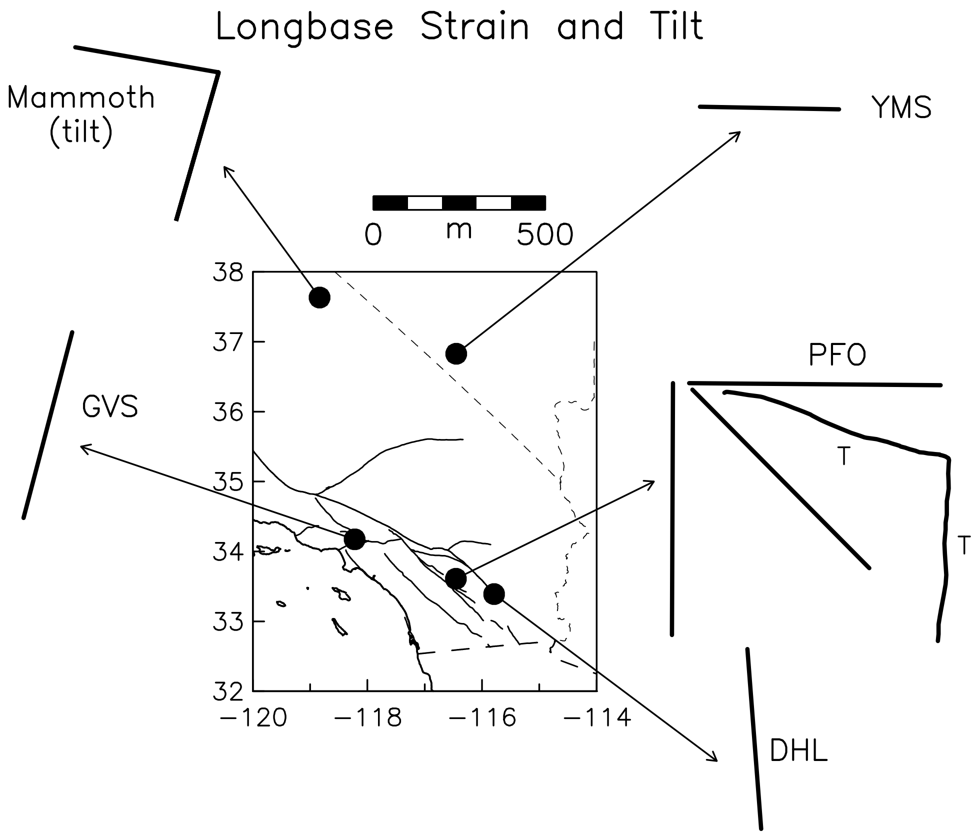

Figure 6: Longbase strainmeters and tiltmeters in California and Nevada at the end of the second millennium. All instruments are plotted on the same scale. The two curves marked "T" at PFO are the paths of the longbase tiltmeter pipes.

of improvements at the same site. Because of increased interest in the seismic hazard from the southernmost segment of the San Andreas fault, the USGS provided funds to build a single LSM there (DHL) in 1994; additional funding available following the 1994 Northridge earthquake (much of it from a private foundation) supported construction of another (GVS) in Los Angeles, in 2002; at the same time, a sixth such instrument (YMS) was installed to monitor strain at a site (in Nevada) proposed for radioactive waste disposal, though this measurement was ended in early 2007 because of budget cuts to the operation of the tunnel it is in. Figure 6 shows the locations of these different instruments; as with the borehole strainmeters, more are being installed as part of the PBO: one additional instrument at DHL, two more to the west of this, and two others just south of Parkfield.

Relatively early in the operation of these instruments it became clear that many of the fluctuations seen were correlated with rainfall, which led to the idea that the driving mechanism was soil motion, best eliminated by tying the measurements to depth. Using vaults a few meters below the surface gave poor results; much better ones were gained by the "optical anchor" (Wyatt *et al.*, 1982a), which used interferometry to tie the measurements to 24 m depth. Successive installations of such anchors at longbase strainmeters and tiltmeters have invariably decreased the noise level. Figure 7 shows a recent example. The best (but also most expensive) technique is again optical interferometry through an evacuated pipe.

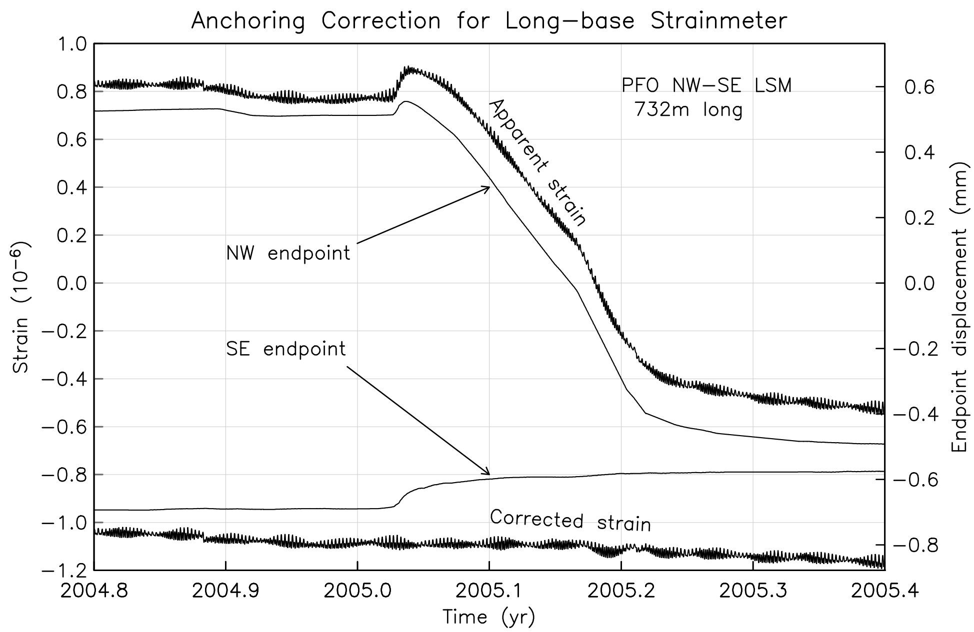

Figure 7: Strain on the NWSE laser strainmeter at PFO, at a time when prolonged heavy rains caused significant soil motion. The top trace shows the apparent strain between two endpoints (concrete piers at 3 m depth in vaults); the next two are the motions of these endpoints as measured by the optical anchors, and the bottom trace the corrected strain.

In strainmeters (but not tiltmeters) the endpoint anchoring uses interferometer arms of the same length, which allows optical fibers to replace the vacuum pipes, which in turn reduces the construction cost (Zumberge and Wyatt, 1998). A direct physical connection (a mechanical anchor) is the least expensive but also gives the noisiest results; such physical anchoring is the basis for the drilled-braced GPS monument now in common use in the United States (Wyatt *et al.*, 1989; Langbein *et al.*, 1995; Wyatt and Agnew, 2004).

To show what such anchoring makes possible, Figure 8 presents the data from the fully-anchored NS strainmeter at DHL. The instrument not only shows tides and short-term changes, but its long-term rate of -0.31×10^{-6}/yr, is comparable to the secular strain rate estimated from geodetic stations nearby and from dislocation models of the local fault zones. Even in the poorly consolidated material (Pleistocene sediments) around Durmid Hill, this instrument records the secular strain. Parts of the strain record show a small annual cycle: 0.035×10^{-6}, with a phase of 240° relative to the 10.7°C annual cycle of the local air temperature. Other fluctuations are very small, the largest being occasional changes, lasting less than an hour, thought to be from creep events on the fault nearby. The primary result from these long-base measurements has been to show, in a few locations, that the secular strain accumulates fairly steadily; while postseismic changes have been observed, no preseismic signals have been found.

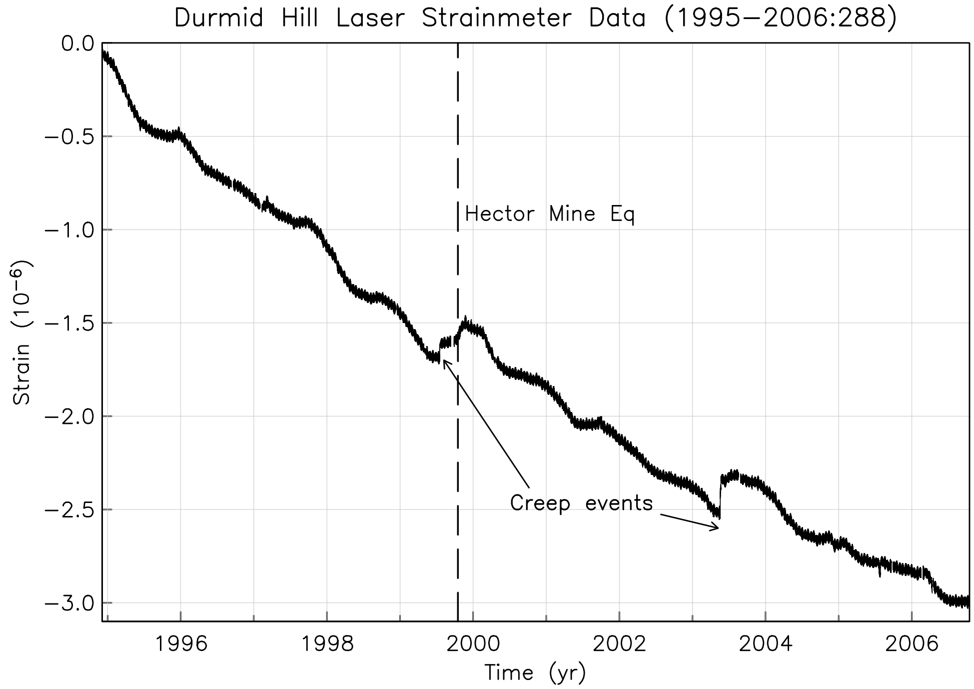

Figure 8: Data from the laser strainmeter at Durmid Hill (33.392^{c}ircN, 115.788^{c}ircW) about 1.5 km from the San Andreas fault, and oriented at 45° to it. The trend is caused by local tectonic strain accumulation; the thickening of the trace is the Earth tides.

8 Instrument Comparisons

In a very early paper on aseismic movements of the Earth's crust, Milne (1896, p. 235) wrote

> Therefore, until levels or horizontal pendulums have been established in duplicate in the suggested manner upon rock, we shall be unable to say that secular movement has been instrumentally measured or recorded.

Unfortunately, such duplicate installations have been all too rare, especially ones done *in situ* rather than just tests of the sensor. Sensor tests are necessary but not sufficient: they are needed during development, but because of the importance of coupling to the Earth, are largely irrelevant to actual performance.

In a setting where costs are limited, there is a natural reluctance to support side-by-side tests because it seems like redundancy. This is made worse by what might be called the "favorite-spot syndrome". Very often, the builder of new instruments usually has in mind some location thought to be especially likely to yield interesting results. This can easily lead to different instruments being run, each for seemingly good reasons, in different places. If the results differ, it is almost impossible to tell if differences in results reflect differences in design or geographical differences in the way the earth behaves: what may appear to be an optimal approach of covering all the bases can in fact lead to inconclusive outcomes. The only solution is to run instruments so close together that there is no reason for them not to record the same thing.

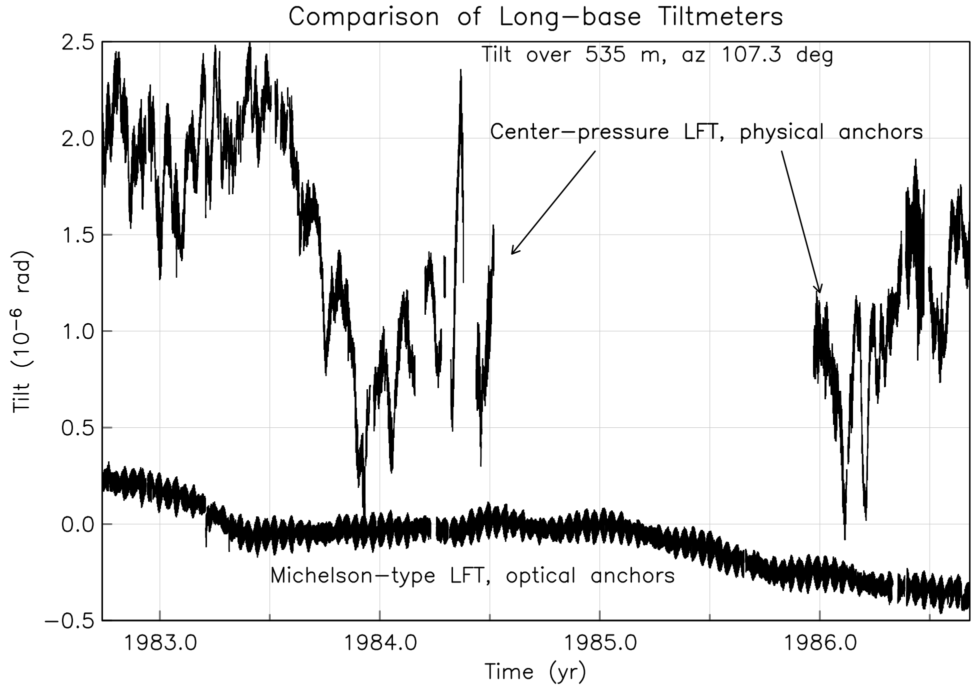

Figure 9: Time series of tilt from two longbase tiltmeters at Piñon Flat Observatory: a Michelson-type longbase tiltmeter with optical anchors (LFT), and the center-pressure system designed by Horsfall and King (1978), with physical anchors. The end points were in vaults about 10 m apart, but the tiltmeter pipes followed nearly the same path.

One early example of such a comparison was in 1972 at PFO, between the shallow quartz-bar instrument designed by Major (1966) and the long-base laser strainmeter of Berger and Lovberg (1970). Over three months the drift rate on the laser system was 0.05 that of the quartz instrument (Lee and Brown, 1972), which was abandoned as a result. The next test, also at PFO, was a comparison of the shallow borehole tiltmeters used by the USGS program (Wyatt and Berger, 1980), which showed no coherent signals and much larger fluctuations than observed on long-base instruments at this site. Partly in response to the failure of the tiltmeter program, the USGS then sponsored a comparison of systems at PFO, which extended from 1981 through about 1986. Results included comparisons between different tiltmeters (Wyatt *et al.*, 1982b, 1984) and also between borehole and longbase strain (Wyatt *et al.*, 1983, 1994; Hart *et al.*, 1996). Another comparison between borehole tiltmeters by Kohl and Levine (1993, 1995) showed how much strain-tilt coupling effects could differ even for nominally similar installations.

Figure 9 shows a comparison between two longbase tiltmeters sponsored by the USGS program: The center-pressure system is much noisier; because it shared anchoring with another Michelson system, we can say that this noise is caused by the tilt measurement (in fact sensitivity to temperature), not the anchoring. (A fourth system that used two fluids (Huggett *et al.*, 1976; Merchant *et al.*, 1987) was also installed but never worked properly.)

Perhaps in part because of the results of these comparisons, and also because of steadily declining budgets, few additional strainmeters or tiltmeters were installed in the United States after 1987, and none of new design. This was also the time when GPS appeared on the scene, with the first geophysical measurements being made in 1986, and the first continuous network beginning around 1990. The ease and flexibility of use of GPS, and its rapid development, attracted some existing practitioners of strain and tilt measurement to pursue it, and also tended to define crustal deformation for scientists entering the field. By 2000 there were fewer than ten scientists in the United States working on strainmeters and tiltmeters and data from them; indeed, one of the challenges for the PBO project is to create, not just a network of strainmeters, but a community of researchers to work with the data.

9 Some General Reflections

What lessons can we draw from the foregoing account—and in particular, what might we identify as the causes for the overall pattern, and do these portend any particular future direction? Based on my own experience, thinking about these questions is enriched by concepts introduced by sociologists of science, particularly Collins (1986, 2004). The second reference, which describes the search for gravitational radiation, contains many parallels to tilt and strain measurements. One is that gravitational radiation detectors are also strainmeters, though the strains to be detected are much below those in geophysics: 10^{-21} or smaller. Initial efforts to detect this radiation with resonant bars gave results eventually deemed implausible; subsequent measurements have used bars at cryogenic temperatures, and most recently large laser interferometers, culminating in the LIGO project, a pair of 4-km laser strainmeters—which are as isolated as possible from the Earth. A strong similarity between the search for astronomical sources of gravitational radiation, like the search for earthquake precursors, is an attempt to observe rare phenomena—no one is sure how rare. Certainly the following statement, from Collins (2004, p. 420) will seem familiar to anyone who has worked on tilt and strain measurement:

> All the ingenuity of the scientists has been directed at struggling to keep going while trying to do the impossible rather than regularly doing the possible. Any signals have to be extracted from noise using any and every means; the problem has not been to drive away false signals but to maintain some grounds for hope. (Collins, 2004, p. 240).

The most immediately relevant points from the sociology of science come from Collins (1986), an examination of how experiments are done, in which he introduces the term "experimenter's regress", to describe the problem of deciding when an instrument is working. This cannot be done without reference to what results are expected. But these expected results depend in turn on the output of some other piece of equipment, which has to be supposed to be working, and so on; in principle we could end up with a chain of error, where bad results from one system lead to bad results from another, and no certainty that our final results are any good. While it has been disputed how much of a problem this really is, (Franklin, 1990), this notion offers a useful way to think about problematic instrumentation. In laboratory situations one way around the regress is to isolate the instrument; this is not possible in strain and tilt measurement. Getting instruments to agree does not prove they are correct, since they might be measuring the same noise source; but disagreement is a sure sign of trouble.

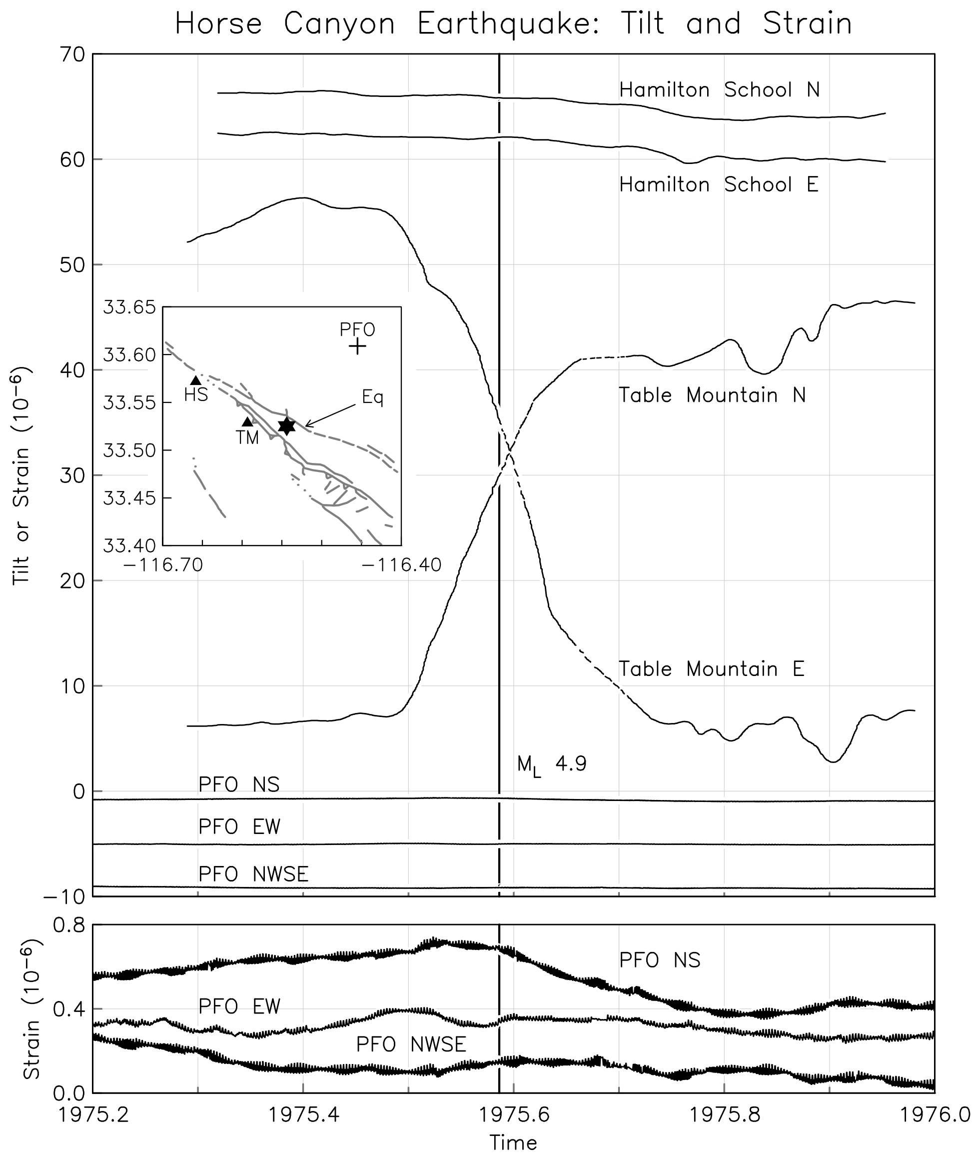

Figure 10: Tilt and strain at the time of the Horse Canyon earthquake (M_L 4.9, 1975:214:0:14, 33.525°N, 116.544°W), as recorded on two shallow borehole tiltmeters of the USGS network, and three longbase strainmeters at Piñon Flat Observatory (PFO). Inset map shows location of earthquake (star) and sensors (triangles). In the upper panel the strainmeter data appear to be straight lines, so these are re-plotted at an expanded scale in the lower panel. Given the depth to the earthquake (14 km), the distances from the earthquake hypocenter to the tiltmeters are 19 and 15 km for the Hamilton School and Table Mountain sensors, and 18 km to the PFO strainmeters. Tiltmeter data redrawn from Berger *et al.* (1979).

Another way to break the regress is to rely on theory; Collins (2004) shows the importance of cosmological theory in ruling out early observations of gravitational radiation. Another challenging aspect of the measurement of strain and tilt is how weak, over most of the time considered here, the theory has been. Compared with (say) seismology, where the theory of elastic wave propagation provides clear constraints, our theoretical preconceptions about long-period crustal motion are weak—and for much of the period discussed here were nearly nonexistent. Such theoretical weakness makes it possible to interpret the same data in very different ways. An example is the data shown in Figure 10, where a shallow tiltmeter appeared to show a large precursory tilt to a magnitude 4.9 earthquake, but longbase strainmeters not much further away showed no changes as large as 10^{-3} of the tilt signal. One interpretation is that the tiltmeters are very noisy and the precursor just a coincidence; another, made at the time, was that:

> No matter how you view the tiltmeter data for the Anza earthquake, you must admit failure to detect a premonitory strain at Pinon Flat. If one accepts both the tiltmeter and the strainmeter data, one concludes that the regions in and near major zones of strike-slip faulting are grossly inelastic, so that detectable strain events which are easily detectable in and near the fault zone decrease in amplitude very rapidly away from the fault. Other data accumulated over the years seem to support this interpretation. (MS letter, J. F. Evernden to J. Levine, July 28, 1977)

Part of the basis for now regarding the tiltmeter signals as spurious comes from our increased willingness to take a particular theoretical view, namely dislocations in nearly uniform materials, as a valid model for deformations; this would demand approximate equality between tilts and strains. The adoption of this theory has not, of course, occurred in a vacuum; rather, other results, from seismology and geodesy, have made scientists more willing to consider it applicable to any new signal. The credibility of large changes in strain in California were reduced as widespread EDM measurements failed so show clear departures from steady accumulation, whether with a single-color system with error about 3×10^{-7} (Savage, 1983; Savage and Gu, 1985; Savage *et al.*, 1986; Savage, 1995) or a two-color system with error about 10^{-7} (Langbein *et al.*, 1982, 1987, 1990). GPS and InSAR data have only confirmed this; ironically, InSAR data do suggest compliant fault zones (Fialko *et al.*, 2002), though not to the extent that would explain the earlier tilt data.

Only some of the challenge of this field comes from the difficulty of determining instrument performance in measurements of phenomena that cannot be replicated at will, are limited in both space and time, and for which theory offers little guidance. A more mundane, but equally crucial, difficulty comes from the challenge of funding long-term observations: a problem not at all unique to strain and tilt measurement, as is shown by the remarks of the oceanographer Henry Stommel (Stommel, 1958, p. 136) on observing fluctuations in ocean currents:

> Very little is known about such fluctuations. It takes years of careful and expensive observation to produce even a very crude description of them. The scientific programs of our ... institutions are not geared to long-term problems of this kind; there is much pressure for novelty, much temptation to follow the

> latest fad, and a persistent though erroneous notion that all worth-while problems will eventually be solved by some simple, ingenious idea or clever gadget.

Stommel's comments of course reflect most strongly the American approach to research funding, which is dominated by investigator-driven projects funded (usually) for short times and in a very competitive way. There are certainly advantages to this method: it provides a great deal of flexibility and can ruthlessly eliminate programs that are not "productive" (however this may be defined). However, this approach carries two dangers with it. The most obvious is that it is very difficult to sustain measurements of long-term phenomena—that is, changes that take place over many fiscal years. Most long-term measurements are not, in fact, made for research, but as a byproduct of something else: we have climate data because of the daily needs of aviation, and seismicity catalogs (in part) because we need to know what is happening when a large earthquake occurs. Strain and tilt measurements, lacking such a dual purpose, can be very hard to justify year after year.

Another problem with the proposal-driven system, perhaps less obvious, lies in the pressure for novelty mentioned in Stommel's comment. In earthquake studies such pressure is another contributor to the favorite-spot syndrome mentioned above: a proposal that can make a plausible argument for studying a new area can easily be favored over one that aims to continue in the same place. At its worst, this leads to substantial inefficiencies, as instruments are installed in some new area only to be abandoned a few years later. One example of this (certainly not the only one) was the installation of deformation monitoring systems at Cape Yakataga, Alaska, around 1980, something in large part justified by the analysis of McCann *et al.* (1980), which argued that this region was a seismic gap whose low current seismicity suggested the likelihood of a major earthquake in the next decade; in fact, 25 years have passed and the seismicity has remained low. While outcomes like this cannot be avoided, they can certainly be minimized by realizing that, since our ability to foretell seismicity is limited, our observations are likely to take longer than we think, and we should not compound the problem by attempting observations in areas with especially difficult logistics. The most outstanding example of a prediction with too small an initial uncertainty is of course Parkfield; that the measurements started because of this prediction were sustained was owing in part to their relatively low operating cost, and also in part to much of the program being within a government agency, somewhat free from the proposal pressure that applies in U.S. universities.

On the other hand, we must recognize that strain and tilt measurement has been helped by its association with earthquake prediction. In another of Collins' terms, this field can be considered Pascalian science, after a theological argument by Pascal: if the potential benefits are very large, the activity is warranted even if the chances of success appear to be low. Balancing this reasoning against the need for continuity and fiscal constraints can make decisions to sustain observations quite difficult.

The question of dual use brings up another point about how funding outside of geophysics has affected this field. Other things being equal, technical advance in a field will be faster the more money is put into it; the fastest way for this to occur is for there to be military or large-scale commercial application. The high precision of GPS is an unplanned benefit from a system designed initially for military navigation (which provided of order $\$10^{10}$ to develop, build, and maintain the system) and subsequently adopted for many civilian uses (which sparked investments of probably $\$10^{8}$ by receiver manufacturers). And, all segments of GPS—satellites, receivers, and the computers needed to process the data—are dominated by microelectronics, for which the rate of improvement (Moore's Law) has been much faster than for any other technology. Over 20 years the price of a GPS receiver has fallen by a factor of 40 in real terms, while at the same time becoming vastly more powerful. This rapid change, as welcome as it is rare, has been what has moved measurement of long-term deformation from an unusual to a common activity.

None of this has applied to strain and tilt instrumentation; aside from a few geotechnical measurements, mostly at lower precision, there is no one outside of geophysics who needs this type of data. Any development funds have had to come from the same source as the research funding, and so have rarely allowed many improvements to be made, and almost never allowed the kind of investment in mass production that can lower manufacturing costs. In any case, these measurements rely largely on technologies that have not evolved rapidly, especially for long-base and borehole instruments, for which a significant part of the cost is construction in the field, including drilling boreholes.

Consideration of costs raises one final point. The costs of building a system for long-term observation should be considered as capital investments; over the life of the instrument, they usually do not exceed the cumulative operating costs, and may be much less. But the U.S. research funding system usually does not distinguish between capital and operating costs, Since they both come from an investigator's budget, there is a substantial incentive to keep the initial costs low, which has been a motivation for seeking simple and inexpensive approaches. Often this has turned out to be a false economy, either because of higher operating costs or the problem that an inexpensive system could not detect any signals—in which case it doesn't matter how cheap it is. Fortunately the funding structure of the Plate Boundary Observatory separates capital and operating costs—allowing, for the first time in the U.S. program in this field, a deployment of measuring systems that has a good chance of returning a large volume of results.

10 Looking Ahead: Lessons for PBO

The Plate Boundary Observatory (http://pboweb.unavco.org) is an NSF-funded project to install about 850 permanent GPS systems, 100 GTSM borehole strainmeters, and five laser strainmeters along the Pacific/North-American plate boundary in the United States, focusing on the San Andreas system and the Cascadia subduction zone. The PBO also is instrumenting five volcanic centers with continuous GPS and tiltmeters. The "capital investment" phase of PBO covers a five-year period for installation; this began in October 2003. In October 2008 the PBO will move into an operations and maintenance phase; this is planned to last for 10 years. (NSF does not, in general, support long-term monitoring). The PBO project also supports many of the continuous GPS sites constructed under earlier initiatives, such as a SCIGN network in southern California, which includes one laser strainmeter.

How do these plans for the PBO look in light of the past history of strain and tilt measurements? Obviously, opinions will differ, but it is clear that the PBO has several advantages, at least for the GPS part. There are certainly signals to measure—or, to put it another way, we can be confident that a large part of the changes in the GPS series will be from tectonics and not something else. In addition, the GPS data have a strong dual-use component, since they are in demand from surveyors who need a base network of permanent stations. Hopefully, this extra usefulness will make it easier to keep the GPS network running past the 10-year lifespan that is envisaged by the science plan—though, given the decentralized U.S. system of government, working out how to fund such a continuation remains a challenge.

On the other hand, it seems likely that this one-decade operation will be too short to advance the field as much as is hoped. Since 1900 the rate of magnitude 6.5 and larger earthquakes on land in the western United States has been about one every two years—though there have been spans of up to eight years with no such earthquakes. Given that PBO is concentrated in this region, it seems quite likely that even after ten years there the system will not have reached diminishing returns. Maintaining the GPS portion of the PBO will pose a challenge, but continuing the strainmeter measurements will pose a larger one. This will not just be a problem of funding. Part of the reason for the small size of the current U.S. strainmeter community is that the last few decades have not brought the kind of substantial new results that encourages students to enter the field; also, the case that many of those involved have not been at universities. If the practitioners do not have students who enter the subject, the field will cease to exist; Collins (2004) shows how this has happened to several branches of gravitation radiation measurement. There is a circular effect here, where lack of results can engender a lack of new measurements, without which no new results can be found. This can lead to the permanent end of the field, since without a steady number of practitioners it can be as difficult to transfer technology over time as from one location to another. One way out of this circle is for the measurements to either become much easier or to find a new use, both of which, thanks to GPS technology, have taken place for long-term crustal motion measurements. Neither course is likely for strain and tilt observations, and only the future will tell how much the intervention by PBO, to add many new measurements, will give this field a more central role in understanding how and why the Earth deforms.

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