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ORAL HISTORY PROJECT

Interview of Timothy M. Brown

8/24/2004

Interviewer: Patrice Pazar

Pazar: I had read that you built a telescope when you were a teenager?

Brown: I had finished my first telescope when I was 13 – 14 years old. I was a junior high school student in Texas. My dad and I started it (telescope) as a project. We got up to the polishing and configuring stage of the mirror and it became clear that it was something for one person to do. Took along time….a year and a half from start to finish. Very crafty technology…in your basement…that amateur astronomers have been using since the 1920s. We bought a kit from Edmund Scientific for $27.00 for an 8" mirror, tool and a bunch of abrasives, pitch and jeweler's rouge. It sat untouched for months at a time but eventually it ended up as a pretty nice mirror.

Pazar: You had built an observatory for this telescope?

Brown: We had moved to Illinois, and built an observatory that was 8' across and 8'high. It was a rotating octagon. It sat on a friend's farm at the edge of town…..a philosophy professor.

Pazar: Your father was a professor?

Brown: Yes.

Pazar: What subject (s) did he teach?

Brown: English literature, mostly. But he, also, had degrees in chemistry and meterology.

Pazar: Did you make another telescope?

Brown: I made a smaller telescope when I was in High School. Seems every ten yars I get the urge to grind glass again.

Pazar: Did you enter any of the telescopes into any science competitions?

Brown: There was a project that I did with my 8” telescope in the Science Fair when I was in ninth grade. I was taking pictures of the moon. They (pictures) came out okay and it (telescope) did okay. I don’t recall that I went to state.

Pazar: At age 14, you were interested in astronomy?

Brown: I was hooked on astronomy when I was younger than that….10 or 12. My brother had a 4” telescope that he had made with a kit that came with a ready-made mirror. I saw a lot of sky through his telescope when I was pretty young. It was the Sputnik era. I was 7 or 8 when Sputnik flew. I got entrained in science and technology and spaceflight and astronomy. ‘We will be commuting to the moon in 15 years’ kind of mind set.

Pazar: Where did you go to undergraduate school?

Brown: I went to Wesleyan University in CT.

Pazar: Why did you choose that school?

Brown: I was looking for a small liberal artsy place with a good astronomy program. When I got there, I found that I got along better with the physicists than with the astronomers. Astronomy at Wesleyan, at the time, meant Parallax work in the ‘old style’. So it was done photographically with long focus refractors. As practiced then, it was immensely labor intensive and, if I may say so, paralyzingly dull. Something I didn’t care for.

Pazar: You started working with physicists?

Brown: Yes.

Pazar: Who was your mentor?

Brown: Jim Faller who was a physicist. Most of what he did had to do with astronomy. When I first got mixed up with him, he was busy doing laser ranging to the moon. He was the primary investigator, where the astronauts carried arrays of retro-reflectors to the moon. Set them (reflectors) up pointing towards the earth and fire laser points from earth that would return enough photons from this reflector that you could measure the round-trip time. Measure the distance between your total scope on the moon with an accuracy of an inch. Great test of special relativity and general relativity. All about measuring small effects. He (Faller) had already done his first measurement at Lick Observatory. His next big thing, took years to come to pass, was the Fly’s Eye Telescope. The idea was that catching these returning laser pulses from the moon just requires a big light. It doesn’t need a lot of energy and capacity. He had this idea….you could buy 6” refractor objective lenses pretty cheaply at $200 apiece. Put 100 of those (6" lenses) together, you have the equivalent of a 60" telescope and you have now paid $20,000 for the optics. Whereas, the objective mirror blank, alone, for 16" telescope would have cost more money than that. He was in the process of building a telescope that had 100 6" telescopes, all on one mount, all sending their light to the same full multiplier tube. Use that for laser ranging. The telescope, ultimately, got built and operated in Hawaii. It took longer than my association with him lasted. Never saw the end of that project.

Pazar: Did you work on that project for Jim Faller?

Brown: I ended up helping with what was the computer control for this telescope. This was the beginning of an era where telescopes moved from being equatorially mounted and very heavy structures, to elevation azimuth mounts, which were computer controlled. Availability of computer control t drive in two directions at once. He had a very smart, young computer guy who came to CT and built the computer hardware and software for the telescope drive. Basically, I learned all the electronics I ever learned from this fellow, and also, much of the software engineering skills. I helped him to do the interfacing and setting up the motors….basically all the computer stuff.

Pazar: Where did you go to graduate school?

Brown: My undergraduate degree is in physics. I was a graduate student at the University of Colorado but I did most of my work in Tucson, working for a professor from the University of Arizona. At the time, I was getting paid by HAO. I was an HAO graduate student.

Pazar: Why did you pick that graduate program?

Brown: It was a complicated and incestuous deal. A fellow named Henry Hill had been the Chairman of the Physics Department at Wesleyan, when I was there. He had a joint appointment with the University of Arizona. He was doing experimental relativity with a telescope up in the mountains, north of Tucson. About the time I graduated from Wesleyan, there was a big fight happening in the Physics Department. Both, my advisor, Jim Faller, and Henry Hill left. Henry Hill went back to the University of Arizona, where he had a half-time appointment and took a full time job. Faller came to work at JILA on the University of Colorado campus (where he remains to this day).

Rabson: What years was this happening?

Brown: I graduated from Wesleyan in 1972. I showed up here (Colorado) in August of 1972. I spent a couple of years taking courses and started looking for a thesis project. I didn't find anything around CU that really excited me. I had, however, gotten interested in radio transfer and stellar atmospheres (by way of a course taught by Dmitri Mojales). This got me back into astrophysics, as opposed to physics. I, also, took courses in stellar pulsations from John Cox, at JILA, and had gotten excited about stellar oscillations.

Henry Hill's telescope in Arizona was intended to measure starlight deflection….gravitational starlight deflection by the sun. The idea was to track a faint star, as the sun moved passed it (as the Earth orbits the sun), measuring the position of the star relative to the sun. With great accuracy, you could measure the deflection of starlight without an eclipse. Previously, the only way was a solar eclipse and take pictures of the star field with the sun. You would do it (take pictures) six months before or after and get it with the sun absent, measuring position differences. Now, you could measure the gravitational starlight deflection, thereby testing general relativity (which is what Henry Hill wanted to do). Basically, meant measuring, very carefully, distances in the sky and the angular separation. His (Henry Hill) idea for calibrating this measurement was to measure the diameter of the sun. The diameter of the sun, was thought, to be absolutely constant. That was going to be his yardstick in the sky, the diameter of the sun. The first chore was to do that (measure the diameter of the sun). He, also, had hatched a secondary spin-off to that idea, which was to measure the diameter of the sun in different wavelength's of light using transmission filters that would have a narrow pass band. This caught the attention of Grant Ashay at HAO, who was my nominal thesis advisor. (If you look on my degree it is his signature, first.) The original idea, was I was to go to Arizona and use this multi-wavelength filter (which Henry was going to have finished in a year) to measure the diameter of the sun in the light of different spectrum lines in the solar spectra. By that means, measure the height of formation of different spectrum lines. The diameter of the sun would, therefore, be different at different wavelengths. Test of various regular transfer theories (that Grant was working on) and how radiation gets out of the sun into the solar atmosphere int space. This idea never happened. The filter ran into technical problems.

In the meantime, Henry Hill had another graduate student, who did a short (2 hour and 40 minutes) time series of the diameter of the sun. So, Henry, did have this instrument that was measuring the solar diameter. What the graduate student found that there were changes all the time in the apparent solar diameter. These changes were fairly large and seemed to have a time scale of forty minutes to an hour. I was fresh from my class in stellar oscillations (with John Cox) and mentioned that an hour is the fundamental oscillation period of the sun. Forty minutes could, easily, be the first term. With that…..we were off and running …..trying to measure solar oscillations and the diameter of the sun. That was, in fact, the topic of my PhD thesis.

Pazar: What was the title of your thesis?

Brown: "Observational Investigation of the Pulsations of the Apparent Solar Diameter."

At this time in 1975, it was known that there were pulsations on the sun of periods of five minutes. They had been know to exist since 1960. But no one knew what they were. There were three or four theories about what caused them and how they should be interpreted. As it turned out, one of those theories was right. But nobody knew which one. It seemed, at least possible, if you could see those oscillations (which you didn't understand) that maybe, longer period oscillations, which would have a more natural explanation and be a breathing mode of the whole sun, could be seen as well.

I spent two years, mostly on the mountain, working on the hardware of Henry's telescope which was complicated because it was computer driven. I was working on my PhD. The upshot there was a data set that consisted of measuring the diameter of the sun along one axis or another (north-south, east-west) for a dozen days with 7 - 10 hours of observation per day. We thought we were succeeding in detecting long-period pulsations at forty minutes to an hour. That was the bulk of my PhD thesis. There was, also, a piece that had to do with trying to explain what we were seeing in terms of stratification of the solar atmosphere. This was essentially theory.

As it turned out, the interpretation was all wrong. What we were really looking at was granulation and super-granulation on the sun. Changes in the temperature structure at the solar surface caused by convection processes rather than oscillation processes. They (observations) fooled the technique we were using to decide where the edge of the solar disc was.

Pazar: What was the technique?

Brown: "FFTD….Finite Fourier Transform Definition of the Solar Limb." I learned a lot….how to measure the solar limb….the defects of that method and it's virtues as well. The upshot of all that work was that after two years I had to recant (my thesis) and publish a paper saying all that stuff was wrong. Henry Hill never did accept that. I haven't talked to him in ten years. But the last time I talked to him, he was still convinced that the sun pulsated the way we said it did, originally. He was concocting more and more elaborate, theoretical underpinnings to explain what we saw (that no one else had been able to confirm). It was an unfortunate turn of events.

Rabson: When did you finish your PhD?

Brown: I handed in my PhD just before Christmas in 1976. The date on the degree is May 1977. In January of 1977, I took a job at Sacramento Peak Observatory in New Mexico. My one and only post-doctorate. Sacramento Peak Observatory had terrific equipment for doing measurements similar to that of Henry's telescope. Not so much the accurate astrometry, the measurement of distances in the sky, but careful photometry, to measure brightness.

Pazar: Where was Henry's Telescope?

Brown: Mount Lemmon, North of Tucson. An hour and twenty minute drive from the University of Arizona campus. Half of which was going east and getting out of the city of Tucson proper, half of it getting up to the mountain. It was at 9000 – 9200'. My understanding, is that, it is not there anymore. They moved it and the site returned to it's original state because it was on Forest Service land. It (telescope) was an amazing construction…..for a primary investigator level, NSF funded project. Henry had been running that project for 12-13 years by the time I left. NSF had put $600,000 into it. It was a technical marvel. I don't know where the pieces are.

Pazar: You had your post-doc at Sacramento Peak Observatory?

Brown: First thing, I sat down and made a better observation of brightness distribution close to the solar lens to try to convince myself whether I was or wasn't looking at solar oscillations (which was in my thesis). I decided it wasn't (solar oscillations). I don't really believe the thesis results.

I didn't do a lot of instrumentation my first year that I was there. I did a little work on high resolution imaging techniques. Early days of adaptive optics and ground-based interferometry. I did a little bit of work with non-redundant aperture arrays. Trying to improve imaging. Mostly that involved cutting holes in big pieces of plastic, which you could put over a telescope. Take images through it and do something on the computer.

I lived for 2 years on the mountain in Tucson. I was living in a single-wide trailer with two bedrooms. When I got there , it didn't have running water. By the time I left, it had cold running water. Forest Service outhouse was the sum total of sewage facilities. If you wanted drinking water, you drove down to the Ranger Station with a ten gallon jug and filled it up. Dumped in clorax, so I wouldn't poison myself. I didn't have the record for the longest time staying on the mountain without coming down. An old buddy and HAO graduate student, Tuck Stebbins, holds that record. He was up there for six weeks. I made it for a little over a month once. I had a great time. It was exactly where I wanted to be. Deer in the woods and a great rasberry patch nearby. And, so, I went to Sacramento Peak Observatory, which is, also, isolated. It is 17 miles to the nearest gas station. 40 miles to the nearest grocery store. Basically, up on the mountain in the middle of the woods with eighty other people. All of whom you can associate with a name and marital status and you know how their marriage is going……and their social security number…and how much money they make….names of their pets. I did fine on Mount Lemmon with no social interaction….so I would be fine on Sacramento Peak. What I didn't realize is that that sort of thing is cumulative. After a year, I suddenly realized I had reached my limit. At two years, it was fine…..at three years that was the end. I started looking for a job somewhere else.

I thought that if I am job hunting, it might be good if I had some idea of what I was going to do. Have something to sell myself with. So I hatched the idea of the Fourier Tachometer. There was, at HAO, a brand new solar interior and variability section that Peter Gilman was running. One of the big questions is…….is there or aren't there giant cells? (Big convective structures that have time scales of a month and spatial scales that are like the radius of the sun.) And how would I measure those? I needed a way to measure velocities, very precisely, on the surface of the sun. It seemed to me, the right way to do that was with an interferometer….interferometric filter. I flushed out that idea a little bit…..went through some numbers and decided that it would probably work. Wrote it up in a three page handwritten memo and gave it to Jacque Beckers, who was the Assistant Director at Sacramento Peak Observatory. We had an interesting back and forth. He took it home over the weekend and made a set of notes about it. Essentially he said that 'this idea won't work….it is no good'. I looked at his notes and said 'Jacque, this number is not right….you should of integrated from here to there'. I marked up his notes, 'will too work', and sent it back to him. He came back and said 'Okay. I give myself a D minus on my original notes'. We agreed that there was something in the idea and decided we would like to give it a try.

Over the following weekend, I put together the Fourier Tachometer Mark 0. One pixel instrument and a fourier multiplier for it's output. A little Michelson interferometer, that hailed back to my senior thesis at Wesleyan where I had built a related interferometer. I still had the pieces sitting around in a box and made into a Michelson with no trouble. So I did that….and needed a way to scan it. That was the only clever thing in it. I went into the Optics Lab at Sacramento Peak Observatory and found a puntatively flat window that had a wedge in it. By driving this wedge of this piece of glass perpendicular to the optical beam, I could add or subtract a little bit of glass from the optical path without having to control the position. It was a hand-driven micrometer to drive this thing back and forth. We found a filter, sitting in a drawer, that was suitable to generate some sort of a solar spiral. On Monday or Tuesday, we put it (interferometer) into the solar beam and were able to measure the wavelength of the laser line. Convinced ourselves that we were, actually, measuring something.

Pazar: Were you measuring something?

Brown: Yes. With that we decided …..okay. This is an entertaining toy but now we need to do something real. At about that time, I got the job at HAO in the early summer of 1978. Showed up here in late August of 1978. I spent one year and nine months at Sacramento Peak Observatory. And so, we started on the Fourier Tachometer Mark I which would be able to measure something useful. Jacque did most of the engineering on that. I contributed the image detector and processing system. The characteristics of that instrument were that it had a Piesall (sp?) electronic ally driven Michelson interferometer. Actually, moved one of the mirrors through a fixed wavelength of light with these Piesall electric crystals (which you could buy off the shelf). There was a linear detector array, one dimensional detector with 512 pixels. We scanned the sun across this detector array. The operation of the instrument involved two motions: one, driving the motor that would scan the image of the sun onto the detector; and the other was the voltage ramp that would drive the Piesall electric crystal to send the mirror back and forth. We were able to make images of velocity of the whole sun. We were able to measure the solar rotation. It wasn't completely satisfactory. It had some serious technical problems. The biggest problem was the Piesall electric drive that drove with a sawtooth shaped waveform. It would ramp up and then fly back really fast. When it flew back, it would excite mechanical resonance. The waveform you wanted to look like this.

But the motion of the instrument would comedown and bounce.

This went into operation in the fall after I moved to NCAR. We were collaborating on that (Fourier Tachometer Mark I) after I had come here (NCAR). Sometime that winter, we decided there was no future in that instrument (Fourier Tachometer Mark I) and had to build a better one. About the same time, Jacque had left Sacramento Peak Observatory and went to become Director of the Multiple Mirror Telescope in Tucson. I started collaborating with Jack Evans at Sacramento Peak Observatory. Jack was 75 years old. He had retired as the Director of Sacramento Peak Observatory, the week before I arrived there (SPO). He was now on Emeritus status. He was a wonderful old guy with an amazing comprehensive knowledge of optics. One example of that, the interferometer that was in the guts of the Fourier Tachometer was a polarizing Michelson interferometer. (This was an idea I thought I had hatched myself…..during the time when we realized the Fourier Tachometer Mark I was not working very well. This idea would work better if it didn't involve moving anything through tiny distances with great accuracy.) And so, I went to talk it (idea) with Jack. He said 'oh, yeah' and went over to his filing cabinet and pulled out a paper he had wrote in 1949 describing the same interferometer (the aforementioned idea). So I thought it was a really cool idea….and it was, but he had invented it a year before I was born.

With a new interferometer and a new detector, we launched into building the Fourier Tachometer Mark II. The agreement to do that with Sacramento Peak Observatory dated in 1980. Finally, made the thing (Fourier Tachometer Mark II) work in the summer of 1984. I remember we had a sign on the instrument that had five rings saying 'this is the Official Fourier Tachometer of the 1984 Olympics'.

Pazar: Are there any other instruments you invented?

Brown: So this has leaped frogged one whole instrument which is the Solar Diameter Monitor. But lets finish up with the Fourier Tachometer, first. It was clear that the Fourier Tachometer was going to take along time and so not to be stuck twiddling my thumbs, I did another instrument, which was the Solar Diameter Monitor. But the Fourier Tachometer went into operation in 1984. What we had discovered was that the Fourier Tachometer wasn't up to the original chore that we had set for it, which was to search for Giant Cells. Because it's large scale calibration was problematic. But on small spatial scales and small temporal scales (distances a tenth of the diameter of the sun and time scales of ten minutes) it worked just fine. It just didn't work well on spatial scales of the diameter of the sun and time scales of a month. On the shorter scale, you could work at the five minute pulsations on the sun, which turned out to be global pulsations of the sun. That wasn't properly understood until the early 1980s. Finally, people appreciated that 'yes', these correct theoretical explanation is that these are sound waves roaming inside the sun and that they are, in fact, global. We learned things about the global structure of the sun. This seems to be another constant in my career. I will build an instrument to do one thing and turns out it doesn't work very well for that but it is great for something else.

That led to a lot of work on the rotation of the sun and the realization that the rate of the interior of the sun rotates as a solid body. The outer part (of the sun), the convection zone, rotates differentially. Turned into something of great importance.

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