UCAR/NCAR Oral History Project

Interview with William Mankin

Interviewers:
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August 11, 2004

Pazar: When did you write the "Physics of a Cup of Coffee"?

Mankin: I used it to start a seminar. There was a photograph I took of some coffee, which had had some cream added. You could see the convective motion. The cream enabled you to visualize that and then I went on to discuss the theory of convection. I worked on theory of convection in the summers of 1963 and 1964 when I worked for Jack Herring at the Goddard Institute for Space Studies in New York City. I was a graduate student. This ("physics of coffee") was a seminar I gave when I was in graduate school to people in our group. A fun sort of thing, wasn't real science. The theory of convection probably was real science. Cooling of the coffee cup showed the theory of convection; in the photograph you could see the convective cells.

Rabson: You had a connection with Jack Herring, who was an NCAR scientist?

Mankin: He was at the Goddard Institute for Space Studies, which was a branch of Goddard Space Flight Center. He did theoretical work. I graduated in the spring of 1962 from Rhodes College. I got a scholarship to go to the Summer Institute in Space Physics. Space physics was brand new. NASA offered the class to sixty undergraduates to interest them in space science. This is where I learned radiative transfer, which has been relevant to my work over the years. In the summer of 1962, I did fairly well. Next summer they invited me back to be a teaching assistant. I was a teaching assistant half time and a research assistant half time for one of their scientists. One scientist was Jack Herring. Young guy working on turbulence theory and convection. This is where I learned computer programming among the first people to have big computers. In the summer of 1964, I went back and worked with Jack Herring. I also went back to see the World's Fair in New York. I spent three summers in Manhattan, working at Goddard doing programming and "experimental mathematics." Didn't know how to solve the equations and ran computer programs to see what would happen. Trying to understand how convection develops—at what temperature gradient convection starts. Not the temperature where turbulence starts. All non-linear and was a dimensionless problem. My summer work did not reach any big conclusions. Jack Herring came to NCAR after I did.

Pazar: What made you decide to go to Johns Hopkins for your PhD?

Mankin: As an undergraduate at Southwestern (now called Rhodes College), the physics department was basically a one-man department. It was Jack Taylor. He had graduated from Johns Hopkins and worked for John Strong. If you did reasonably well in first year physics, he (Jack Taylor) started brainwashing you that there was nothing to do but go to graduate school, and no place to go but Johns Hopkins, and no one to work for but John Strong. This little school (Rhodes College) that graduated about three physics majors a year had more graduate students in the physics department at Johns Hopkins than any other undergraduate school including Johns Hopkins itself. Jack Taylor was a very affective recruiter. At one moment, we had seven students from Southwestern at Johns Hopkins. I really had no choice. Jack Taylor brainwashed me.

I knew in high school that I wanted to study physics. When I was in college, I majored in physics. In graduate school, I was in the physics department. For three years, I took classes and passed my PhD qualifying exams. I had to pick someone to work for and was debating between Gerhard Dieke, who was a classical spectrometist, or John Strong. In August of 1965, Dieke had gone to a conference in Scotland and ran up the stairs in this castle and collapsed of a heart attack at the top. By the time I got back there (Johns Hopkins), my choice had been reduced from two to one. So I worked for John Strong. As did Bob MacQueen, who was also from Southwestern.

Pazar: Did you start working on infrared after you began working with John Strong?

Mankin: No. I had been working on infrared. Jack Taylor was an infrared person. I had always been interested in optics and light. Taylor directed me into the infrared. John Strong had his own Laboratory of Astrophysics and Physical Meteorology. It was part of the physics department and partly independent. He Strong did infrared work both in astronomy and atmospheric science. He had a program of balloon astronomy and had done a number of balloon experiments on measuring water vapor in the Venus atmosphere. My first project, I worked on a balloon experiment to measure the far infrared radiation from Venus. Venus in the near infrared is fairly cool, whereas in the radio region it is very hot. Somewhere in between it goes from cold to hot. So where does it go from cold to hot? Why is it hot? Is it really hot or is it emitting non-thermal radio waves? The experiment was unsuccessful. Balloons are kind of a risky business." Apparently, while in flight the power supply did not turn on.

Subsequently, I worked with Robert MacQueen regarding MacQueen's thesis, which was two-fold. One part was the 1966 eclipse in Bolivia and the other was a balloon experiment. So while MacQueen was at the 1966 eclipse, I was back in Baltimore designing a guidance system for the balloon experiment. MacQueen was measuring the thermal emission from the corona—dust around the sun. Interplanetary dust spirals in.because of the Poynting-Robertson effect. And as it gets closer to the sun, it gets hotter. Eventually, at some point, it is so hot, it vaporizes. The hotter it is the more infrared it puts out, until it vaporizes and then it stops. So as you look outside-in, closer and closer to the sun….the radiation gets brighter and brighter, until it stops at 4.3 solar radii. This is the temperature the dust vaporizes. By knowing this temperature, you can ascertain what the dust is made out of. The dust is made of silicates. After MacQueen graduated and came out to Boulder, I began my own experiment to measure far infrared from the sun, how it varied from the center to the limb. As you look at an angle, you see higher in the atmosphere. As you go to longer wavelengths, you see higher in the atmosphere. By measuring at different wavelengths and moving the center out to the edge. I was able to determine how the temperature in the solar atmosphere varies with height. Not a lot different from what we do with satellites. This work was not published.

Pazar**:** What was "Far Infrared Filters for Solar Observation"? (Mankin, William. Infrared Physics, 1973, Vol. 13, pp. 333-336)

Mankin**:** That was a piece of equipment that I invented for that experiment while at University of Massachusetts. John Strong left Johns Hopkins University because he would have to retire at age 65, whereas the retirement age at the University of Massachusetts was 70. So I went along. Spent a year and a half there at the University of Massachusetts. That is where I did my thesis experiment, which was the balloon experiment. "

Rabson: Were you using the National Scientific Balloon Facility?

Mankin: Yes, in Palestine, Texas. First Venus experiment was done at Holloman Air Force Base. Bob MacQueen's and my thesis experiments were done at Palestine; the flight log for mine is from March 23, 1968.

Pazar: Explain the thinking behind this new far infrared filter?

Mankin: In the far infrared, the big problem is that light is not very intense. And you have all this visible light. The sun at 6000 degrees puts out a huge amount of visible and near infrared light, not very much far infrared light. Need to separate it out and throw away the visible light with a very high degree of rejection. Strong suggested that I use Reststrahlen Filters, which is a technique developed at the start of the 20^{th} Century. Because crystals in the far infrared will get highly reflective at some particular wavelength. What happens is, say a quartz crystal, all the silicon atoms move in one direction and the oxygen atoms in the other and makes it (crystal) very shiny, shinier than the best mirror. But at shorter wavelengths, where there is no resonance, that reflects fewer percent. So if you reflect light on multiple crystals like this, than every reflection attenuates what you are trying to see, hardly at all. What you don't want to see is attenuated by a factor of 25 or so. So Strong had built this apparatus, back in the 1940s, that used these crystals to filter out the far infrared. Turned out that his device was awfully difficult to use. I invented this little thing to hold the crystals….four crystals that reflect light here [pointing at schematic]…here….here…and here and comes back out. So it means you can stick it in the beam and light comes back out, the same way it was going in. This is a tremendous advantage. Second, four reflections results in a factor of 25 to the fourth rejection, plus the first surface is polarized. And, because the plane of this crystal is different than the plane of that crystal [pointing at schematic] it has very low reflectivity. By crossing, you enhance the rejection by another factor of 10 or so. Ended up with a little metal device. Still have the prototype. The same technique as Strong's device in a much more convenient way. And particularly if you wanted to change the filters, mount these little things on a wheel [pointing at schematic] and rotate the wheel. Whereas Strong's device was big and impossible to move around.

Pazar: So what was Strong's reaction to your modification of his far infrared filter device?

Mankin: Not as impressed as I thought he would be. Used that (modified device) in the far infrared channels: two channels. The channels were 18-50 microns.

Pazar: Did you bring this modified Reststrahlen Filter Device to HAO?

Mankin**:** No, the actual instrument stayed at University of Massachusetts. Probably thrown away by now.

Pazar**:** After you worked with Strong, you decided to come to HAO?

Mankin: Yes, I went to HAO, to work with Bob MacQueen and Jack Eddy . John Firor and Jack Eddy had built a Fourier Transform Spectrometer and had flown it on the Convair 990 in 1968, to measure the far infrared from the sun. Very similar to what I was doing. And so, Bob MacQueen and Jack Eddy were going to continue that work because John Firor had become Director of NCAR. I had come in as a visitor to HAO to work on the Fourier Transform Spectrometer. But, the first project I worked on was the 1970 eclipse down in Mexico, where Bob MacQueen was trying to repeat the experiment he had done in 1966 in Bolivia, except with a spectrometer rather than filters, to try to get the spectrum of emission of the dust. Because silicates, which we thought dust was, have bands in the 9 micron region. And so, if we get a spectrum from the 8-12 micron in the window region. We could use chemical analysis to determine what kind of dust it was. The experiment on March 7, 1970 at best was inconclusive.

Subsequently, I used the Fourier Transform Spectrometer for the far infrared. Started building that to fly on the Sabreliner, [a plane] that NCAR had gotten a year or two before that. You can't just do the experiment on the ground because the water vapor in the far infrared absorbs so much light that you can't even see the sun. So you have to get up to high altitude. (The original purpose of flying my first experiment on the balloon was to get up above the water vapor). With the airplane, you could get up above most of the water vapor. By using the Fourier Transform Spectrometer, we could isolate the region between the absorption lines—see the sun in between the absorption lines.

Pazar: Who originally came up with this idea (for the spectrometer)?

Mankin: All of us: Bob Lee, Lee Lacey, Jack Eddy, Bob MacQueen and I.

Rabson: Where was the HAO machine shop in those days?

Mankin**:** In the HAO building, down on campus which is now the linguistics building. Machine shop was in the basement.

Pazar**:** Was the Fourier Transform Spectrometer instrument a Fabry-Perot or a Michelson?

Mankin**:** Michelson.

First took the Fourier Transform Spectrometer up to Pikes Peak in April of 1971. Pretty wintry on Pikes Peak in April. Stayed up there for a month, because at high altitude and low temperatures we were getting some signal from the sun. Mainly, our purpose was to test the instrumentand make sure it worked. The instrument flew on the Sabreliner in October of 1971 and 1972.

We (Bob Lee, Lee Lacey, Jack Eddy, Bob MacQueen and I) were talking about building a large ground-based interferometer with a very high resolution with a big telescope, called FIRLAT (Far Infrared Large Aperture Telescope). There was a lot of planning. The purpose was to see far infrared light that had been predicted to come from the solar corona. They were designing and conducting site surveys, art of reason for going to Pikes Peak. Because of HAO periodic budget cuts there was not enough money to build this FIRLAT telescope. Too expensive for an experiment. We weren't sure it would be productive. No one had ever seen these lines, no one was sure that the existed, and it turns out I don't think they do. Certainly not observable lines. So we were leaning towards…not worth (FIRLAT) the investment. We did not have enough money to continue and that was about the time of the infamous JEC [Joint Evaluation Committee]. The atmospheric parts of NCAR were being reorganized. I was looking for a job. Came over with the same equipment from HAO. Took the same data. Same airplane, same instruments. Try a different science. Instead of looking IN between the absorption lines to look at the sun, look AT the absorption lines to look at what was doing the absorbing. This is the rest of my career.

Rabson: It wasn't called the Chemistry Division in those days?

Mankin**:** It originally was Atmospheric Quality and Modification, because it included the stratospheric work, the quality being issues about the ozone, the modification being the National Hail Research Experimen (NHRE). Those two things were put together into one project called AQM (Atmospheric Quality and Modification). Then NHRE was no longer part of it, and what had become of "M" and "Q" had separated. The latter became the Atmospheric Quality Division. Then we weren't worried about atmospheric quality; we were worried about the chemistry. Then it became the Atmospheric Chemistry and Aeronomy Division. Still doing upper atmosphere stuff with Ray Roble and building satellites to look at the upper atmosphere and so forth. Eventually, Ray Roble moved to HAO and we stopped worrying about the mesosphere and it became the Atmospheric Chemistry Division. That is the evolution of it. All the same thing.

Rabson: The ozone issue in the early 1970s: did that have to do with the Supersonic Transport [SST] planes?

Mankin: Came out of Paul Crutzen's work. Well, actually, it went back to Sydney Chapman from HAO, who had in the 1930s developed a theory of the ozone layer. It was the first accurate theory. It explained why the chemistry worked the way it did, why there was an ozone layer. And the correct amounts. By the 1960s, the scientists had measured the various chemical reaction rates and discovered that Chapman's theory, when you put the right numbers in, predicted twice as much ozone as there was. In 1970, Paul Crutzen discovered if you take a pure oxygen atmosphere and add a little bit of nitrogen oxides (assumption from previous UV work on rockets was that there was some nitric oxide in the upper atmosphere), NO and NO_2, that they would catalytically destroy ozone. This is indeed what happened, depending on how much you put in the calculation, could reduce the amount of ozone by half. Paul Crutzen got the Nobel Prize for that discovery, that tiny amounts of other chemicals can catalytically destroy ozone and control amount rather than the amount of oxygen. Not long after that discovery they were talking about building Supersonic Transport planes. Harold Johnston at Berkeley said that Supersonic Transports use jet engines, jet engines have high temperature combustion…and that produces nitrogen oxides. Just like your automobile exhaust. If we would be flying around Supersonic Transports, wouldn't that destroy the ozone layer? People at the airplane manufacturers said, "Don't be silly." Other people were doing calculations and scratching their heads. No one knew quite how much nitrogen oxide was in the atmosphere already due to natural causes. And no one knew how much Supersonic Transports put out. So the DOT [Department of Transportation] started this big program called CIAP, Climatic Impact Assessment Program.

John Gille was talking about what we could do in the infrared. John Gille had the first satellite experiment, the LRIR, and introduced contributions of infrared to stratospheric research. That is when I wound up in ACD. John Gille was a reorganizer during the JEC [Joint Evaluation Committee]. He was one of the "Young Turks," along with Ray Roble, Bob Dickinson and Dieter Ehhalt . They were organizing an Upper Atmosphere Project and that's when I came into ACD and brought the equipment from HAO. HAO not only had no money to build new instruments but had no money to run the old instruments. HAO was quite content to let me take the old equipment with me. Moved into this office on the first of September of 1973. Been in the same office ever since; must be a record.

Pazar**:** What was the "Airborne Far Infrared Solar Spectroscopy" (Mankin, Wiliam G., J.A. Eddy, R.H. Lee, and R.M. MacQueen. Instrumentation in Astronomy II. Proceedings of the Society of Photo-Optical Instrumentation Engineers. Vol. 44. March 1974. Pp. 133-136.) paper published during this time?

Mankin: That paper is a result of the solar work at HAO and came out in 1974. The meeting was earlier than that. We were making measurements of brightness temperature as a function of wavelength. Using HAO science and spectroscopy to identify various lines and tell how much water and ozone. Turns out, in the far infrared the ozone has a line almost everywhere. Anytime, you see a line it is most likely ozone. You can't see the things that are at a much lower concentration. You can measure ozone without all this trouble. After a couple of years, we decided to move to a much shorter wavelength. In 1974, we bought a short wavelength mid-infrared instrument built by the EOCOM Company. So we called it the EOCOM. We gave up far infrared, long wavelength infrared used rotation lines, and we moved into the vibration rotation bands at shorter wavelengths. Instead of working from 300-1000 microns, we worked on 2-15 microns, shortening our wavelength a lot.

Pazar**:** Did you modify the Fourier Far Infrared instrument?

Mankin: No, but cannibalized it. Didn't throw it away ,kept it for 15 years. Took parts of it. First time we flew the short wavelength instrument; it was mounted in the airplane atop the table from the far infrared instrument. Took the far infrared parts out , put the near infrared parts in and flew it. Every time we flew, we would re-modify. Eventually, replaced the table (big base) of the far infrared with a new one. In 1977, a new table we built in the shop, which is the table we are using now. In the 1980s, we scrapped all of the far infrared instrument and recycled it. Recycled the aluminum custom castings. Kept a few of the big optical elements. Kept black bodies, which were used as reference. Kept the tracker, which was mounted on the top and did all the pointing and let the sunlight in. Also kept torque motors.

Pazar**:** You only work with short wavelength infrared, now?

Mankin: Yes, haven't worked with far infrared since the mid-1970s. Some people have done good stuff with far infrared using balloons. The problem with airplanes is that there is a window. You can't have a window that is required by the airplane and transmit the infrared at wavelengths of 100 microns. Far infrared wavelengths longer than 300 microns show an ozone line everywhere. In Italy, Bruno Carli has done wonderful work with high resolution spectrometers on balloons. A group at Harvard-Smithsonian Center for Astrophysics has done balloon work. Same instrument for a balloon as that of the DC-8, without a window. The instrument is effectively outside of the plane. They built a big box around the instrument with no window. Therefore, it is at the pressure of the outside air to eliminate the window. We didn't have engineering to build something to put the instrument outside of airplane. Anyway, there are some things you can see in the far infrared that you can't see in the near infrared. But there are a lot more in the near infrared.

Pazar**:** What are you working on now?

Mankin**:** HIRDLS (High Resolution Dynamics Limb Sounder). Also, building a new near infrared interferometer, which we started along time ago. We had the first interferogram from this instrument last spring (2004). It is much bigger than the EOCOM with sixteen times the path length sixteen times better resolution. It matches the instrument in resolution in Thule, Greenland. The Thule instrument is a commercial instrument. The new instrument is to operate on an airplane. It is being built here and will supercede the one (EOCOM) that is flying presently.

We have three interferometers. We have the near infrared interferometer, EOCOM, that has a maximum path difference of 16 cm. (Path difference determines spectral resolution). The Bruker interferometer has a path difference of 256 cm, higher resolution. It not only measures the area of the line but the shape of the line. The shape of the line can determine not only how much stuff but also at what pressure. Therefore, you can tell where it is in the atmosphere and get a profile. The Bruker is [part of the] NASA Network for Detection of Stratospheric Change. It is a ground-based network with stations around the world, monitoring the ozone layer long term. The third interferometer is being built and will match Bruker in resolution but not much physically bigger than the EOCOM. Possibly will take the place of the EOCOM or end up having two instruments. One can be used on the ground in another site other than the Bruker. The EOCOM is not planned to be cannibalized.

Pazar: What does the Bruker instrument at Thule measure?

Mankin: It measures anything you can see. You can't see as much from the ground as from an airplane because of the water vapor. In particular the NO_2, which is one of the things we excelled at because of the airplane. Can't see the main band, from the ground, because of the water vapor. It is totally absorbed at that wavelength. We have to use a much weaker band.

Rabson: Where is the third instrument (new interferometer) being built?

Mankin: The NCAR machine shop and being set up in the lab. We are taking parts and making an instrument. Some of the important parts were commercially bought. There are three crucial things that were purchased: one is the beam splitter. It was very expensive. The other two things are retro-reflectors, which reflect the light back to the beam splitter and they were made a long time ago. Also, these were very expensive. It was a long time before we used them.

Pazar: Have you decided on a plane for this new interferometer?

Mankin: HIAPER [High Performance Instrumented Airborne Platform for Environmental Research], which is made by Lockheed. Possibly the NASA DC-8. We tried to design the interferometer to go on an airplane like the ER-2 where you don't have an operator to go along with it. It is designed it to fit in the pod of the ER-2.

Rabson: Is the plan for the HIAPER aircraft, that there won't be any scientists, technicians or engineers on board?

Mankin: I don't think so. I don't know. Can't imagine having someone not flying along to make sure the instruments worked correctly. Some instruments are automatic. Our instruments…we could make run automatically. It makes more sense to have someone go along to observe and to fix something if it stops working,.i.e. if the sun tracker loses track. Yes, you could automate it but as long as you have an airplane with a shirt sleeve environment, there is no good reason to make it automatic. Despite the progress of computers, people are a lot smarter. Not near as fast, but a lot smarter.

Pazar: Any failures of the Fourier Transform Spectrometer?

Mankin: No. It worked pretty well in the far infrared. It was, largely designed by Lee Lacey. A wonderful designer. Bob Lee did all the electronics for it.

Pazar: How many programs did Fourier Transform Spectrometer perform?

Mankin: It was used three times. At Pikes Peak for one month, then October, 1971, and 1972 on the Sabreliner. Each observing period was one month with approximately ten flights apiece.

Rabson: There is a photograph in the HAO collection of Bob Lee on top of Pikes Peak

Mankin: We had a flag. We also have graph paper from the X-Y plotter. It was a calendar. What we observed, which days were cloudy, which were clear. We also had set records. The most memorable record was the "coldest temperature ever recorded on Pikes Peak." It was 2 degrees above absolute zero at the bottom of our liquid helium tank! Also, we had the "world's highest flying Ronald MacDonald kite" at the summit of Pikes Peak. It actually was at a lower altitude than the summit because it was flying down below the summit. Some of Jack Eddy's humor.

END OF INTERVIEW