Oral Histories in Meteoritics and Planetary Science – XX:

Dale P. Cruikshank

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SUPPLEMENTAL INFORMATION

Recollections of Gerard Kuiper

DS: Okay, so we have recorded your oral history, let's spend some time recollecting your interactions with Gerard Kuiper. You described how you met him at Yerkes Observatory as an undergraduate and how you went to graduate school after he moved to the University of Arizona

DC: Alan Binder and I applied to graduate school in Arizona and did our graduate training there. At about the same time, Bill Hartmann came to Tucson as a graduate student from Penn State, Elliott Moore and Tobias Owen also came as students from the University of Chicago. Several astronomers and atmospheric physicists also came to Tucson in those first years; Joseph Chamberlain, Donald Hunten, and Lloyd Wallace came to Arizona and joined the staff at the Kitt Peak National Observatory. The National Observatory had decided to locate in Tucson and build its observatory at Kitt Peak some 40 miles outside of town. By the time Kuiper got there Kitt Peak National Observatory was becoming quite a significant national establishment, and that provided a strong draw to the area. The Kitt Peak offices were located on the edge of the University of Arizona campus, just across the street from the University's Steward Observatory.

When Kuiper left Yerkes several people involved in his lunar studies enterprise went with him, including David Arthur and Ewen Whitaker. Eventually Tom Gehrels, formerly associated with Kuiper at the University of Chicago, joined the group in Tucson. Tom was not a Moon person as such, but he was engaged in polarization measurements of many Solar System objects, as well as stars. Georges van Biesbroeck, who was a double star specialist who also worked on comets, asteroids and other Solar System bodies, moved to Tucson at that time, perhaps in large part to take his is sister and his aging wife to a more clement environment than the Wisconsin countryside. Eliott Moore, mentioned before, was a graduate student at Yerkes who did a lot of the lunar photography with the Yerkes 40-inch telescope for inclusion in Kuiper's lunar atlas supported by the U.S. Air Force; he came to Tucson and finished his graduate education at the University of Arizona.

DS: The University was also rapidly growing at that time.

DC: Yes, and there was vision, which is very important. Just as a quick aside, one of the reasons the university embraced Kuiper to come there as a new faculty member in late 1960 was that he

promised to set up a curriculum in planetary sciences, which did not exist anywhere else in the world, and most people had never even thought of it. So he had promised to do that, but I have to say that, while that was a promise, by the time I left it had not happened.

DS: You mean a graduate program?

DC: A graduate program, but even without a formal curriculum in planetary science, the research went on great. People who were graduate students working in the Lunar and Planetary Lab in those first ten years or so had their PhD programs either in geosciences, astronomy, or atmospheric physics, all three of which were well established programs at the University of Arizona. So that worked out okay and that fact alone exemplified the vision Kuiper had about what a planetary science program would encompass, and that planetary science would draw upon those three disciplines and chemistry as well, providing people came to it with a background in physics. It all made sense, it's just that a formal program in planetary science had not been established by the time I had finished and gone.

DS: I can imagine that Kuiper would not have been attracted to that because it is a huge administrative task.

DC: Yes, it was a big logistics issue. It would have been tough to pull it off even with the full support of the deans and chancellor, and it to some degree required the acquisition of new faculty members with backgrounds and skill not represented by the original staff.

Gerard Kuiper

DS: Talk to me now about Gerard Kuiper. You have mentioned already that you encountered him first as an undergraduate and that you found yourself working with him in the darkroom. What were your impressions of him at that time? You were in awe of him but he had time to work with a summer student?

DC: Yes, I was in awe, and he did indeed have time to work with a summer student. By the summer of 1958, he was engaged in getting the best available pictures of the Moon assembled into a comprehensive collection for the publication of an atlas of the Moon commissioned by the U.S. Air Force. I first encountered him at the observatory, told him my name (I knew his name, of course), as we stood in front of the bulletin board in the hallway with one of the other observatory employees. Kuiper came up, held out his hand, and said, "I am Mr. Kuiper, the Director of the Observatory", and he greeted me and welcomed me to the summer program. After that he forgot my name, but that's okay I would have forgotten it myself. That was the actual first physical contact and, as I say, I knew his name very well from books and such. Then the next encounter was in the dark room where we were making prints for the atlas. Then, over the summer there were various opportunities to show him what was coming out of the darkroom, as well as some social gatherings at his house to which the whole observatory was invited. Although big name scientists can be a bit quirky, they were very nice to me, and the whole experience was great. I was getting used to meeting people. Although Kuiper would drop in from time to time, on a day-to-day basis, my supervisor was Joe Tapscott, the manager of the darkroom complex where the lunar photos were being processed and slides and glossy prints were made for commercial sale. I had a chance to help out with all that work, and especially enjoyed making glass lantern slides of astronomical photos for use in old style projectors. Those slides and projectors are now antiques!

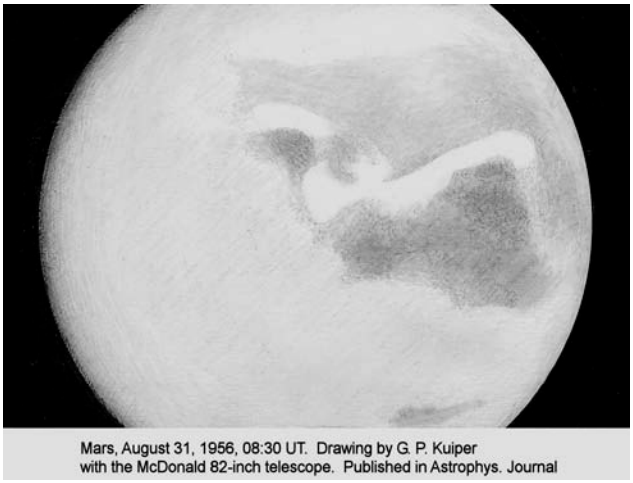
Gerard P. Kuiper Yerkes Observatory Photograph

Gerard P. Kuiper, 1949. Yerkes Observatory Photograph

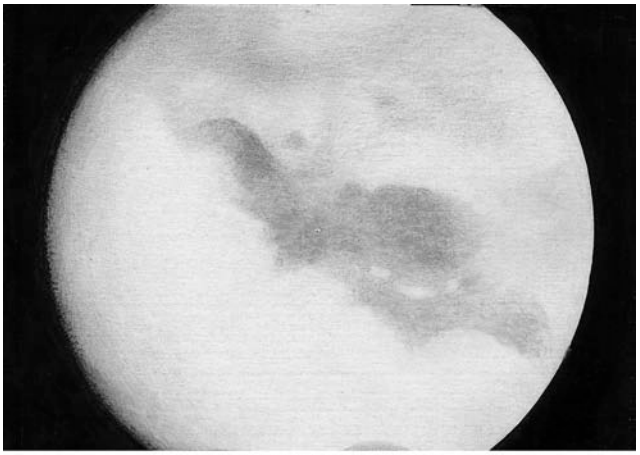
Photos 1 and 2, from the Yerkes Observatory files. Photo 2 is dated 1949.

DS: This is still when you were getting used to being around foreigners.

DC: Yes, that is right. The only thing familiar was the Wisconsin countryside, which is similar to the Iowa countryside. Standing in the rural Wisconsin countryside is this gothic gargoyle-decorated building that is Yerkes Observatory itself. That alone is very impressive, it is breathtaking. My contact with Kuiper was on and off during the summer. One other contact I remember gave me the first glimpse of the wilting remarks that he could make. As an amateur astronomer I had been interested in one particular crater on the Moon that was different from the others, it is Alpetragius, a crater that is a fairly decent size with a central peak, but the central peak is anomalously large and the whole surface of the crater is very smooth. This encounter occurred at some sort of coffee event in the library and I was talking with one of the other lunar people about my favorite crater. I said I didn't know how Alpetragius was formed but that it had this unusual appearance. Kuiper overheard this from a few feet away, came over, and politely but firmly inserted himself into the conversation and said that, "Yes, I have looked at that crater with the 82-inch telescope at McDonald. It is covered with splash remnants of a nearby impact". Then he turned around and walked away. Problem solved. No further discussion needed. Basically, "I know how this crater came to be the way it is, and goodbye". Thank you very much. [Laughter] I mean, he was polite, but dismissive of all that had gone before. This was characteristic of the way he was. He had answers to issues and questions; he told you what they are. End of story.



Mars, August 31, 1956, 08:30 UT. Drawing by G. P. Kuiper
with the McDonald 82-inch telescope. Published in Astrophys. Journal



Mars, 1956. Drawing by G. P. Kuiper
with the McDonald 82-inch telescope. Published in Astrophys. Journal

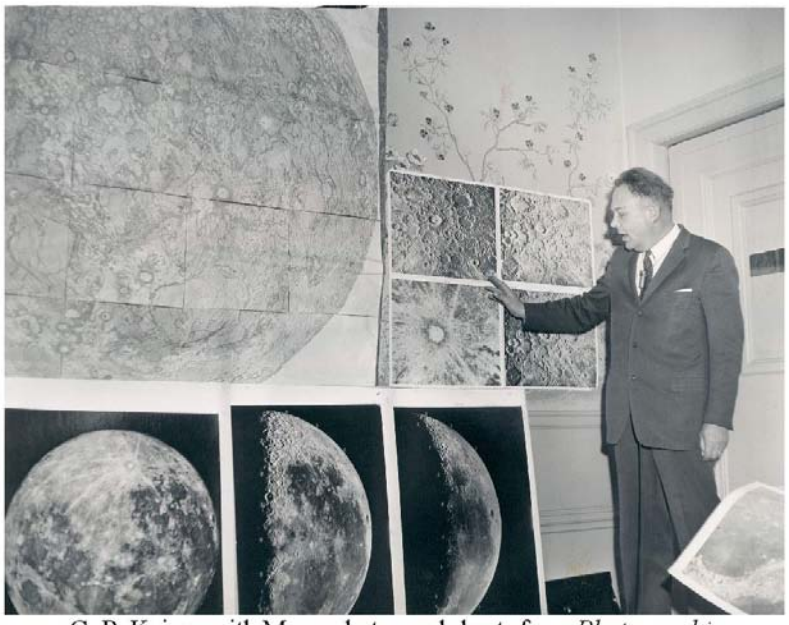
Photos 3 and 4. Drawings of Mars by Kuiper with the McDonald telescope in 1956, which was a very favorable perihelic apparition of Mars. The W-shaped cloud was the subject of a press release by Kuiper, and I saw a brief piece about it in the Des Moines newspaper.

IAU, 1958. F. J. M. Stratton, G. P. Kuiper, E. Hertsprung, H. Shapley (photo: Owen Gingerich)

Photo 5. This picture taken by Owen Gingerich at the 1958 General Assembly of the International Astronomical Union, shows Kuiper with his former professor, Einar Hertsprung.

Photo 6. Group photo at Yerkes Observatory, summer of 1959. Dale is in the upper-right of the picture. Yerkes Observatory photograph

Photos 7 and 8. These pictures from the summer of 1960 show Kuiper and the rock that was recovered from a nearby farmer's field, and initially thought to be a possible meteorite. It was not. Photo 8 shows Kuiper and his son Paul examining the rock. Photos by D. Cruikshank



G. P. Kuiper with Moon photos and sheets from *Photographic Atlas of the Moon* (1959). Photo source unknown.

Photo 9. Kuiper and Moon photos. Source unknown.

DS: So you graduated with your BS in late 1961, but after the summers at Yerkes Observatory (1958, 1959, and 1960) you had spent the summer of 1961 at the Lunar and Planetary Lab. Then in early 1962 you went to Arizona and applied for admission to the graduate program to work with him.

DC: Right, but in the end I went through the program in geosciences. Kuiper was not closely affiliated with the astronomy at that time, it seemed, but instead primarily a research professor, as I recall. It was clear all during the ten years I was in Tucson that his principal focus was on research, building the Lunar and Planetary Lab, and on establishing new observatory sites.

DS: I have got the paperwork for his appointment. He was appointed in three groups, his funding was coming from three groups, one was the observatory, one was the astronomy department, and I can't remember what the third was.

DC: He was physically located in the first Planetary and Lunar Laboratory in the Institute of Atmospheric Physics. That might have been the third entity. It was a very dynamic program, and everyone thought well of it. It also fit rather well with his interests. The astronomy department was led by Edwin Carpenter, who was an old-time astronomer who shared Kuiper's interest in white dwarf stars. The astronomy department was slowly transitioning to a more modern program, and had some fine professors, such as Walter Fitch and Ray Weymann. Some of the astronomers at Kitt Peak, such as Aden Meinel and Beverly Lynds also taught in the department. I think Kuiper and Carpenter had their differences and in a sense represented two different worlds in astronomy. I presume that Kuiper's appointment was at least in part in the astronomy department, but perhaps in atmospheric physics as well.

Photo 10. Kuiper and observing assistants Dale Cruikshank and Alan Binder, at the Kitt Peak National Observatory 36-inch telescope, June, 1962. Photo source unknown.

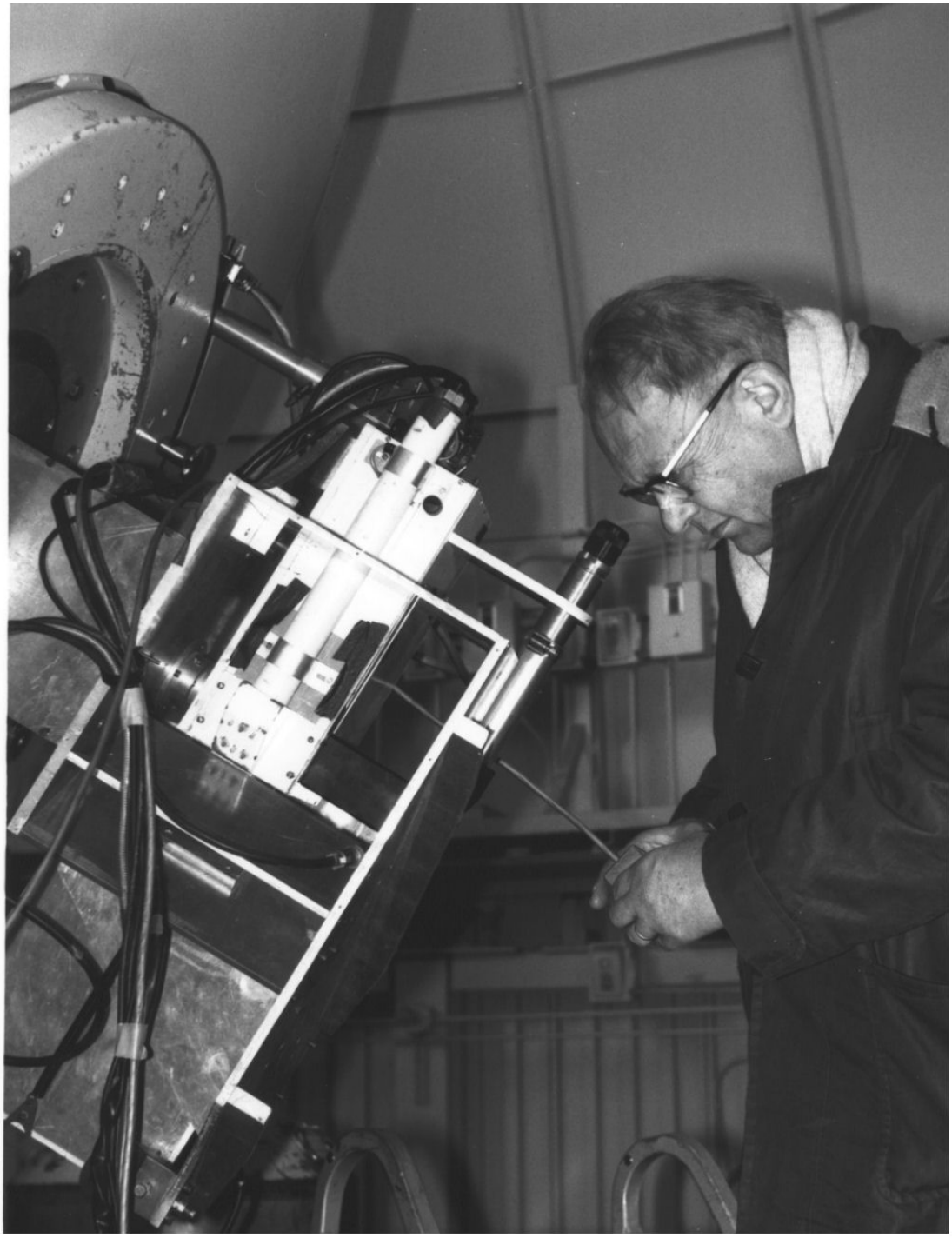
Photo 11. Kuiper and engineer Richard Goranson with the IR spectrometer on the Kitt Peak 36-inch telescope during daylight observations, 1962. Photo by D. Cruikshank

DS: So you knew from the outset that he would be your PhD advisor?

DC: That was the expectation and hope, yes.

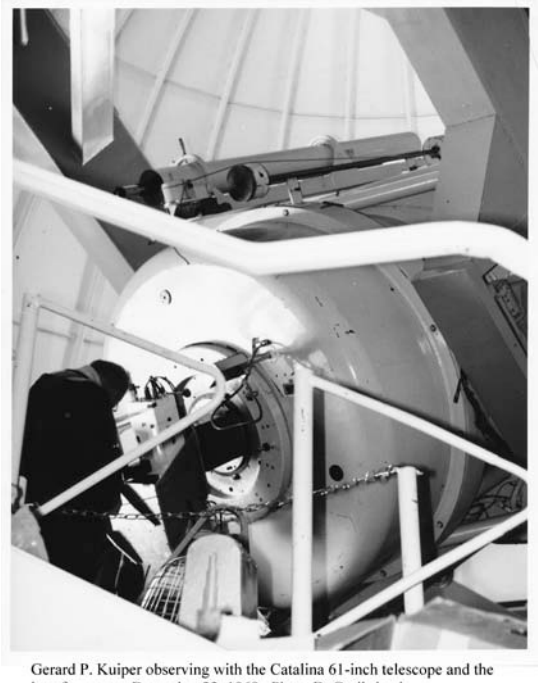
DS: But you ended up in geosciences and you are an astronomer.

DC: I came up through a hybrid orientation. I was always interested in the planets, and as planetary science was being defined it was clear that this field represented an intersection of traditional astronomy, geosciences, atmospheric science, and chemistry. Many people were pursuing the curriculum in astronomy and astrophysics and had no interest in planets at all. A few of us students, such as Bill Hartmann were interested both in astronomy and geology; Bill got a master's degree in geology and then did his PhD in astronomy. I thought that geosciences would work out better under my particular circumstances. Bill and I occasionally talked about the curious fact that while a person interested in the planets could also be very interested in traditional astronomy (stars, dust, galaxies), the reverse was rarely the case. Kuiper himself was perhaps the original archetype of such a scientist who had been trained in traditional astronomy and then developed a very serious interest in the planetary system, making fundamental contributions to both areas. As another point of interest, it appears that a considerable number of traditional astronomers who have eschewed the Solar System all their professional lives are currently developing a compelling interest in the planets, now over 2000 in number, found around stars other than the Sun!



Gerard P. Kuiper observing with the Catalina 61-inch telescope and the interferometer, December 22, 1969. Photo D. Cruikshank

Photo 12. Kuiper was a great observer, and completely at home with a telescope and an instrument. Dale took these three photos of him (while assisting with the work) at an observing run in December, 1969, using the 61-inch telescope built by Kuiper and a source of great pride to him.



Gerard P. Kuiper observing with the Catalina 61-inch telescope and the interferometer, December 22, 1969. Photo D. Cruikshank

Gerard P. Kuiper observing with the Catalina 61-inch telescope and the interferometer, December 22, 1969. Photo D. Cruikshank

Photos 13 and 14, by D. Cruikshank

DS: So, although you sometimes represent yourself as an astronomer, you are much closer to being a planetary scientist, with the breadth of interest that is implied by the term.

DC: Yes, when I'm among astronomers I can pretend to be a geologist, and when I'm with geologists, I can present myself as an astronomer. This dual posture allows me to get away with minimal knowledge in both fields, but my training and subsequent research enable me to make sensible comments on both topics from time to time.

My pathway to the geosciences program at the University of Arizona was both inspired and enabled by one particular faculty member in the geology department, SpencerTitley. He was widely known and respected as an ore geologist, but he had become interested in the Moon largely through contact with Eugene Shoemaker at the Geological Survey in Flagstaff. Shoemaker and other people in the Survey had become interested in the Moon in part because of the national effort toward safely landing people there. Shoemaker was interested in craters from the viewpoint of hypervelocity impacts of meteoroids, and also in view of explosion craters made on Earth by nuclear bombs. He and his colleagues were mapping regions (quadrangles, in geologic parlance) on the Moon using visual observations and photography, essentially defining stratigraphic units of differing surface texture, and other parameters, using crater density, albedo and geomorphic features. The purpose was in large part to establish a sequence of relative ages of lunar surface units defined by these parameters. Of course, we had to wait for the lunar samples to be returned by Apollo to get the absolute ages. So Titley got interested in that work and made maps of a couple of the lunar quadrangles. He had students who did mapping of a couple of lunar quadrangles, too; Alan Binder was one of them. I never did that, but I bring this up to point out that at least a few geoscientists were getting seriously interested in what was to emerge as planetary science, too. So it was essentially up to us as students to put together a plausible Ph.D. program that included geology and astronomy, and my dissertation was eventually directed jointly by Kuiper and Titley. Now that is

not to say that those two were on good terms. They were not, especially towards the end, but I did not get too much of the fallout from that. The situation worsened after I finished and left.

DS: Polite professors keep this away from their students.

DC: In this case they managed reasonably well. So that worked out okay, but technically since there was no planetary science department at that time my degree comes from the geosciences department.

Kuiper's move to Arizona

DS: Before we get too far away from the earliest years in Tucson, you might just share with me your thoughts on how Kuiper came to leave Chicago and move Arizona.

DC: He left the University of Chicago and the directorship of the observatory somewhat under duress, although the situation was not commonly spoken of in the hallway, and certainly not with the summer assistants and people of my class. So I don't know the details, although there are people who surely do. To my knowledge, it has never been written down or discussed widely. I think that Kuiper reached a tipping point with his professional colleagues and other people at the observatory resulting from the perception that he was being high-handed in appropriating resources at the observatory, primarily in the pursuit of the lunar mapping thing for which he had U.S. Air Force support. This annoyed the other faculty members, and also pointed out that he was moving further and further away from traditional astrophysics. But to a person, I am sure those people recognized the important contributions he had made to astronomy and astrophysics earlier in his career. Certainly Morgan recognized that, Chandrasekhar recognized that, and probably the other people did, but I didn't talk to them very much. But they recognized his shifting interest, his high-handedness, his abrasive character (although they all had their moments), and I guess I would characterize the situation as Kuiper being forced out of the directorship and being made to feel so uncomfortable that he sought more friendly ground elsewhere.

DS: Even with an ego we can say that he got very hurt that he was treated this way and just say, "I could do better elsewhere"?

DC: Yes, indeed. There is a curious element of this which is that he had actually talked to the administrators at a small college in Texas, Sul Ross College (now Sul Ross State University) in Alpine, Texas about setting up there. I don't know what persuaded him to not do that, maybe he got wind of the arrangements being made at the University of Arizona, the fact that the Kitt Peak observatory was there was an appealing draw I think, and that the desert southwest has the sites for potential new telescopes. I am sure also appealed to him, but another component of this was that his old school mate Bart Bok, a fellow Dutchman, who had emigrated to the US and established most of his career here, had negotiated with the University of Arizona to become the Director of the Steward Observatory and to head the astronomy program after Carpenter retired. However, Bok had also been given the opportunity to direct the Mount Stromlo Observatory in Australia, and for some reason he very much wanted to do so. He made an arrangement with the University of Arizona that he would accept that appointment two years away, maybe even three years, I don't remember now. So Bart Bok told me later, after Kuiper's death, that this agreement had been struck. Kuiper expressed an interest in going to Arizona, the President of the University got in

touch with Bart, asking, "Does this make sense, can you live with this, etc., etc. ?", and according to Bart Bok he had the deciding vote. He decided yes, bring Kuiper to the University of Arizona.

Now this was transparent to me and a lot of other people at the time; maybe some of the senior people knew about it, but I certainly didn't. What I did know is that two or three years later Carpenter passed away and not long after that Bart Bok showed up on the scene as director of the observatory. So two old classmates from the University of Leiden came to Arizona, friendly, sparring adversaries I would say, but scientifically they did not really tread on each other's toes because they had such different areas of interest. As a point of curiosity, there are other Dutchmen involved in this story in a broader way whose scientific work Kuiper did tread on, or vice versa, but it isn't really central to the Arizona story, just a curious aside. The names H. P. Berlage, D. ter Haar, and W. J. Luyten come to mind. Sometimes Kuiper was characteristically abrasive and hard-headed, but have been admonished by other Dutchmen not to characterize the entire race that way, so you didn't hear it from me.

DS: So that is what you have to say about his move to Arizona which I think was a critical event in his career.

DC: I am hopeful that you will find someone who knows the details or someone who has written all this down.

DS: The accounts I have seen discretely avoid the reasons for the move.

Okay, so when you came to Arizona he had only been here a year or two. It's the sort of arrangement universities are usually good at doing. So the Lunar and Planetary Lab was created as soon as he got there?

DC: Yes, yes.

The Lunar and Planetary Laboratory

DS: As an entity independent of the regular departments?

DC: It seems to have been. It was physically located in the Institute for Atmospheric Physics.

DS: Then it morphed at some point into the Department of Planetary Sciences.

DC: Right, but not before it moved. A couple of years, maybe three or four, after Kuiper got there it became clear the facilities at the Institute for Atmospheric Physics were too small, and he negotiated with NASA for a new building, now the Kuiper Building, named in his honor. Although it has subsequently been significantly enlarged, it began as a five-story building with an optical shop, with laboratories, photographic darkrooms, and a major gas absorption cell 40 meters long that terminated in the spectroscopy lab that was designed and built to do the spectroscopy of gases in planetary atmospheres.

Photos 15 and 16. Photo 15 shows graduate student Toby Owen with the small (62-ft) absorption cell in the original Lunar and Planetary Lab facility. He used data obtained with that tube for his dissertation on planetary spectroscopy (LPL Photo). Photo 16 shows the larger 40-m absorption cell under construction in the new Lunar and Planetary Laboratory (now the Kuiper Building). Photo by D. Cruikshank

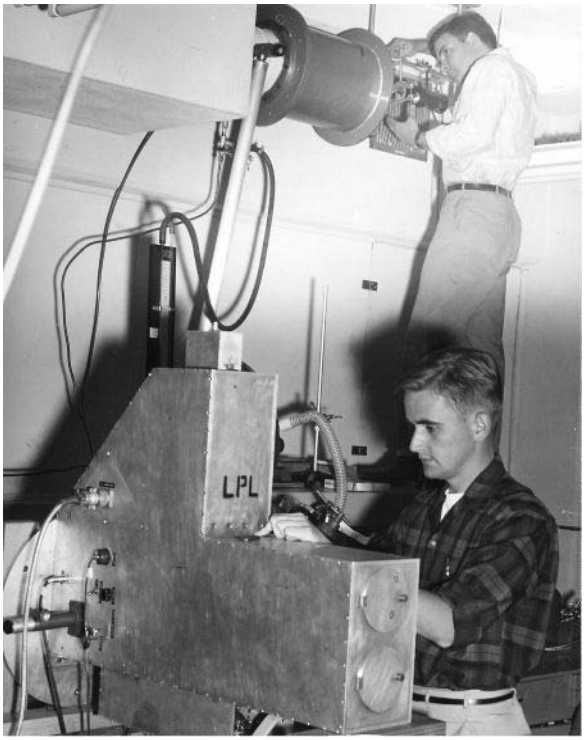


Photo 17. In this LPL photograph, Toby Owen is working on the absorption cell to send light into the infrared spectrometer below (operated by Dale).

DS: Is that absorption cell still there?

DC: No it has been removed, as a matter of fact. It was in use off and on for a few years, and then it was regarded as a hazard because some users were filling it up with three or four atmospheres of hydrogen and it would have blown out the whole side of the building if it had gone off. I used the big tube a few times for methane and ammonia studies, but there wasn't, at least in the beginning, a truly safe exhaust system. On a smaller scale, I managed (accidently) to spread some hydrogen sulfide through the building once, and that stuff really does find its way through the ventilation system.

DS: I have had the same experience cleaning meteorites with HCl and filling the building with H_2S.

DC: Anyway that building came about; it's been a Godsend to the program. When everything was settled in there the Lunar and Planetary Lab. embraced some big names. Frank Low and his operation were there, Tom Gehrels and his operation were there; he was flying a balloon with a 20-some inch telescope on it doing polarimetry in the ultraviolet from above much of the telluric ozone.

DS: These are people affiliated with the LPL under Kuiper's direction?

DC: Yes, that's right. Harold Johnson was also there, Aden Meinel was affiliated both with the Lunar and Planetary Lab and I think the Kitt Peak National Observatory, he then came to found and direct the Optical Sciences Center at the University of Arizona; the building that they occupy is named for him. So there were big names and big personalities, some of which interacted poorly. Others were there in their own universe and tried not to interact. One element of this though that still astonishes me is that Frank Low had his students, Tom Gehrels has his students, Kuiper had a few students, and we never talked to the other students. The students in these little enclaves were certainly not encouraged to, and in a few cases were told not to, mingle with the other students. It was a very unhealthy environment.

DS: It is not optimum.

DC: No it wasn't at all. It's different now, very different. I think it was because of the strong personalities of the leads of those groups and the tensions among those leaders, those top guys.

DS: So Kuiper brought all these people to Arizona and they couldn't get along?

DC: In a nutshell, yes. They interacted of course, but often only in a formal way, at least as seen through a student's eyes.

DS: They didn't drink beer.

DC: No, they did not. But this brings to mind another interesting facet of Kuiper's personality and behavior. He often needed someone to listen to him, a sounding board, you could say, as he worked through a difficult issue. It was almost immaterial just who that person could be, and I frequently found myself in his office listening as he described a situation he was dealing with. This most often occurred after hours and on week-ends--he would be in his office during those quiet times, and when he wanted to talk about an issue, he would walk over to the lab to see if anyone might be there, or down the hallway to the office that Binder and I shared, to find someone to talk to. Since I was commonly there in the evenings and week-ends, I was often summoned to his office while he

talked through a situation, sometimes scientific, but frequently related to personnel and the organization of the Lab. Sometimes it would be about his interactions with NASA Headquarters people, or scientists in other institutions. He would typically begin by saying, "This is confidential, but...". I would acknowledge the confidentiality, and he would start talking about things that I often had no business knowing about, or didn't understand, or both. Some topics were of direct interest to me, and I would comment or ask questions accordingly, but most of the time I was there simply to serve as a listener. Hartmann and Binder had these experiences, too. I'm sure they also heard some very interesting things. Looking back on these experiences, I can say that it was good to have Kuiper's attention in these curiously personal encounters, and especially good that he felt he could trust me (or whomever he was talking with) knowing about situations and events that were sensitive at various levels.

Kuiper as a mentor

DS: So you are in Kuiper's group, and you met with him early and often to set up your own research program?

DC: Well, it was somewhat less formal than your question implies. As a student-assistant in his spectroscopy lab I certainly met with him often, and frequently accompanied him to the observatories at Kitt Peak and in Texas. Preparation for the observing sessions were made in the lab, where the resources consisted of the absorption cells, the darkroom complex, and the spectrometers. In the course of doing those preparations every day of the week and doing other things of interest to Kuiper, other projects came to mind. The other students and I, including Al Binder and Bill Hartmann, and later Toby Owen, just decided to do some of those things ourselves. We asked Dr. Kuiper if we could buy a flask of phosgene, for example, or carbonyl sulfide or some other chemical we thought could conceivably be in some planetary atmosphere and he would say, "Oh yes, go right ahead. Here's the funding number, go and do this". His blessings also extended to our requests for telescope time at Kitt Peak and on the new Catalina telescopes, and in various combinations the other students and I pursued projects of interest to us. So we had a tremendous amount of freedom in that respect, for which I am extremely and eternally grateful, and I have tried to pass that attitude on to students over the years as best I can. So we greatly appreciated the availability of resources and the support that he gave us, and let me just add this is support he really did not have to provide.

During those years in the 1960s, Kuiper developed a strong interest in the potential for making infrared astronomical observations in a high-flying airplane, above the great majority of the water vapor in the atmosphere. Flying out of NASA Ames Research Center, a Learjet and a four-engine Convair 990 aircraft were outfitted for telescopes and related gyro-stabilizied optics. Kuiper's own work in airborne astronomy and his enthusiasm for the importance of high-altitude observing capabilities gave a tremendous boost to the development of this subject. Recognizing his contributions to the field, NASA named the C-141A transport aircraft that was outfitted with a 92-cm telescope the Kuiper Airborne Observatory; it was put into operation in 1974, less than a year after Kuiper's death. In the course of Kuiper's earliest airborne astronomy, which was a program for the spectroscopy of the Sun from the Convair 990 at 40,000 feet altitude, there was an eruption of the Kilauea Volcano. Since the solar observing flights were being conducted from Hawaii, Kuiper and I traveled from Oahu to the Big Island to see the eruption. When we got there, there wasn't much going on in the volcano, and because the break between science flights was only about two days, we went back to Oahu to resume the work. So we came back to Arizona, having finished that

series of flights, and I asked Kuiper if I could go over and see the eruption if it resumed, and see about trying to do some spectroscopy on the fumes and flames coming out of it. He said, “By all means. Go”. He provided that resources to go to Hawaii, and to develop my own little project for flame spectroscopy. There had been a few publications, by astronomers, I might add, about field spectroscopy of volcanic flames, and there were some interesting unanswered questions: "What was burning?", for example. I took my project, and the spectrometer that Kuiper financed, to Hawaii when I moved there in 1970, and eventually the work paid off with the discovery of hydrogen combustion in air, and we got a nice paper out of it in 1973, the year he died.

Photo 18. G. P. Kuiper at Kilauea Iki, Kilauea Volcano, Hawaii, January, 1968. Snow-covered Mauna Kea on the horizon. Photo by D. Cruikshank

Photo 19. Robert Strom and G. P., Kuiper at Kilauea Volcano, 1968, observing an eruption in the floor of Halemaumau Crater. On the left is the volcano spectrograph that Dale built and used for photography of the spectra of volcanic flames. Photo by D. Cruikshank

Photo 20. G. P. Kuiper aboard the NASA Convair 990 (Galileo) with the large solar spectrometer used for the creation of a high-resolution atlas of the Sun with minimal atmospheric interference. NASA Ames Research Center photograph.

Photo 21. Kuiper and engineer Fred Forbes at Moffett field, boarding the Galileo aircraft, April 1967. Steve Larson photo.

Solar spectrometer (left side) on the NASA CV-990 aircraft for infrared spectroscopy.
A. B. Thompson, G. P. Kuiper, G. T. Sill D. Cruikshank photo
(July-August, 1968) See LPL Communication No. 126, 1968

Photo 22, by D. Cruikshank

Photo 23. D. Cruikshank, G. P. Kuiper, F. de Wiess, and G. T. Sill, circa 1969, with the long focal length solar spectrometer. NASA Ames photograph.

I raise that as an illustration of his support of student initiatives to do new things. We learned in the lab what the capabilities of the instruments were and ideas for new things came to us. Kuiper encouraged us to move ahead with our ideas, and often supported them with the financial resources available to him. I should say that at that time he had a lot of money from NASA and other sources, so it was not a hardship for him to grant these favors, but he certainly did so without, as far as I can recall, any hesitation at all. That was a wonderful thing. It's worth adding that he was not only generous with tangible resources, but with his trust that we would do things that were scientifically relevant and that we would do them carefully.

DS: He had equipment that was world class, driving enthusiasm, but it seems tha he had pretty much; abandoned stellar astronomy?

DC: Not entirely. It was clear there was work to be done in the mid- and near-infrared with the lead sulfide cells on stellar spectra, particularly on the late-type stars, so we used the spectrometer on the 82 -inch telescope at McDonald to look at stars. Kuiper's program included and early-type stars, too, but late-type stars were of particular interest. In the course of this work he discovered the bands of CO in some red giants. That was fun. To be there at the telescope as the spectrum slowly appeared on the strip-chart recorder….

DS: Real traditional astronomy with a spectrometer.

DC: Yes, we were tracing through the spectrum with the gratings and the strip-chart recorder, waiting for the bands to be resolved. We watched the excursions of the pen going up and down and up and down through the rotational bands of CO at 2.35 microns.

DS: Was this the first observation of that type? Finding molecules in these cool stars with a spectrometer.

DC: Yes, I think it was. Certainly one of the earliest. I am not sure if it was the very first. With the early-type stars you have the atomic hydrogen sequence, and that was fun because the predictions of the Bohr model told you where all of these series in the hydrogen spectrum should be, but they hadn't been seen in stellar spectra before.

DS: They had been seen in the lab?

DC: Some of the lines and series had been seen in emission sources in the lab, but absorption lines in the near- and mid-infrared hadn't been seen, I think. In any case, you are standing there at the telescope watching the lines come out one after the other, and another and another. Kuiper saw an element of elegance and beauty in this, which I picked up on at the time, and have come to appreciate more in later years.

DS: The strange thing is he had an incredibly fertile mind, incredible ambition and drive for all aspects of planetary science, yet he had very few students, you said there were like five. Something like that.

DC: Tom Gehrels is still ambivalent on whether he considers himself a student of Kuiper's but I think by the broadest definition he was. He got his degree at the University of Chicago in 1956, while Kuiper was there, and he was working on asteroids and stuff

DS: So Gehrels got his degree on asteroids and then came to Arizona.

DC: No, I don't think his degree was specifically on asteroids. Gehrels discovered the wavelength dependence of polarization in a variety of sources, stellar, and planetary. He pursued that for much of his career after I met him, and for the early part of his career it was the polarization of stellar sources.

DS: Okay. Now this is when Kuiper was still very active in stellar astronomy.

DC: Yes, that is right.

DS: Then Kuiper started this massive program of asteroid survey.

DC: Yes, with his Leiden compatriots.

DS: Gehrels picked that up and ran with it for most of his career?

DC: To a degree that's true. The asteroid photometry and survey began in the mid-1950s while Kuiper was still at Yerkes, and two major reports were published in 1958. Much of the work was done in Holland by C. J. van Houten, but van Biesbroeck and Gehrels were actively involved.

DS: You described to me earlier on that Kuiper was a typically Germanic person.

DC: Kuiper was perhaps not as formal as the stereotypical picture of the German professor but there was a distinct formality about him. At the same time he could be very warm and engaging, but you were always careful about what you said to him, or said even in his presence. I thought that he tolerated me because I was eager to learn. I wasn't the quickest student he had ever met, or the brightest, but he sensed in me an eagerness to learn and a persistence to carry on until I understood and finally got it right, which I still try to do. At the same time I was polite and semiformal without being obsequious. Now, this is my view of his view of me. He also kind of liked me because I was of mid-west American stock, I think.

DS: Easy to get on with.

DC: That's right. I never talked back.

DS: You always called him sir?

DC: No, it was always Dr. Kuiper. That was fine with me. I learned a style of interaction in my contact with him that persists to this day. I didn't always speak the way I do now. When you speak to foreigners that's something in itself. My experience in Russia taught me to speak more clearly and with a somewhat pronounced annunciation. And always to use correct grammar.

DS: Clark Chapman's quote on the LPL site from people who had an association with Kuiper says there was a natural selection effect. Unless you had a certain intelligence, but also a certain confidence, I guess, you just fell by the way. So only confident smart students could survive Kuiper.

DC: I endorse that, and I know many who fell by the wayside or didn't even get close because of the aura of this man. He was intimidating and he was frequently very abrupt. Dealing with that was a good lesson. I occasionally saw in his interactions with other senior people who were on a more nearly equal status as him, he could also be dismissive and abrupt and even rude. If you step back from that you realized you were like everyone else then it was easier to accept. While a single word from him could (and occasionally did) send a shudder down your spine, he was often quite charming and engaging.

DS: I think that is true of most character traits that if you understand them you can work with them. If you don't understand them, it is fatal.

DC: Yes, if you let yourself react immediately, and I was guilty of that a few times, and don't learn from the experience, the consequences may not be good.

DS: Now we were talking about your interactions with him. Now you worked on spectroscopy.

DC: Primarily yes.

DS: …and you published a lot of papers without his name on them.

DC: yes, that is right, again using the resources of the LPL.

DS: …and he was happy about that.

DC: Yes, he was happy about that. He wanted science. He wanted to be able to report back to NASA that the lab was doing well and getting good results.

DS: …but you also had five or six papers with him? How was that process?

DC: That process started with his suggestion that, "You go look at this and show me what you find." Some of the stuff I published with him consisted of collections of lab spectra that we took of gases of potential interest in connection with planetary atmospheres. One example was searching for carbon oxide gases on Venus, as well as SO_2 on Venus. The other papers we coauthored included a series of seven installments of an atlas of the high-resolution infrared spectrum of the Sun obtained with the NASA Convair 990 mentioned earlier.

DS: How did the process go? You wrote a rough draft, gave it to him, he tore it up?

DC: The papers were mostly brief, based on the spectra we got; we reproduced these tracings that we got out of the lab and at the telescope, and then the description was usually fairly perfunctory. I think there were cases where he wrote the draft and some in which I wrote the draft, and then we interacted.

DS: You don't remember him as being heavy handed.

DC: I don't. I think that was a fairly smooth process.

DS: No personal interaction problems in writing papers.

DC: Not in a negative way, no. On the other hand if I were sitting at his side and we were going through the paper and he suggested something then, by golly, that was going to be it. I have to say, too, that there were times when he corrected my English, and it was strange when a person whose native tongue is not English corrects your English, but he was right every time. He made my writing better, because he would critique my writing on these drafts, and also on my PhD thesis. The net effect of that was that I learned to write better as a result, and to use my words very carefully, because I know the great man is watching over me and is going to correct any grammatical or syntax error I might make.

DS: So the writing process went very smoothly.

DC: Yes, I don't have any recollections of problems.

Photo 24. G. P. Kuiper and Toby Owen at a conference in Marfa, Texas, late 1960s. Photo by D. Cruikshank

DS: It was science driven. Okay. Umm, he was married by this time, right?

DC: Long before I met him, yes.

DS: He married…

DC: Sarah Fuller.

DS: When he was at Harvard?

DC: Yes, when he was at Harvard. I seem to recall him saying that Sarah's father had donated the ground on which the Harvard College Observatory was built. So there was a connection that way. How he met her I don't know, some kind of social setting I presume.

DS: Throughout the whole period at McDonald Obsrvatory she was still his wife and he took her to Chicago and Arizona. Did she outlive him?

DC: Yes, she outlived him by several years. In fact she remarried after Kuiper died and they lived in Kuiper's Tucson home. She married a man called Roth. I was in touch with her during those years. That in the end did not work out well. I believe she married a third time, I'm not a 100% sure of that.

DS: This would have been in the 80s.

DC: Well Kuiper died in 1973 so this must have been a few years later.

Kuiper the Editor

DS: There's something you have not made any mention of and another thing that really interests me. Okay, Kuiper the scientist striving to get new data, striving to understand the solar system, new insights, getting to know the planetary system better, but Kuiper the editor? There is one distinguished Chicago astronomer, whose name is in my files and I can't remember, he wrote an obituary for Kuiper, and in the obituary he says that Gerard Kuiper's greatest contribution to astronomy were these books he edited, the nine volumes on stellar systems and the four volumes about the solar system. This Chicago astronomer says these are his greatest contribution, not his long list of discoveries. How do you react to that?

DC: There were two compendia that were assembled at more or less the same time. The compendium on Solar System studies was to consist of five volumes, but in the end only four were finished. The compendium called Stars and Stellar Systems was somewhat larger, maybe as many as twelve volumes, each with its own editor. Kuiper was the general editor of the series. Kuiper once told me that he had outlined the entire stellar series in a concentrated effort that took two weeks. After the University of Chicago Press agreed to publish it, Kuiper then sought the buy-in from the individual volume editors.

It seems to be a common perception that Kuiper undertook these volumes in order to leave a legacy. And in the view of the writer you quoted the volumes did indeed constitute a legacy, but I think his individual discoveries were highly significant, both in stellar astronomy and in the study of the planetary system. I have used those compendium books, on and off, some of them, some not. They are a valuable collection of material, although most of it is outdated by now. The planetary series was the first such compendium, in English at least, that had ever been published, certainly the most comprehensive. The fact that so many other senior astronomers and scientists signed on early to both series I think is testament to the value of them.

DS: He was interested in the accumulation of all knowledge, not just his own. The solar system series my first introduction to this topic and the meteorite volume, "Meteorites, Asteroids, and Comets", I think it was, I studied cover to cover. The others I had…

DC: Well, the first one was on the Sun, then "The Earth as a Planet", and then "Planets and Satellites". The fourth volume is the one you mentioned. meteorites.There was a plan for another, on planetary atmospheres, ….there they all are, on the very top shelf of my book case.

DS: [Looking at the books on the shelf]. Oh, the Moon was in there, with meteorites.

DC: There was an atmospheres book before that, from a symposium organized by Kuiper on the fiftieth anniversary of Yerkes Observatory. That appeared in two editions. That was a very good book. I personally thought that the compendium book on the "Earth As a Planet" was very important for defining the way we now think of the Earth as a planet.

DS: That was 1958.

DC: Something like that. I believe that geologists didn't think of the Earth in a planetary context. I would guess that subconsciously, anyway, that book had an important influence on shaping our view of the Earth as a component of the planetary system.

DS: But you wouldn't endorse the idea that the books were more important than his discoveries?

DC: Were they more important? I think that is too close to call. The individual discoveries are very important view, the stellar discoveries, working out the state of Beta Lyrae geometry, the effect on the spectra. That was a major discovery.

DS: Was that the first time that sort of thing had been done?

DC: I think so.

DS: People can learn this much about stars from the spectra?

DC: Yes, this was the first time that there was clear evidence for the transfer of material from one object to another. That must have seemed like an amazing concept at the time. Now we take it for granted.

DS: He wrote several papers on that and then a Sky and Telescope article.

DC: Yes, and then the determination of the bolometric correction. I am told that this was in use for very many years. Then there was the double star work, the white dwarf work, and so on. I believe all those had really important influences in setting the foundation blocks of modern astronomy. Kuiper's colleague Otto Struve, wrote a book called "Astronomy of the 20th Century" (1962, with V. Zebergs), and he honors Kuiper's contributions to astrophysics, which are well laid out and presented in a way that would convince an objective reader of his importance.

DS: He made very important contributions to stellar astronomy and then people have said he founded planetary science.

DC: That's right. Yes. By the time I met him in the summer of 1958 he had already had this amazing career in mainstream astronomy.

DS: He had two careers.

DC: He really did, and they interlocked at infrared spectroscopy.

DS: Talking of infrared spectroscopy, tell me what you know about the detector.

The Infrared Detector.

DC: Kuiper told me that he had, as part of his work with the Alsos mission following the war, discovered that German scientists had made progress on infrared photo detectors made of lead sulfide. Kuiper was proud his work as part of the war effort, particularly the wrap up at the end of war, interrogating scientists and so on. But let me make a quick aside to another story related to Kuiper's work at the end of the war. It concerns his rescue of Max Planck before the Russians could get to him. Have you heard this story from anyone else? I wish I had kept notes or that someone

else had recorded this. In any case, Germany capitulated, the Russians were coming in from the east, the Americans and allies were coming from the west, they were going to meet in Berlin. Ultimately they did, but Max Planck was holed up in the outskirts of Berlin, somewhere in the German countryside. Kuiper tells the story that the American soldiers found out where Planck was and that our side had a chance of getting to him before the Russians did. Kuiper had the equivalent rank of a colonel, or something like that, with sufficient authority to appropriate a jeep and a couple of people. So they took a jeep and raced off across the countryside to Planck's house and found him and his wife before the Russians could get there. They all sped away to the west to the Allied forces, surely saving Planck from whatever the Russians might have had in mind. Which may have been good, but likely not. Kuiper told me, and Mrs. Kuiper also told me sometime later, that each year at Christmas they would receive a card from the Plancks. Even after Max's death, Mrs. Planck would send a Christmas card laced with gratitude for the rescue so many years earlier.

Photo 25. G. P. Kuiper and Ronald Schorn, March, 1973, discussing Kuiper's work on the Alsos mission. Photo David Morrison

Well anyway, during the Alsos interrogations they found out about these infrared detectors, and either got the recipe or possibly some detectors--it was never clear to me about that, took that to Robert Cashman in the US whom I guess he knew in some other context, and on the basis of that collaboration apparently emerged the lead sulfide detectors that were ultimately used in Kuiper's spectrometers. I think that originally all that material was classified. I don't know if Kuiper got his hands on one while they were classified or after. He never made that clear. I certainly didn't ask. He would tell these stories from time to time, and with a clear sense of pride.

DS: Well, he brought one back, or he brought the information back, from Germany…

DC: Certainly that.

DS: … so he knew all about it before it was declassified. It was declassified quite early on, a year or so after getting back.

DC: Yes, it was.

DS: The spectrometer article was published in 1948.

DC: What I was about to say is that he would tell these marvelous stories , but only rarely did I feel that I could ask questions related to them and probe deeper. It was again because of the nature of our relationship. I would have loved to have known more but it didn't seem appropriate under most circumstances to push and ask. Now that I have the self-assurance that I have in my later years, I wish had the opportunity to discuss these things with him.

DS: Youth is wasted in the young.

DC: Yes, it is. I have to admit, even while being aware that this conversation is being recorded, I can admit that I have had dreams over the years of interactions with Kuiper where I was able to probe and press and learn more and understand more in depth about those and other events.

DS: Okay, you were just talking about the lead sulfide detectors, the thought I wanted to explore with you is this. In looking at Kuiper's life we are trying to find new insights into the history of planetary science and solar system exploration, to look at the history of the whole field from Kuiper's perspective, because it would give us a new perspective, and one of the impressions I got looking at his publications and looking at his career is that could I say that it was the second war and Germany's invention of the infrared detector that really created the planetary sciences as we know it? ? Isn't that Kuiper's secret of success, that infrared detector?

DC: It was a pivotal moment. I don't think there is any question about that and its influence on opening up the near infrared spectral region on the subsequent history of the study of the atmospheres and surfaces of the planets. I don't think you could call it a defining moment for planetary sciences itself but at least a critical juncture. It expanded into an entirely new wavelength range the work that had been done extensively in the visible part of the spectrum. It opened up the ability to detect molecules in planetary materials that you simply cannot do at shorter wavelengths.

DS: Molecules are critical to planetary science.

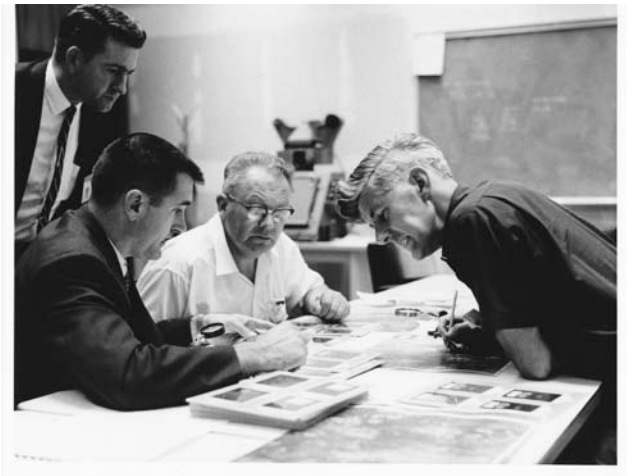
DC: They certainly are, not only in gases, but in planetary ices, and minerals. I have never thought of it in quite those terms, but it certainly merits exploring the view that it was this development, this adaptation of this technology developed for the military, that really did impact and change the landscape.

DS: The amateur historians really love to oversimplify these things. I heard someone say that the history of science is the history of inventions.

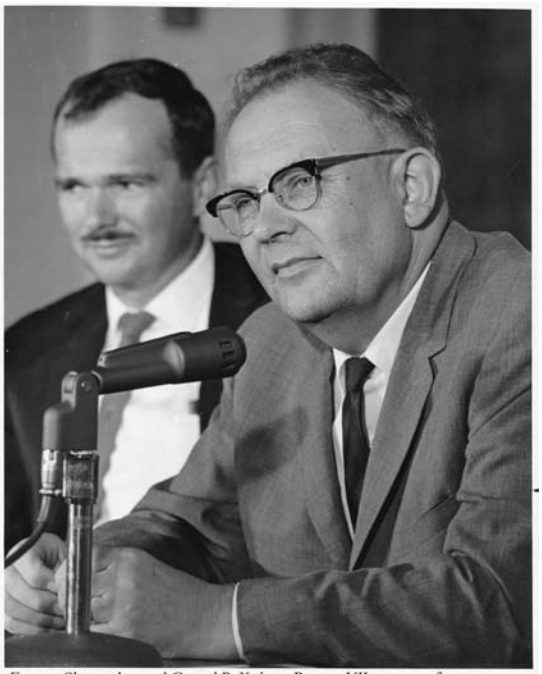
DC: That's right. Martin Harwit has written extensively on this very phenomenon. The development of a new technique, or extending into a new wavelength region, is really the key to many major milestones in science of all kinds, particularly physics. That certainly applies to astronomy and astrophysics, as well.

DS: …and cosmochemistry. That is where I first became aware of this. You can see the history of the discovery of elements ties in with the invention of analytical techniques. I think that still goes on today.

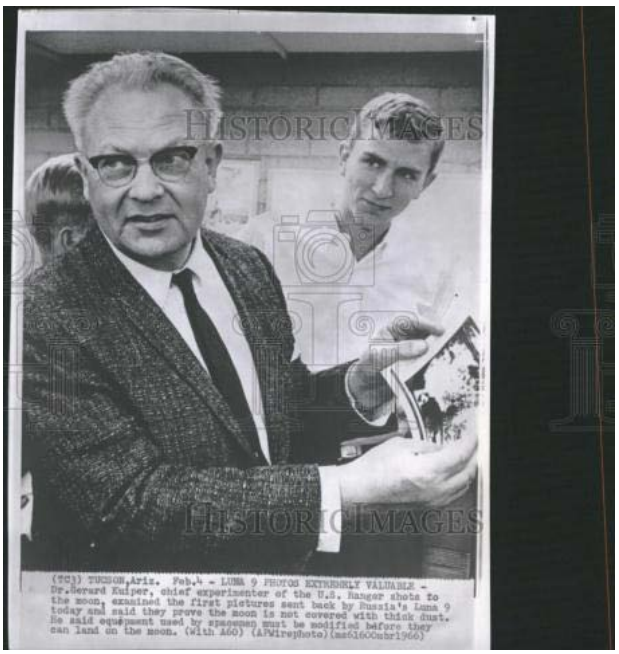
DC: I am writing this down, a note to myself. Carry on.



Photos 26-30. G. P. Kuiper and colleagues (Whitaker, Shoemaker, Hartmann) during the Ranger era in the 1960s. JPL and other sources.



Eugene Shoemaker and Gerard P. Kuiper, Ranger VII press conference
NASA photograph 64-H-2215



G. P. Kuiper, Ranger press conference, JPL

Gerard P. Kuiper, Ranger VII press conference NASA photograph 64-H-2214

Kuiper the man

DS: The last thing I want to pursue with you, and I really do appreciate you spending so much time with me on this, and I have a feeling this won't be the last time,…

DC: I hope not.

DS: …is that you said you are going to dream of Kuiper tonight?

DC: Well, I have done so over the years. It's been largely in a positive way. In a broader, less specific sense, the opportunity sometimes arises to learn more about a person's insights and so on, if one isn't too timid and the other person isn't too intimidating. I now wish that I had not been too timid to ask questions of Kuiper about some specific things that were so important in his research career and important to the development of planetary science. But he was intimidating, and could be so abrupt and dismissive. Still, with hours and hours together at the telescope, day-long drives back and forth to the observatory, working side by side in the lab, and in social settings, I feel that I got to know him fairly well, even in view of the wide gulf in age, experience, and intellect between us. I have learned from my experience with Kuiper to make an effort when dealing with younger people to make them comfortable asking more pertinent and even personal questions. I think the way a person learns to be a scientist is to learn how other people work, not just the mechanics of it, but in particular the way people think. One needs to observe the way they come up with ideas or reject them, whatever the process is. And so the more comfortable one is when a young person is talking to an older, more experienced person, the better that learning experience and information transfer can be.

DS: You are touching on a big topic of what makes Kuiper what he was. You were with him as an undergraduate during the summer periods, then you were his graduate student for five or six years,

DC: 1961 to 1968.

DS: Seven years as a PhD candidate and he still stays with you now.

DC: Yes, that's right.

DS: ….and you mentioned that Tom Gehrels[1] is still…

DC: although he may be less willing to admit it.

DS: Well you'll speak for him now! So Kuiper gave you an important education, providing unique facilities, a wealth of intellectual territory, but he didn't give you one thing which you are trying to get by dreaming.

DC: Right. That's right, I am still trying to understand him and his way of thinking. He wasn't able to put me at ease so that I could interact more effectively and more intimately, let's say, and I long for that. It is not a frequent dreaming pattern, but I do remember having these dreams, and in them I have found the courage to ask penetrating questions. In the dreams, he answers those questions, but of course when I wake up, I can't remember what he said!

DS: It tells you something about him, but it also tells you something about the way the generations interact and what is best for culture in the long run, about the nature of the mentor-student relation. So, you got half a deal there.

DC: Yes. It would be fascinating to explore these issues with his offspring, Silvia and Paul. As seen from the outside, it did not seem like a particularly good father-child relationship. At the same time while we were all living there in Tucson he was elected, or appointed, or named father of the year, and I have to say that those of us who knew the family were a bit surprised. [Laughter]. On the other hand, Silvia, whom I know better than I know Paul, speaks glowingly of her father. She has only good memories. Perhaps Paul does, too, I don't know.

DS: Complicated business.

DC: Yes.

DS: There are many ways to be a father.

[1] Tom Gehrels passed away on July 11, 2011, after this interview was conducted.

G. P. Kuiper and N. U. Mayall, Kilauea Volcano, late 1960s. Photo by D. Cruikshank

Photo 31. Kuiper and Nicholas Mayall at Kilauea Volcano. Photo by D. Cruikshank

Photo 32. Kuiper and Ewen Whitaker at the LPL 61-inch telescope, Catalina mountains. LPL photo.

Photo 33. Kuiper and Robert E. Murphy at the 61-inch dome and dormitory, Catalina Station, January, 1969. Kuiper was enormously proud of the observatory facility that he built there, and delighted in showing it to visitors. Photo by D. Cruikshank.

Laurel Wilkening and Gerard P. Kuiper, Tucson, December, 1973

Photos 34 and 35. Kuiper at his desk at the Lunar and Planetary Lab. (LPL Photo), and perhaps the last photo taken of Kuiper before his death. He is shown here in December, 1973, about to board a light aircraft piloted by Godfrey Sill. Kuiper, Sill, and Laurel Wilkening were flying around in southern Arizona and northern Mexico in search of potential observatory sites. Photo probably by Godfrey Sill.

Note: A view of Kuiper's life and professional work is found in the following two references:

Cruikshank, Dale P. 1974. A 20th Century Astronomer. *Sky and Telescope*, March issue, 59-64.

Cruikshank, D. P. 1993. Gerard Peter Kuiper. *Biographical Memoirs National Acad. Sci.,* 257-295.

Note: The material in this interview was extracted by Derek Sears from the larger text of the full oral history interview that he conducted with me because it deals specifically with Kuiper in some aspects of his career and personality. This document gave me the opportunity to add a number of photos of Kuiper and associates from my collection, although they do not exactly follow the subject lines of the interview. I think these pictures enhance the story. Dale Cruikshank, Oct. 7, 2012