



Report

Oral Histories in Meteoritics and Planetary Science – XX: Dale Cruikshank

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Abstract—In this interview, Dale Cruikshank (Fig. 1) explains how as an undergraduate at Iowa State University he was a summer student at Yerkes Observatory where he assisted Gerard Kuiper in work on his Photographic Lunar Atlas. Upon completing his degree, Dale went to graduate school at the University of Arizona with Kuiper where he worked on the IR spectroscopy of the lunar surface. After an eventful 1968 trip to Moscow via Prague, during which the Soviets invaded Czechoslovakia, Dale assumed a postdoc position with Vasili Moroz at the Sternberg Astronomical Institute and more observational IR astronomy. Upon returning to the United States and after a year at Arizona, Dale assumed a position at the University of Hawai'i that he held for 17 years. During this period Dale worked with others on thermal infrared determinations of the albedos of small bodies beyond the asteroid Main Belt, leading to the recognition that low-albedo material is prevalent in the outer solar system that made the first report of complex organic solids on a planetary body (Saturn's satellite Iapetus). After moving to Ames Research Center, where he works currently, he continued this work and became involved in many outer solar system missions. Dale has served the community through his involvement in developing national policies for science-driven planetary exploration, being chair of the DPS 1990–1991 and secretary/treasurer for 1982–1985. He served as president of Commission 16 (Physics of Planets) of the IAU (2001–2003). He received the Kuiper prize in 2006.

EARLY HISTORY AND UNDERGRADUATE TRAINING

DS: Why don't you start by telling me about your career before graduate school?

DC: Yes. I would be happy to. I got interested in astronomy at a very young age, around 13. In 1954 there was a total eclipse of the Sun that crossed Minnesota and Michigan not far from where I grew up in Iowa and I persuaded my mother to take me and a high school friend to go view the eclipse in Michigan. This was the eclipse of June 30, 1954. Seeing the eclipse of the Sun, even though indistinctly and through a bit of fog convinced me that I really had to be an astronomer. I started out with an interest in the Sun, but my interest quickly spread to the planets as a result of the books I started reading. I bought my first astronomy book in

1951 or 1952. It was *Sun, Moon, and Stars*, by W. T. Skilling and R. S. Richardson. I later bought *Exploring Mars* by Richardson and became especially intrigued with the planets. My high school years were spent with amateur telescopes looking at the planets and Sun with the eventual intent of being an astronomer one day. It never crossed my mind to do anything else professionally. In 1956, there was a perihelic apparition of Mars and that attracted a lot of attention worldwide, even in Iowa, and at the local municipal observatory I assisted in managing the crowds that came through. They had an 8 inch telescope there and I made good friends with Philip S. Riggs who was the astronomer-trained mathematician at Drake University. He didn't do any astronomical research at that time, and the observatory was basically a public facility. During that apparition there was an article in the local newspaper



Fig. 1. Dale P. Cruikshank.

about Gerard P. Kuiper who was observing Mars through the 82 inch telescope in Texas. He had seen an interesting cloud and that was written up in one of the Des Moines newspapers. So I picked up on his name early on, associating him with Mars in particular.

Well, I graduated from high school in the spring of 1957, and I went to Iowa State University while at the same time making enquiries about graduate school, even though I was only in my freshman year. I had heard about Yerkes Observatory and the astronomy program at the University of Chicago. I wrote asking about graduate studies and I immediately got a reply from Joseph Chamberlain at Yerkes who was in charge of student and summer appointments and all that. He mentioned the summer opportunities for undergraduates, and after applying, I got a summer job at Yerkes.

DS: You were an undergraduate physics major?

DC: Yes, a physics major. At Yerkes that first summer I was paid \$3.50 an hour, an immense salary at that time, but it was only for 20 h a week. That was enough to live on. I had a room at the boarding house that was operated by the eminent astronomer George van Biesbroeck and his wife and sister, who ran this facility specifically for visiting astronomers and students. It worked out very well.

DS: Tell me something about Joseph Chamberlain.

DC: He was an atmospheric physicist with an interest in planetary atmospheres. He wrote a couple of books including an advanced monograph "The Theory of Planetary Atmospheres" that was quite popular. I had the pleasure of writing something with him many years later.

DS: So what were you doing at Yerkes?

DC: My job was to work in the service department and with a group in the photographic laboratory. I mostly made slides and prints of the sky, the observatory, and photos of important astronomers, for sale to the public and educators. One of the other projects going on there involved the early phases of the preparation of Kuiper's photographic lunar atlas, for which he had funding from the US Air Force. The Moon had potential military uses and the Air Force was funding a couple of academics around the country to gather information, especially pictures of the lunar surface. They wanted a uniform set of prints of the best photographs available. Well, I was involved in helping make these prints. Less than two weeks after I arrived at Yerkes I found myself in the darkroom along Kuiper's side processing 16 by 20 inch prints that we made from the best original negatives available, some quite old. That was a tremendous thrill, being with this man that I had come to know through newspaper articles and mentions in books. It was also a thrill because having grown up in Iowa I had never previously met a foreigner. Yerkes was crawling with foreigners. The van Biesbroecks were from Belgium, Kuiper from Holland, Chandrasekhar from India, and so on. That alone was an amazing experience.

DS: I was on the faculty at the University of Arkansas for 30 years and some Arkansas undergraduates still have that experience!

DC: All-in-all it was a very heady summer and I had so much fun. At the end of the summer, I went to Chamberlain and thanked him. He replied that they were happy with my work and he invited me back next summer. So I went back in the summers of 1959 and 1960, and even spent some Christmas vacations at Yerkes. The people at Yerkes were very nice to me.

DS: So you went in over your Christmas vacation! How did that work?

DC: Well, I wasn't paid anything. I would go see my friends, do a few odds and ends, but most of all just be there. That in itself was such a thrill. An important component of this whole connection with Yerkes was that the 40 inch refractor, this ancient telescope, was grossly underused. They were doing a certain amount of lunar photography to support the lunar atlas project but that could only be done at certain times of the month. During the darker nights of the month there was a continuing program of stellar astrometry that had been going on about 60 years, but the telescope was idle for much of the time. Graduate students in residence at Yerkes could use the telescopes, and Kuiper, as the director, authorized the summer students to make observations as well. After some training several of us, including Alan Binder, who also stayed in the field, were

given to use this massive telescope. It was an unimaginable thrill. In the daylight hours we would look at Mercury and Venus, and the other planets after dark. Then there was the library, which is one of the treasures of this country for its depth and completeness.

DS: So Chamberlain was in charge and you found yourself working with Kuiper?

DC: Chamberlain was generally in charge of the students, and I worked with Kuiper and the group of people he had assembled there. After that first summer, two Moon experts, Ewen Whitaker and D. W. G. Arthur, both from the United Kingdom, joined Kuiper's group. At that time, in the whole of the United States there was almost no professional astronomer (or geologist) with any interest in the Moon.

DS: Did Kuiper have a reasonably big team at that point? He had these two folks, and you?

DC: Well, I was at the level of a calculator and photographic assistant. In addition, there was a geologist Carl S. Huzzen, and an advanced graduate student, Elliott P. Moore, as well as a few more individuals who made calculations and worked in the darkroom. All the people I have mentioned, including van Biesbroeck, who did not work on the lunar atlas project, eventually moved to the University of Arizona when Kuiper left Yerkes and moved there in late 1960.

DS: So Kuiper resigned his directorship and left Yerkes to go to Arizona, and at some point you graduated with a bachelor's degree?

GRADUATE SCHOOL

DC: Yes, bachelor's in 1961 from Iowa State University. By this time, Kuiper had started the Lunar and Planetary Laboratory at the University of Arizona, originally a part of the Institute of Atmospheric Physics, but then later a separate entity. As I was applying for the graduate program, I worked in Kuiper's spectroscopy laboratory with fellow students Alan Binder and Toby (Tobias C.) Owen. Bill (William K.) Hartmann was another incoming graduate student who worked with Kuiper, but on lunar studies rather than on spectroscopy. Toby was a few years further along than the rest of us. The spectroscopy laboratory was primarily focused on planets, but also the near-IR spectra of stars. An atmospheric scientist named Leon Salanave helped redesign Kuiper's original spectrometer to make use of high efficiency and inexpensive replica diffraction gratings, which were superior to prisms. The new spectrometer was greatly superior to Kuiper's original instrument. With this new spectrometer and improved lead sulfide IR detectors, on the new 36 inch telescope at Kitt Peak, and the 82 inch at McDonald Observatory in Texas, we did a lot of observational work collecting

spectra over 0.5–2.5 μm of the planets and stars. In that part of the spectrum, there is an enormous richness of diagnostic spectral features both in stars and planets. So it was a very productive time.

DS: So near-IR spectroscopy of planets was the topic of your Ph.D. thesis?

DC: That was the work in the lab. In the end I carved out a piece which was to do near-IR spectroscopy on isolated regions of the Moon. In the pre-Apollo years I was trying to use reflectance spectra to determine lunar mineralogy and with Kuiper's modified instrument mentioned earlier I was able to isolate regions on the Moon about a kilometer in size and thereby explore the interiors of several prominent craters (Plato, Copernicus, Aristarchus, etc.), as well as selected regions in most of the maria.

DS: Did you do mineralogy? You found basalts?

DC: Yes. The first pyroxene band at 1 μm had been recognized, and some people give me credit for recognizing the second pyroxene band between 1.6 and 2 μm . By comparing the 1 and approximately 2 μm pyroxene band others have found ways to get Ca and Mg abundances of the pyroxene. In addition to the telescopic work, I was irradiating basalt samples in the laboratory using a coronal discharge and a proton gun. I published a paper on that work from my thesis as well. Other people had tried irradiating silicate rocks, but there was the issue of whether or not the pump oil from the vacuum pump might be getting cracked and deposited on the samples. So we avoided that problem by using a liquid nitrogen cold trap.

DS: Did you compare the spectra with maria?

DC: Yes I did, but then Apollo happened and I lost interest in the whole thing. However, looking back at my thesis work now, almost 45 years later, it makes sense, and doesn't seem too juvenile. When I did the work, we had no guarantee that Apollo would succeed, and if it had not, my work would have had a longer shelf-life. But now, with more than 800 pounds of lunar samples in the bag and various satellites collecting data from lunar orbit, my work is clearly relegated to the status of a historical curiosity (even though it was published in *Science!*).

DS: So you entered the Ph.D. program in 1961 and you finished in 1969?

DC: Well, I finished up in '68. There were a couple of years of hiatus, and in the end I completed my degree in the University of Arizona school of geosciences.

POSTDOCTORAL PERIOD IN RUSSIA

DS: Then. . . ?

DC: During my university years I had become interested in Russian language and culture, and while

working in Kuiper's lab I became aware of a parallel spectroscopic effort in progress in Moscow by the young Soviet astronomer Vasily Moroz. I very much wanted to visit the USSR and see what Moroz was doing. In a sense he was Kuiper's counterpart because he had also developed a lead sulfide near-infrared spectrometer. At that time the United States and the Soviet Union, through their academies of science in each country, had an interacademy exchange program that allowed for a certain number of person-months per year to be exchanged between the two countries. I applied to that program with Kuiper's support and a letter of recommendation and I managed to get one of those 10-month appointments. The plan was to take my wife and two children. However, there were some extenuating circumstances. Recall that this time period was the height of the cold war. The Vietnam conflict was underway and there was tension in eastern Europe. In particular there was social and political unrest in Czechoslovakia. There was a world geological congress scheduled for Prague in August 1968, so I arranged to go to Prague, give a paper, and then go on to Moscow. So I set out for Prague on the 17th of August with the plan for my family to join me in Moscow on September first. I arrived in Prague on August 18, spent the 19th resting up, and the Congress began on the 20th. In the middle of the night, August 20–21, the Soviet Union invaded Czechoslovakia. I was staying in the home of a Czech family, and within hours the neighborhood was encircled by tanks. Military planes were flying over, the television station had been commandeered by the Soviet invasion force, and Prague was instantly isolated from the rest of the world. There were no trains, planes, telephones, or telegraph for a full week. Then, when services were restored, my dilemma was whether to return to the United States or go on to Russia and face a developing and potentially very serious international situation.

DS: Your wife and children were still in the United States?

DC: Yes. After a week in Prague I learned that there was to be a single train to Vienna. I wasn't even sure what country Vienna was in at that time, but I managed to catch that train. At the Czech/Austrian border attachés from the US embassy in Vienna came on the train to help any Americans that might be on board. So they arranged for a hotel room in Vienna where I camped out for almost a week. I called my wife, trying to decide what to do. Eventually the shooting in Czechoslovakia seemed to die down, and there was no major conflagration of eastern Europe, so I decided to go on to Russia and my family joined me a few days later.

The reception in Moscow was chilly. Even in normal times Americans were a curiosity in the USSR because there were very few of us, particularly over the winter

months. Soviet citizens didn't know what the future would bring in view of their invasion of Czechoslovakia and the outcry from the rest of the world. I found my hosts to be cordial but cautious. In the end, my time in Moscow, almost a year, worked out well and I got well acquainted with Moroz, his associates, and the students at Moscow State University. Moroz's official affiliation was the Sternberg Astronomical Institute of Moscow State University. The Soviet Academy had established the Space Research Institute and Moroz had a connection to that, but it was strictly off limits to foreigners. During that time, we did some observing in the Crimea with the 1.25 m telescope located at the Crimean Astrophysical Observatory. Together with Moroz I also used the larger telescope, the 2.6 m Shajn telescope, which was then the largest in Russia and Europe. We trucked Moroz's infrared spectrometer to the observatory and we published our results on Jupiter, Mars, and a jovian satellite in two or three papers, with some of Moroz's colleagues as coauthors. So by-and-large that 10-month sojourn in the USSR was successful. It also was a personal success since I became good friends with Moroz and his family. His wife, Irina N. Glushneva, was also an astronomer; she just recently passed away. I am still in touch with his daughter, whom I met when she was just 5 years old, and who has since become a planetary scientist. I have now coauthored a paper with her. That has been a great joy as well.

DS: Okay, so this boy from Iowa who went to the observatory of the University of Chicago and saw foreigners for the first time is now trying to avoid a war breaking out in Europe, and goes as a postdoc to visit a Russian observatory. So you grew up!

DC: Yes, I grew up! At the same time I had a wife and two kids.

DS: Tell me a little bit about your wife. You married her while you were a graduate student?

DC: That has an interesting component too. She and I are no longer married. She is in fact from Afghanistan. She was the first female student to ever leave Afghanistan to be educated in the West. Her father had been the Minister for Agriculture in the monarchy structure of King Zahir Shah that existed in Afghanistan at the time. In 1963, she came to the University of Arizona to learn home economics outreach, or rural extension, that she could take back to Afghanistan.

DS: You really were determined to become cosmopolitan.

DC: I certainly had never met an Afghan before I met her!

DS: It worked out just fine.

DC: Yes, we had two lovely children.

DS: You got married in what year?

DC: 1964.

DS: You have now had 10 months in Moscow and

POSTDOCTORAL PERIOD IN ARIZONA

DC:back to Arizona. Kuiper had offered me a research associateship, which I took. I was anxious to get back onto home turf, a familiar scene, familiar food, familiar work, and so on. Now by the time I got back there in mid-1969, two other astronomers had joined on, Uwe Fink and Harold P. Larson, both of whom were very much interested in infrared spectroscopy. However, they were using the interferometer technique instead of the old scanning diffraction grating technique I used. Kuiper was very much interested in interferometer spectrometers because they collected the signal simultaneously from a range of wavelengths without scanning. These advantages have proved important but limited, so there is still value in the old grating spectroscopy, particularly since we now have two-dimensional arrays and cross-dispersed grating systems.

DS: Is it easier to put on a spacecraft?

DC: Nothing is easy on a spacecraft. Interferometers require moving parts so gratings and two-dimensional arrays are generally better. A major problem is the data rate. The two-dimensional arrays produce a lot of data.

DS: So you are back in Arizona as a research associate, for a limited period? 1 year, 2 years?

DC: It was not limited. Kuiper still had a considerable amount of support from NASA at that time and I think in view of that and my track record with him that appointment could have gone on more or less indefinitely.

DS: You went back in 1969? The year Apollo landed.

DC: I went back in the summer of 1969, yes, and we were settled back in Tucson when we watched the televised landing of Apollo 11 on the Moon.

DS: How long were you there then?

THE INSTITUTE FOR ASTRONOMY, UNIVERSITY OF HAWAII

DC: I stayed there at the Lunar and Planetary Lab for a year. During that year there were a lot of successes, but also a lot of stress over scientific issues, and there were a couple of personal issues. It was clear the lab was going in a direction I was not prepared to go, particularly with the interferometry. I was happier with my old gratings and single element detectors. However, Fink and Larson were doing marvelous things with interferometry, and did so for many years, with both ground-based and airborne telescopes.

DS: So you left the LPL and Gerard Kuiper. When this interview is over I would like to ask for your

recollections about Kuiper, since he played such a critical role in establishing modern planetary science and you were one of the few students he had; in fact, you wrote his biography for the National Academy of Sciences. [This portion of the interview is available in the Supporting Information of this article.] Now, what was happening in Hawaii'?

In 1970, the astronomy program at the University of Hawaii', centered at the Manoa campus on Oahu, was beginning to thrive. Kuiper had essentially identified and opened up the summit of Mauna Kea on the Big Island of Hawaii' as an excellent observatory site, but then the University of Hawaii' took over. That site is particularly suited for infrared astronomy because at 4200 m it is well above most of the atmospheric water vapor that is such a big problem for the infrared. The University of Hawaii' had NASA help to build a 2.2 m computer-operated telescope that was about to begin operations. The primary interest of the Hawaii' astronomy program was originally the Sun and they had operated a solar telescope at Haleakala on the island of Maui for several years. However, they were getting into nighttime astronomy, including an interest in the planets. NASA had built the 2.2 m telescope specifically for planetary studies in support of projected space missions. Planetary astronomers David Morrison and William M. Sinton were already there, and they persuaded me to apply. So I went there in the summer of 1970, and I remained in Hawaii' for 17½ years. I regard the time in Hawaii' as my primary career building years. It set me off on paths that would not have been possible at any other observatory because of the attributes of Mauna Kea for infrared astronomy and the opportunities we had to build the appropriate instruments to exploit those qualities. Those years were very productive. I got a lot of results out, and a lot of recognition by planetary astronomers elsewhere in the country and abroad.

One consequence of the growing recognition of the importance of Mauna Kea was that during the time that I was there the governments of Canada and France got together to build their national telescope on Mauna Kea. The Canada-France-Hawaii' 3.6 m telescope was opened in 1979. NASA decided to build the NASA Infra-red Telescope Facility (the IRTF) there, also dedicated in 1979.

The United Kingdom science establishment located their big infrared telescope on Mauna Kea; it also came into operation in 1979. So in that 1 year and the years building up to it Mauna Kea was humming with activity, site testing, astronomers coming and going, new telescopes being built and tested and on and on. It was a very busy, dynamic, exciting place, and when I went over to the observatory from Honolulu to do some observing I invariably ran into people from all over the world. It

was a very exciting and stimulating environment. Working there was not easy, because of the exertion and reduced oxygen, but very rewarding.

DS: How did that work in those days? You were based in Honolulu and it's a fairly healthy plane ride, a 20-min plane ride right? You got to buy an air ticket, arrange for accommodation. How many times a year did you get to the telescope?

DC: It's 30- or 40-minute flight time. The transportation to Hilo was easy, but the drive to the summit over a terrible road was a bit arduous. During the 17½ years I was in Hawai'i, I was on the mountain at least once a month, because I was an observation-intensive astronomer. So I spent an enormous number of nights on the mountain and basically loved every minute of it. You are with nature at its roughest. Sometimes there were intense snowstorms. Although I come from Iowa, the most intense snowstorms I have ever seen were in Hawai'i, on Mauna Kea, even in July. For a time I was the associate director of the Institute of Astronomy with specific responsibility for the observatory. I remember calling up on the telephone a company in Colorado that manufactured snow blowers and I ordered the first snow blower that had ever been ordered by someone in Hawai'i! They could not quite come to grips with this for a while.

DS: It makes a point though. Altitude. That's why the telescopes are there! What would be your favorite three research results while you were there?

DC: Research results. Well, my colleagues and I discovered that Trojan asteroids have very low albedos. This was not previously known. It was possible to determine this only because we could detect the infrared signal, the heat signature, and simultaneously detect the visible light. These measurements taken together give an object's dimensions and thereby the surface albedo (Fig. 2).

Another discovery was the bands that were ultimately identified with sulfur dioxide on the surface of Io (Fig. 3). This was another observation we could make because we could work in the infrared; sulfur dioxide has very strong absorption bands between 3 and 4 μm . This was before Voyager got to the Jupiter system and found the active volcanism on Io. We found this extremely interesting spectral activity that at first we could not interpret. Sulfur dioxide volcanism was far removed from our ideas of what was plausible.

DS: So you found all these bands and didn't know what they were?

DC: Correct. We found the bands in the spectrum but couldn't identify them. The spacecraft detected SO_2 , but it was gas. Then it all began to make sense. We then figured out that we were seeing frozen SO_2 on the surface.

DS: Oh I see. I was sitting next to Ed Scott at a Meteoritical Society meeting when those pictures came in,

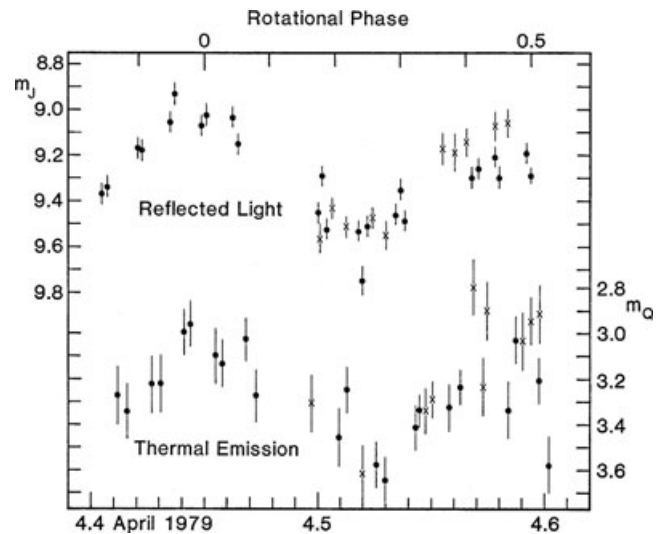


Fig. 2. Rotational lightcurves at visible and thermal wavelengths for Trojan asteroid 624 Hektor, by Hartmann and Cruikshank (1980). The synchronism of the two lightcurves demonstrated that Hektor is an elongated object presenting a varying aspect to an observer on Earth as it rotates on its axis. Before this observation, it was possible that the object was more or less spherical with a strong albedo contrast on its surface. As the largest Trojan asteroid, Hektor is a significant object.

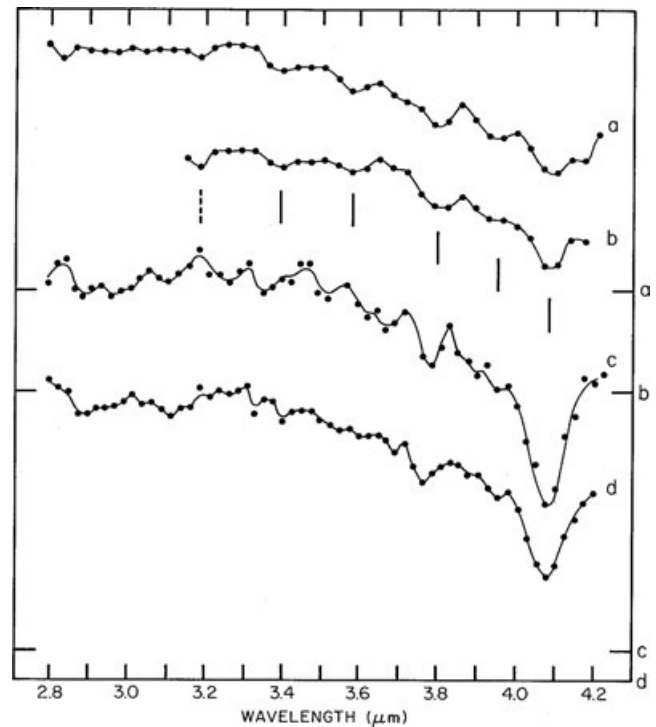


Fig. 3. Io spectra from Cruikshank, Jones, and Pilcher (1978). These are the first spectra to show any significant absorption bands in Io's spectrum, and while they were unidentified at the time the paper was published, the discovery of active volcanism on Io by Voyager 1 in 1979, plus the detection of SO_2 gas by Voyager 1, pointed the way to the identification of these bands as SO_2 frost on Io's surface.

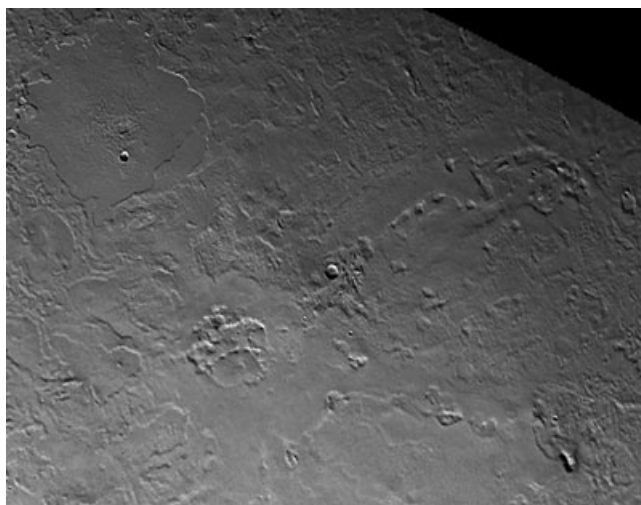


Fig. 4. This is a Voyager 2 picture of a section of Triton, obtained during the flyby of the Neptune system in 1989. This view, reprocessed by Paul Schenk, shows the frozen nitrogen ice sea that covers most of the surface. The discovery of nitrogen on Triton was reported in Cruikshank, Brown, and Clark (1984).

it was a plenary session, a big audience, when up came those pictures, a full disk picture of Io, and Ed leaned across to me and said, “It needs a shot of penicillin.”

DC: Yes, that’s right; I thought it looked like a bad pizza.

Probably my third favorite discovery, if I have to pick just three, was the detection of methane ice on Pluto, but just let me quickly insert a fourth choice—the discovery of molecular nitrogen on the surface of Triton (Figs. 4 and 5).

DS: More IR spectroscopy?

DC: Well the Pluto work was really a form of photometry. At that time the spectrometers were not sensitive enough to measure something as faint as Pluto. But together with Carl Pilcher and Dave Morrison we devised a photometric technique with narrow bands in the regions of deep absorption for various kinds of ice. We wanted to do an ice survey for small bodies in the outer solar system that were undetectable by spectroscopy because of their faintness. So we set up a set of filters and observed for Pluto, among other things, the ratio of the absorption in these various bands. Our measurements gave a strong indication that there must be frozen methane there. The important thing was that it was not rock. Silicate rocks would not have the signature that we found. Water ice would have had a different signature, but methane ice had a particular and predictable brightness profile over the filter bands we had defined, and that gave a strong indication of its presence on Pluto.

DS: So you found solid state methane?

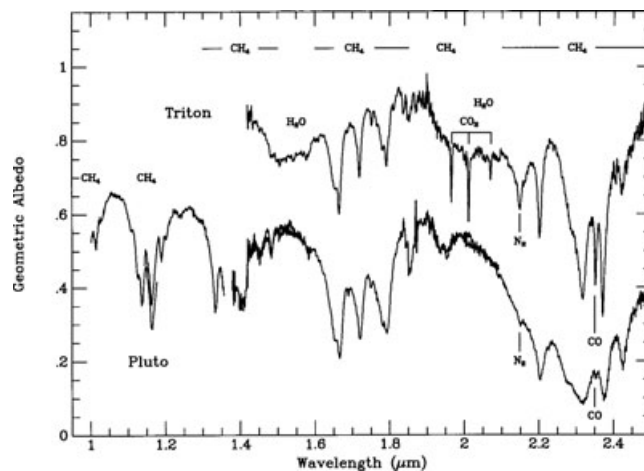


Fig. 5. Near-IR spectra of Triton and Pluto are shown with the major bands identified. The spectra were obtained by Cruikshank and colleagues, and elaborated (modeled) by two French thesis students, Eric Quirico and Sylvan Doute. This composite figure by Cruikshank has been published in various places, most recently in J. B. Dalton et al., *Space Sci. Rev.* 153,113-154, 2010.

DC: Correct, we didn’t see silicates. Now this was of interest on its own merits but the detection of an ice had a multiplicative effect. Our understanding of Pluto at the time was very incomplete. We did not know how big Pluto was, which meant that we did not know its surface reflectivity. The options were that it was a very low-reflective, large object, which was my hunch, or that it was a very highly reflective object that was quite small. At that time it was still thought that gravitational perturbations induced by Pluto on the orbit of Neptune were responsible for changes in Neptune’s orbit. Ultimately, that was supposed to have been responsible for the discovery of Pluto. But if it were perturbing the orbit of Neptune, Pluto would have to be pretty large to have sufficient mass and consequent gravitational effect, and so on. There were some very basic unknowns. The detection of the icy surface, as opposed to silicates or some other neutral material, told us that it was a highly reflective surface—more so than a rocky surface—and the planet is thereby likely to be smaller than the maximum estimates. If it really is that small—we estimated that it was about the size of the Moon—and if it had any kind of a plausible bulk density, you know, between 2 and 3 g cm⁻³ then there is no way that its mass could have affected the movements of Neptune. That, in turn, implied that the discovery of Pluto was an accident. So that entire chain of logic we laid out in our *Science* paper in 1976 when we reported our methane result, and it has all turned out to be correct

DS: So, 17 years at Hawai’i. Then what?

NASA AMES RESEARCH CENTER

DC: Well, then for various reasons I thought it might be time to leave Hawai'i. I had gone there not intending to stay so long in the first place. I wanted to get back to the mainland United States. I wanted to try different things. I had been in touch with the astrophysics community in the context of SIRTf which originally was the Shuttle Infrared Telescope Facility, later to be the Space Infrared Telescope Facility (and ultimately the Spitzer Space Telescope). In any event that was supposed to be a dedicated infrared telescope in space that would be primarily for astrophysics, but it would have a strong planetary impact as well. I was the planetary representative for that facility and I was appointed to the science advisory committee. Together with that came an opportunity to consider coming to NASA, here at NASA Ames. So after discussion with Mike Werner, he and Jim Pollack found that they could create a position for me here. I said my good-byes in Hawai'i, reluctantly in many respects, but at the very end of 1987 I came here to California to join NASA. I think my appointment officially started in January 1988 and I have been here ever since. Just as a minor aside, when I got here SIRTf was a going concern, but within 6 months or so it was picked up and moved to the Jet Propulsion Lab and so the key personnel, not including me, followed it down to southern California. I stayed on here.

DS: Would it be fair to say that your time at Hawai'i was basically ground-based astronomy and that you came to Ames in an effort to get involved in missions?

DC: In broad outline that is correct. It turned out to be a long process to get Spitzer approved, designed, built, and launched, but the mission turned out to be a remarkable success.

DS: How has your involvement in Spitzer gone? Has it gone as you wished?

DC: It's gone pretty much as I wished. One of the things I wanted to achieve when I signed on with Spitzer was for the planetary community to take notice of this facility. As I said earlier, it actually came through the astrophysics program at NASA as one of the Great Observatories, but I wanted to make sure the planetary community was aware of it, would get behind it, and use it, as well as use it myself, and that turned out very well. Planetary scientists have made use of Spitzer.

DS: Has all your research at Ames been Spitzer-related?

DC: No, I have maintained a strong interest in ground-based planetary science, particularly planetary astronomy, and I am involved in other missions.

One of my favorite results from ground-based planetary astronomy after I came to Ames is the study of

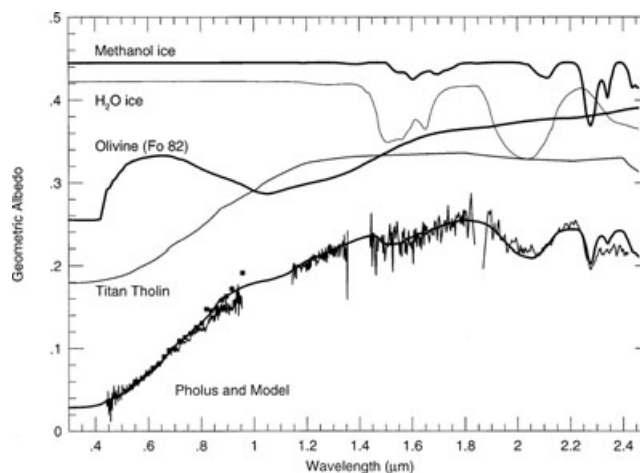


Fig. 6. The spectrum of Centaur object 5145 Pholus shows one of the very reddest surfaces in the solar system (so far equaled by only one other outer solar system body). In this figure from Cruikshank et al. (1998) we assembled a complete spectrum from various wavelength segments observed by different people and telescopes, and made the first rigorous scattering model for an outer solar system body. Notably, methanol was discovered in this process—the first time it was found on an object that is not a comet.

the Centaur object 5145 Pholus. We found from near-infrared spectroscopy that this curious outer solar system body is covered with a mixture of mafic silicates, water ice, red-colored organic solids, and frozen methanol. It is the first detection of methanol on a solar system body that isn't a comet (Fig. 6). In that and in other work since coming to Ames, I have benefited greatly from my association with Ted Roush, who has inspired and guided my learning about computational models of planetary surfaces. The Pholus work represents, I believe, the first successful model of a complex, multicomponent surface of a small, asteroidal or asteroid-like body.

During the time I have been at Ames the Cassini mission to the Saturn system was built and launched. It's been in orbit around Saturn for about 5 years now. I have a connection to that through the infrared spectrometer on board and that activity continues to be a part of what I do. Some of the aspects of that are planning for the encounters with Saturn's satellites as well as interpreting the data that continue to come in.

DS: What would you say is the most exciting piece of science to come out of Cassini?

DC: To me the most exciting and rewarding is our detection of CH stretching modes in the organics on two or three of the satellites. That organic material tends to be concentrated in the low-albedo material in those satellites (Figs. 4 and 5). Besides SIRTf, one of the principal reasons I came to Ames was because of Lou

Allamandola, who is an astrochemist here. He had set up a big astrochemistry lab and had several people working with him. That group, Lou in particular, pointed out to me some aspects of organic chemistry that make perfect sense in terms of the small bodies of the solar system and the origin and nature of the low-albedo material that partly or fully covers them. Lou has taught me what little I know about organic chemistry, and this has been a tremendous aid in my spectroscopic work with both ground-based telescopes and with spacecraft, particularly Cassini. With the capabilities of Cassini, I could now find spectroscopic features that correspond to the aromatic and aliphatic hydrocarbons that have been known for some time in the interstellar dust—the dust in the interstellar medium—and we had every expectation of finding these on planetary bodies in part because the carbonaceous meteorites had to come from somewhere and they contain these materials. The question before Cassini was, can we find these materials exposed on planetary surfaces, whether asteroids or planetary satellites or elsewhere in the solar system, and maybe someday even have some chance of tracking down the parent bodies of the carbonaceous meteorites. Previous attempts to do this, including some by me, had failed. It appears that the organic solids on bodies in the Saturn system are apparently connected in some ways to the organics in the interstellar medium. Material from the ISM was the feedstock of the solar nebula, and the bodies in the present-day solar system appear to retain some of that original interstellar material. We're still trying to understand if the organics we see on Saturn's satellites and on other solar system bodies represent original material from the ISM, material highly processed in the solar nebula and on solid planetesimals, or more likely, a combination of all of these possibilities.

DS: Were you involved in Galileo?

DC: I was not involved in Galileo. I tried to be, but I was on one of the teams that was not selected. It is a bit of a sore point. On the other hand Galileo, while it had some great successes, had such a tortured history in getting off the pad and on its way to Jupiter that I was probably spared a huge amount of grief. But even before Galileo was launched there was the Challenger disaster. Galileo was originally meant to be launched from the Shuttle, but the Challenger disaster and other factors caused the project to be delayed for several years. But Galileo was eventually launched, and although it was somewhat reduced in capability by the failure of the high-gain antenna on the spacecraft to open, in the end it was a wonderful success.

Another mission I have become involved with is the New Horizons mission to the Kuiper belt and the Pluto system. The spacecraft has been in flight now for a few years and has another three to go; it arrives in July of

2015, so fairly soon. So that's going well, and again it includes spectroscopy of an icy body, so I am looking forward to that. I'm especially gratified to be involved with New Horizons because it is a kind of culmination of my research interest in Pluto that began in 1976 with the discovery of frozen methane on its surface.

SCIENCE MANAGEMENT

DS: You and I have actually had a conversation once, about 10 years ago, when you were heading up one of the study groups for the last Decadal Survey.

DC: That's right. Yes. Yes.

DS: I was making the case for asteroid sample return and you were kind enough to ask me to go through those arguments in front of your panel. It was by telephone and you showed a Powerpoint while I talked. So I think of you as being one of these leaders in policy making.

DC: I have been involved in policy-related matters, but I consider myself extremely inept in that arena and uncomfortable. It's true I was involved in the first solar system Decadal Survey about 10 years ago, serving as the Chair of the primitive bodies subpanel, and in connection with that we had this exchange, of course. The report from that first survey came out in 2002.

Well, I am happy to say that the recommendations that came out of my subpanel were taken very seriously by the steering group, and the first New Frontiers mission, which is called New Horizons, is a result of that effort. The New Frontiers mission category, which I think was capped at \$650M, was a brand new program that emerged from the survey. There were things that we couldn't do for Discovery-class funding levels, but which did not need the giant mission budgets, so we needed intermediate class missions. NASA embraced this concept, and New Horizons to Pluto was the first such mission to be selected, with Juno being the second to be selected.

DS: So, Dale, you were talking about your participation in the Decadal Survey. Why don't you enlarge on that a little bit? Talk about how that process works, how scientists get involved in policy making, how you felt about it, how satisfying, what your concerns are, frustrations, etc.

DC: While I was still at the University of Hawai'i, I was very much involved in writing the annual proposal to NASA for the support of the laboratory and the planetary sciences program and how those two things were linked. NASA built the 2.2 m reflecting telescope in Hawai'i on Mauna Kea as a part of the planetary science program to help support planetary missions, and they continued their support of that telescope's operation as a part of the planetary research program. There were four

or five of us at that time who did our research at the Institute of Astronomy with that new telescope and so it was our responsibility to write the annual proposal for renewed funding, and of course to write the semiannual and annual reports on what we had been doing. I took over that responsibility fairly early on, although I was not listed as the principal investigator on the proposal. Primarily for administrative purposes, the PI was the Director of the institute John Jefferies, although it was realized by everybody that it was different group of people who actually did the work. My name became known at NASA and elsewhere partly because of that. At some point along the way I was invited to serve on review panels for other proposals that came in, I did some of that. I was also, for reasons I still do not understand, asked to formulate a small committee to examine NASA's research and analysis programs in science and to make any recommendations for the future. This preceded any involvement with Decadal Survey activities that were set up for planetary science. So I did that, and we wrote a report that was published by NASA in 1992, and distributed widely. Although the report was more or less ignored, it did somehow get my name in place for future appointments. I did a few other odds and ends for committees I served on in the meantime, but at the time the agency decided that it really wanted the National Research Council to do a Decadal Survey type report along the lines that the astrophysical community had been doing for some time, but this time focused on planetary science. My name entered the field as one heading up one of the subpanels, and I was appointed to head the subpanel on small bodies. Our panel was one of six topic-based panels that contributed to the first solar system Decadal Survey, which was made public in 2002 and published in 2003. That was a difficult activity, but the people I worked with on my panel were very talented and very supportive. Among us, we had a lot of good ideas and somehow together we pulled ourselves through the sorting out process and recommended to the steering group a set of missions and research topics that was adopted. In fact, the top mission in our category became the top mission for the entire survey, which was the New Horizons mission to the Kuiper belt and the planet Pluto. That mission, by the way, came in on time, on budget, and was ready for launch on schedule.

DS: That was APL.

DC: Yes, the Applied Physics Laboratory. So we are very happy about New Horizons and very encouraged that it is doing well in flight. It's covering almost a million kilometers a day and with a total flight time of 9½ years, you get an indication of how big the solar system is! It's doing well and we expect nothing less than great success.

Apparently, on the strength of the first Decadal Survey activity, I was asked to serve on the second Decadal Survey that has just finished its work. This one was headed up by Steve Squyres, the first one was headed up by Michael Belton. We are pretty happy with the programs that we have recommended in this second solar system Decadal Survey. NASA has given every indication from the beginning that they will embrace this not just as advisory but essentially as a blueprint so far as the budget allows. That's an uncertainty at this point as it always seems to be on an annual basis. But anyway we are happy with what we came up with and we are now in the process of presenting this to the community. So far the reception has been largely supportive.

Serving on these panels for evaluating programs and recommending new ones is a process I am not comfortable with. It requires a tremendous attention to detail, and while I consider myself a detail-oriented person, I'm not very good at the kinds of details that go into deriving cost and technical risks and other such things that have to be used in evaluating missions one against the other in order to establish a set of priorities. Now, I am happy to make judgments and comments on the science merits of various missions, but I am uncomfortable with the engineering aspects. Fortunately, both of these Decadal Survey activities have involved a mix of different kinds of very talented people. So my word is not the last word, or the first word, and in the technical or cost aspects the real experts are called upon to make those decisions. So, in all, the process is a very good one, well thought out in advance, taken very seriously by the participants, and the results I think are quite robust.

DS: This year they had the funding for specific missions evaluated independently.

DC: That's right. Each of the top 13 or 14 missions that filtered their way up in terms of scientific quality were looked at for cost and technical risk.

DS: Do you think the whole community gets polled on these Decadal Surveys or is it some elitist group of management type scientists who call it? How democratic is the process?

DC: I think it's very democratic. Certainly the opportunity exists for everyone to have a voice. This is done in the right way, that is to call town meetings in advance of the forming of the report at various planetary gatherings of related scientists like the American Geophysical Union, the Division for Planetary Sciences of the American Astronomical Society, etc. At the same time we invite papers to be submitted by the community and in this case of the Decadal Survey there were 199 white papers submitted.

DS: That is a lot of work. I think Arkansas submitted seven.

DC: They are only six or seven pages each to keep each paper focused and manageable. Those 199 white papers were prepared by various combinations of more than 1600 individual authors.

DS: Do you think that they were all read.

DC: I know they were all read, at least those that I was sent and in the area I was working on for the Decadal. I personally skimmed through all 199 of them. They were sorted by scientific area, the Moon, the inner planets, were put in one pile, those for the outer solar system were in another, and so on. The subpanels that dealt with those topics certainly read all of them. They combed them for suggestions. There were certain spurious white papers that didn't make sense in light of current realities, but they were all looked at, and I would say taken seriously.

DS: We said also that we have the exploration program, the human exploration program that is a much bigger program at NASA that offers a lot of science opportunities. But there the engineers and managers tell us what programs they want to execute and then NASA looks to the scientists to ensure we can get whatever science we can out of those missions.

DC: Yes, but that doesn't happen very often. There are a couple of examples. The LRO, which did come from the ESMD, but had a strong science component we are all happy about.

DS: But the whole lunar program, at least a year or two back, had an effort to involve science.

DC: Yes, and at that time we thought we would soon be back to the Moon. That's less clear now. The hope is now that if the human exploration vector is pointed toward asteroids that asteroids will be embraced by cross-cutting missions and research. If that is the case then there will be a very strong science component to come out of that effort.

DS: These are different times from the heady days of Apollo that Don Bogard was telling me about last week. I still can't believe we did this. We landed humans on the Moon. Apparently as little as 4 years before the landing NASA had given little thought to the science potential of what they were doing or, more specifically, to the fact that they were going to bring back rocks. Go to the Moon, bring the men back.

DC: Flags and footprints.

DS: Dale, thank you very much for providing us with this personal insight into the last five decades of planetary science.

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SELECTED BIBLIOGRAPHY

- Binder A. B. and Cruikshank D. P. 1964. Evidence for an atmosphere on Io. *Icarus* 3:299–305.
- Cruikshank D. P. 1977. Radii and albedos of four Trojan asteroids and Jovian satellites 6 and 7. *Icarus* 30:224–230.
- Cruikshank D. P. 1980. Near-infrared studies of the satellites of Saturn and Uranus. *Icarus* 41:246–258.
- Cruikshank D. P. and Silvggio P. M. 1979. Triton: A satellite with an atmosphere. *The Astrophysical Journal* 233:1016–1020.
- Cruikshank D. P., Morrison D., and Lennon K. 1973. Volcanic gases: Hydrogen burning at Kilauea volcano, Hawaii. *Science* 182:277–279.
- Cruikshank D. P., Pilcher C. B., and Morrison D. 1976. Pluto: Evidence for methane ice. *Science* 194:835–837.
- Cruikshank D. P., Jones T. J., and Pilcher C. B. 1978. Absorption bands in the spectrum of Io. *Astrophysical Journal* 225:L89–L92.
- Cruikshank D. P., Brown R. H., and Clark R. N. 1984. Nitrogen on Triton. *Icarus* 58:293–305.
- Cruikshank D. P., Tholen D. J., Hartmann W. K., Bell J. F., and Brown R. H. 1991. Three basaltic Earth-crossing asteroids and the source of the basaltic meteorites. *Icarus* 89:1–13.
- Cruikshank D. P., Roush T. L., Owen T. C., Geballe T. R., de Bergh C., Schmitt B., Brown R. H., and Bartholomew M. J. 1993. Ices on the surface of Triton. *Science* 261:742–745.
- Cruikshank D. P., Roush T. L., Bartholomew M. J., Geballe T. R., Pendleton Y. J., White S. M., Bell J. F., III, Davies J. K., Owen T. C., de Bergh C., Tholen D. J., Bernstein M. P., Brown R. H., Tryka K. A., and Dalle Ore C. M. 1998. The composition of Centaur 5145 Pholus. *Icarus* 135:389–407.
- Cruikshank D. P., Wegrzyn E., Dalle Ore C. M., Brown R. H., Baines K. H., Bibring J.-P., Buratti B. J., Clark R. N., McCord T. B., Nicholson P. D., Pendleton Y. J., Owen T. C., Filacchione G., Coradini A., Cerroni P., Capaccioni F., Jaumann R., Nelson R. M., Baines K. H., Sotin C., Bellucci G., Combes M., Langevin Y., Sicardy B., Matson D. L., Formisano V., Drossart P., and Menella V. 2008. Hydrocarbons on Saturn's satellites Iapetus and Phoebe. *Icarus* 193:334–343.
- Cruikshank D. P., Meyer A. W., Brown R. H., Clark R. N., Jaumann R., Stephan K., Hibbitts C. A., Sandford S. A., Mastrapa R. M. E., Filacchione G., Dalle Ore C. M., Nicholson P. D., Buratti B. J., McCord T. B., Nelson R. M., Dalton J. B., Baines K. H., and Matson D. L. 2010. Carbon dioxide on the satellites of Saturn: Results from the Cassini VIMS investigation and revisions to the VIMS wavelength scale. *Icarus* 206:561–572.
- Dalton J. B., Cruikshank D. P., Stephan K., McCord T. B., Coustenis A., Carlson R. W., and Coradini A. 2010. Chemical composition of icy satellite surfaces. *Space Science Reviews* 153:113–154.
- Hartmann W. K. and Cruikshank D. P. 1978. The nature of Trojan asteroid 624 Hektor. *Icarus*, 36:353–366.
- Owen T. C., Cruikshank D. P., Roush T., deBergh C., Brown R. H., Bartholomew M. J., Elliot J., and Young L. 1993. The surface ices and the atmospheric composition of Pluto. *Science* 261:745–748.

SUPPORTING INFORMATION

Additional supporting information may be found in the online version of this article:

Data S1. Recollections of Gerard Kuiper.

[Correction added on October 26, 2012, after first online publication: additional figures were added to Data S1.]

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