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Hugh H. Kieffer

RING PARTICLE SIZES AND COMPOSITION DERIVED FROM ECLIPSE COOLING CURVES AND REFLECTION SPECTRA

I am going to talk about two different aspects of the investigation of Saturn's rings. Neither one of these is entirely new. In fact, the first is somewhat old, but I would like to review it. It is the only discussion on the probable chemistry of the rings that is included in these talks.

In 1970 some people at MIT and I analyzed spectra from Saturn's rings taken by Gerard Kuiper (Pilcher et al., 1970). The gist of this study was that the reflection spectra of the rings, when properly normalized to absolute reflection spectra, corresponded very closely with the spectral reflectivity of water frost.

Figure 1 is from that publication, and I would like to point out the major features. The initial spectra of the ring and that of the Moon are shown. The ratio was done by hand-dividing these two spectra. The spectrum of water frost obtained in the laboratory is shown, and the correspondence with the ring spectrum was quite good. The spectrum of ammonia is also shown, as it was a strongly proposed candidate until this time.

One interesting feature is the small feature at 1.6 μm, which has a strong temperature dependence, something that we hadn't anticipated early in the game. Since then, studying Saturn's rings and the Jovian satellites, it has become clear that this feature is quite temperature-dependent and can give you an indication of the physical temperature.

In terms of what is published at the moment, the basic extent of this discussion was simply to point out that there is a very strong correlation between the spectral reflectance of Saturn's rings and that of water frost. This is a strong indication that, in fact, the rings are at least covered with water frost. All one can say here is that they are covered to the extent that the upper surface, which is contributing most of the reflected energy, is composed of water. A minimum thickness for the water frost is thus on the order of 200 μm, perhaps a millimeter.

University of California, Los Angeles.

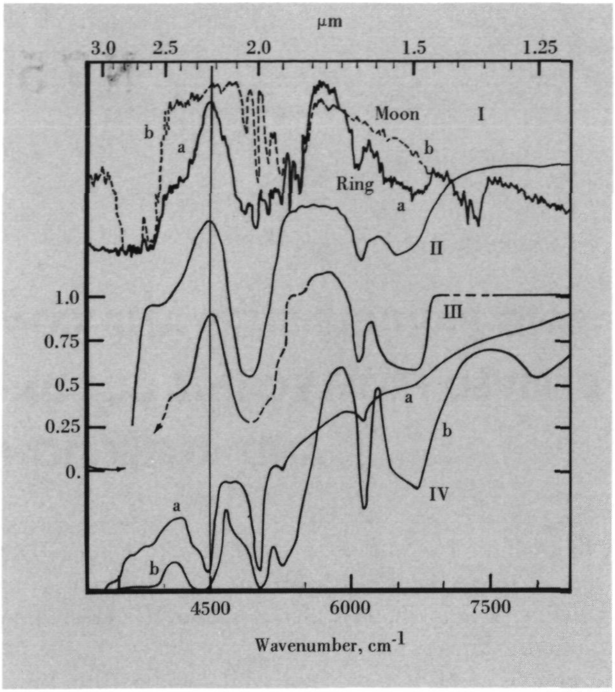

FIGURE 1.—*Comparison of reflectance spectra for* H_2O *and* NH_3 *frosts and Saturn's rings. Ia—The Saturn ring spectrum of Kuiper et al.* (1970). *Ib—Lunar comparison spactra. II—Fine-grained* H_2O *frost spectrum of Kieffer (1970). III—Normalized Saturn ring spectrum (Ia divided by Ib). IV—Kieffer's preliminary* NH_3 *frost spectra, fine grained a, coarse grained b.*

Figure 2 gives some indication of the temperature dependence of the 1.6 μm (6100 cm^{-1}) feature at several temperatures: somewhat below room temperature, at a temperature appropriate to the Jovian environment, and at a temperature appropriate to Saturn's environment. The feature strength increases with decreasing temperature. A thorough analysis of the equivalent width of that feature and its implication for Saturn has not yet been done. So far, this has not provided any improvement over the radiometric temperatures. Should a discussion of the physical temperature arise as opposed to the brightness temperature of the rings, it is possible that a detailed study of this band in the laboratory and improved resolution in the astronomic spectra could determine the physical temperature directly, without requiring knowledge of the equivalent cross section of the rings. The group at the University of Arizona has also been looking at this feature. At the moment the spectroscopic temperature is in good agreement with, but not as accurate as, the radiometric temperature.

For reasons which are not related to this workshop, we have been looking at some spectra of the Galilean satellites (Kieffer and Smythe, 1973). They don't directly tell you what Saturn's rings are composed of; however, the analysis has

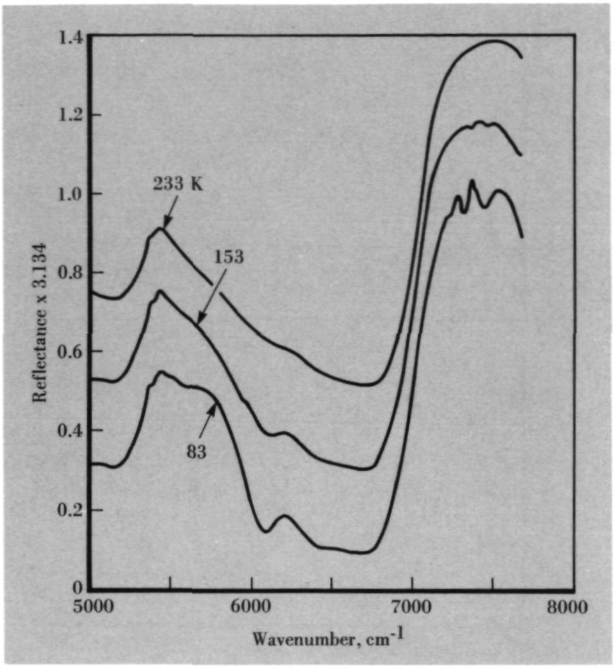

FIGURE 2.—*Temperature dependence of 1.6 μm (6100 cm^{-1}) water frost feature.*

given us some idea of what upper limits can be placed on other components that might be present.

Figure 3 displays an example of the Jovian satellite spectra obtained by Pilcher's group at MIT with an interferometer (Pilcher et al., 1972). The resolution here is about 120 wave numbers. The spectral reflectivity of Saturn's rings is very similar to that of the Galilean satellites, and it is similar enough, I think, to use the analysis we have done on the Galilean satellites to obtain some strong indications about the composition of Saturn's rings.

The basis of this analysis was to digitize the spectral reflectivity of both the Galilean satellites and a series of laboratory samples of pure frosts and then find out how much of each frost spectrum could be present in the astronomic spectrum before the "best fit" is appreciably worsened. We presumed the Galilean satellites spectra were composed of water frost, some amount of a grey background, and some amount of a third material. For the third material, we have tried methane, ammonia hydrosulphate, hydrogen sulphide, ammonia, and carbon dioxide. We also tried all pairs of this triple set.

By doing a study of how good a fit we could obtain with a variety of these components, we have established an upper limit on the composition of other materials. The major components in the best fit to each satellite were water frost and "grey," with the coefficients for the other components essentially zero, or in some cases negative, which is physically not allowable. The best

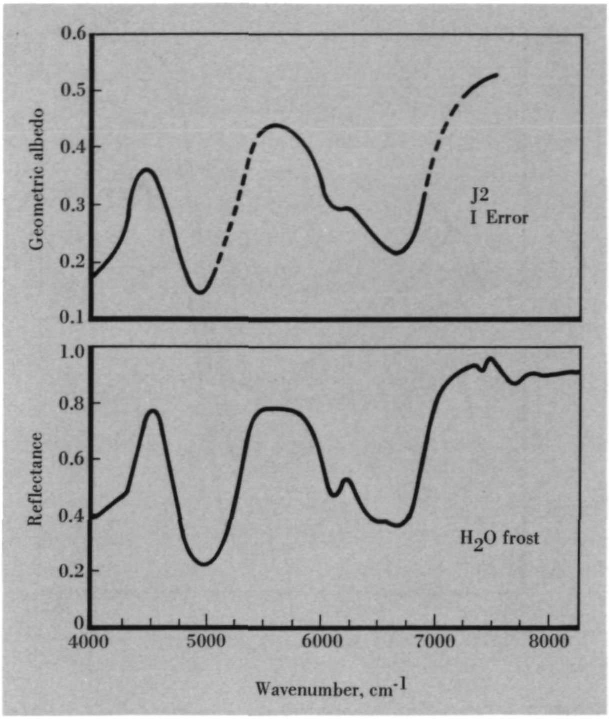

FIGURE 3.—*Reflection spectra of Galilean satellite J2 and water frost.*

fit is near zero, and on that basis I say that the probable value is zero. Their upper limit, based on the residual after the fit gets appreciably worse, is on the order of 15 percent. For the Galilean satellites, the amount of grey varies from 10 percent to, in some cases, quite a large fraction of basically grey material. For instance, in this wavelength region, Io is almost flat spectrally. But I think a grey material is sort of a catchall that probably covers most of the silicates in this region. They are comparatively grey relative to the volatile chemicals. Their spectral features in this wavelength region are generally small, and the only materials that seem to fit very well are things which are extremely hydrated, like Montmorillinite. We have not done an analysis of what silicates might be present. But in terms of what volatiles might be present, I think we can establish fairly strong upper limits with probable values of zero in the sense that for no volatile does any abundance appreciably improve the fit to the astronomic spectrum. In other words, it would be straining the credulity of the signal-to-noise ratio to say that you have a little bit of some volatile here. The major component in all Galilean spectra is water frost or just grey material. Saturn's ring spectrum is very close to the J2 and J3 spectra.

The similarity of the Saturn's ring spectra and the Jovian satellite spectra in this wavelength region is so great that I feel convinced that, to the extent that the signal-to-noise ratios of the astronomic observations were the same, we would come up with a similar upper limit for the composition of the rings, remembering that this refers to that part which is reflected radiation.

James Pollack In deriving these limits, did you make laboratory measurements of mixtures of these materials?

Kieffer No. These are based on laboratory spectra of pure components, and each is treated individually. When you try a least-squares fit with seven components, a lot of the coefficients come up negative.

Pollack In that case, I am very nervous about the way you derived your upper limits. The scattering processes when you have a mixture are not simply going to be the superposition of what happens when you have the two separate ones.

Kieffer That's correct. In the text, which I haven't presented in full, we have some covering comment to the extent that we presume the spectra are additive, which, of course, they would not be if there were an appreciable amount of material other than water. Without going into a detailed discussion, I think the reason this gives a reasonable upper limit is that the materials have similar absorption coefficients, and we are talking about small quantities. Whether they are additive or multiplicative for small amounts, we are talking about on the order of 10 to 15 percent, probably quite a bit less, and I think in this respect treating the spectra as additive is a good approximation.

The basic point is that there is no appreciable indication of a positive contribution to these spectra other than from water frost.

Another interesting thing that comes from the infrared spectral measurements is a minimum value for the particle size. The particles clearly have to be as large as the mean grain diameter suggested by the depth of the absorption features. Based on physically looking at water frost samples and on photographs taken after we measure their reflectance spectra, the textural scale in Saturn's rings—the physical size of the scattering particles—is on the order of 100 μm. You can go a factor of 2 or 3 in either direction from that, depending on the details of the discussion, but that is certainly the textural scale indicated by the depth of the absorption features.

When dealing with particles whose shape may be as complex as a snowflake, the term "particle size" is not very well defined. I use the term "textural scale" to be the volume-to-surface ratio measured with a resolution of 1 wavelength. This is a fairly good measure of the expected path length between surfaces for a random ray and is therefore a measure of the particle size determined from spectroscopy. It has the advantage of being largely independent of how particles are clumped together. For complex shapes such as snowflakes, the physical particle size over which a frost crystal or clump has strength may be one or two orders of magnitude larger than the textural scale. The spectra of large particles may be governed by detailed surface texture. Certainly, in the terrestrial environment and the laboratory environment, a great variety of textural scales is possible and a great variety of complexity of the individual grains is possible. I don't know of any theory or any study that would predict with any confidence whatsoever the type of crystal shape you would expect in the appropriate astronomic environment. It is very difficult to anticipate what textural features might be present under the conditions of low temperature, low pressure, very long time scales, temperature cycling, rotation of the particle, shadowing, and so forth. At the moment, one can only say that the spectral observations indicate a textural scale on the order of 100 μm which therefore establishes a minimum particle size.

One thing I should point out for those of you who have been doing scattering studies: you should certainly not anticipate that the particle size or the scattering objects are anything like power law distributed for frosts. Scattering object sizes are very likely bimodal or trimodal. And of course you would also expect lots of 60° and 90° angles instead of random angles, as are used in most theories. So, to that extent, you must recognize the approximations inherent in a smooth scattering function and a smooth particle size distribution.

Watson, Murray, and Brown (1963) studied the stability of various volatiles in the solar system. They pointed out, among other things, that water has much lower vapor pressure and is considerably more stable than the other geochemically probable volatiles: methane, ammonia, and hydrogen sulphide. I have recently gone through a calculation to see what this implies in terms of the chemistry versus the particle size of the rings and the resulting reflection spectra. Presume that the first 100 μm of material determines the reflection characteristic, because there is no easy way of sampling what's below that. Given an astronomic time period of several billion years, 100 μm of water is quite stable at the temperature of the rings. Its sublimation rate into a vacuum is stable by about 4 to 11 orders of magnitude compared to 100 μm in 3 billion years. The other components mentioned above are unstable by at least 4 orders of magnitude in each case.

That means that even if we started out with an object of mixed chemistry, a lot of methane and ammonia and some other things, and if there were no action to stir this stuff up—if it were just allowed to sublime—after a period of time like billions of years, certainly the top 100 μm should be largely water, regardless of the initial volatile chemistry. Silicates, of course, are quite stable.

I think this is one reason why water spectra are so predominant in the outer solar system. Regardless of the initial volatile chemistry, that is what survives on the surface after a few billion years. This loss of volatiles has very likely also occurred on the Galilean satellites, but they have an appreciable gravitational field and the volatiles will be cycled onto the poles rather than being lost altogether. That is part of a further study.

Let me go on to what I think will serve as an introduction to some of the later talks: the question of thermal measurements and their implications on particle size.

The basis of this study (Aumann and Kieffer, 1973) is simply presuming that the ring particles possess spherical symmetry and then calculating their temperature history as they go in and out of solar eclipse behind Saturn. We then compare these calculations to some reported observations of the brightness temperature of the rings.

The mean surface temperature T of a spherical, homogeneous particle in a ring composed of similar particles is determined by the heat balance equation:

$$\bar{F}+K\frac{dT}{dr}\bigg|_{r=R}=4\epsilon\sigma T^4\ (1-\omega_R/4\pi) \tag{1}$$

We simply assume that there is some incoming flux F into the surface, plus some heat flow from the interior. This is equal to the thermal radiation decreased by a factor that includes the back radiation from the other rings. ω_R is the solid angle of the remainder of the ring as seen from an average particle. The total amount of energy coming in is the direct insolation, the sunlight reflected from Saturn, thermal radiation from Saturn, sunlight reflected from the other ring particles, and thermal radiation from the other particles.

We applied this equation to particles for a variety of radii and thermal inertias for a typical mid-B ring particle. In other words, the length of the eclipse is that which would be appropriate for the middle of the B ring. We assumed that the direct insolation and the reflected sunlight from Saturn and from the rings diminished with a $1/e$ time of 50 s as the particle went into the eclipse. This is much shorter than the eclipse period, so that penumbral details are not important. The thermal radiation from Saturn remained constant.

Figure 4 shows typical cooling and heating curves for such particles. Large particles don't undergo a measurable temperature change, while very small particles cool quickly and then return quickly. The point of interest here is that for Earth-based observations you are obliged to look at a time period somewhat following the eclipse. The most diagnostic observations would clearly be within the eclipse, and those are possible only from a spacecraft and not from Earth.

Figure 5 shows the temperature as a function of particle radius at a time 1000 s after the exit from eclipse. What is of interest here is the limited effect

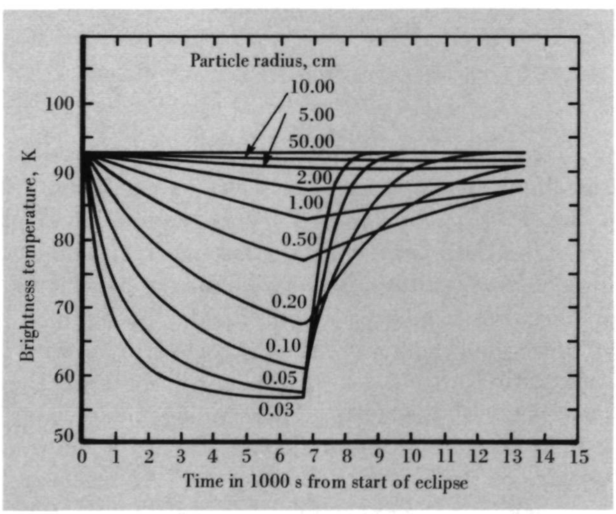

FIGURE 4.—*Brightness temperature of solid ice-like mid-B ring particles as a function of particle radius and time from entering Saturn's shadow.*

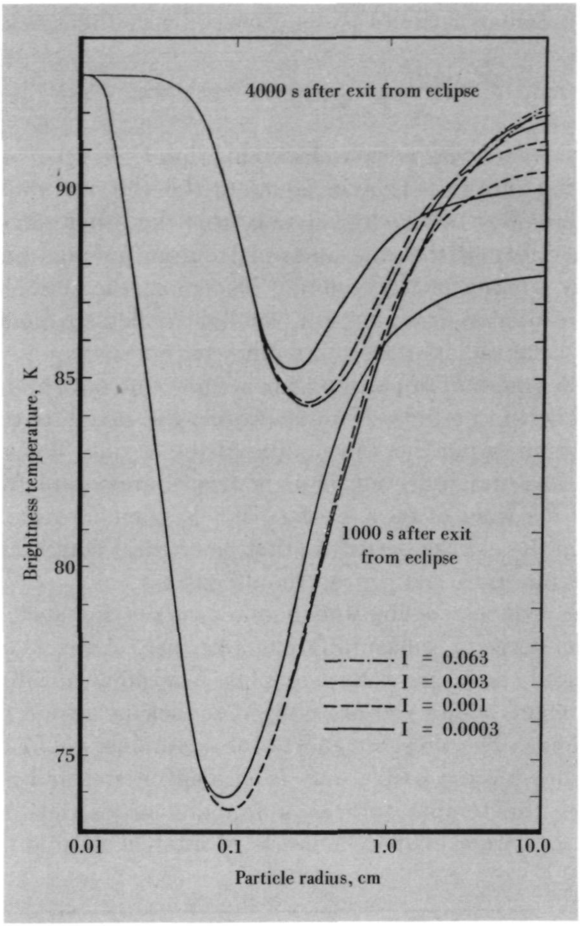

FIGURE 5.—*Particle temperature 4000 s and 1000 s after exit from eclipse.*

of particle inertia over its entire probable range; that is, from that of dense water at 100 K, which is about the same value as for silicates, down to the lowest thermal inertia that has ever been reported for an astronomical object, the very uppermost surface of the Galilean satellites (Hansen, 1972). The whole range yields very little variation in temperature. The predominant parameter is the particle radius. This was somewhat of a surprise, but it is due basically to the fact that we are looking at the time when the temperature is recovering toward its initial value.

To compare this with Earth-based observations, we took these results and simulated the observational geometry. That is, we modeled an aperture size similar to that used by the Hawaii group, looked at part of the ring that was 5 arc seconds from the edge of the shadow, and averaged the brightness temperatures appropriately. Figure 6 shows the expected brightness temperature for this portion of the ring as a function of particle radius, again for a very wide range of thermal inertias. The two dashed lines mark the range initially reported by the Hawaii

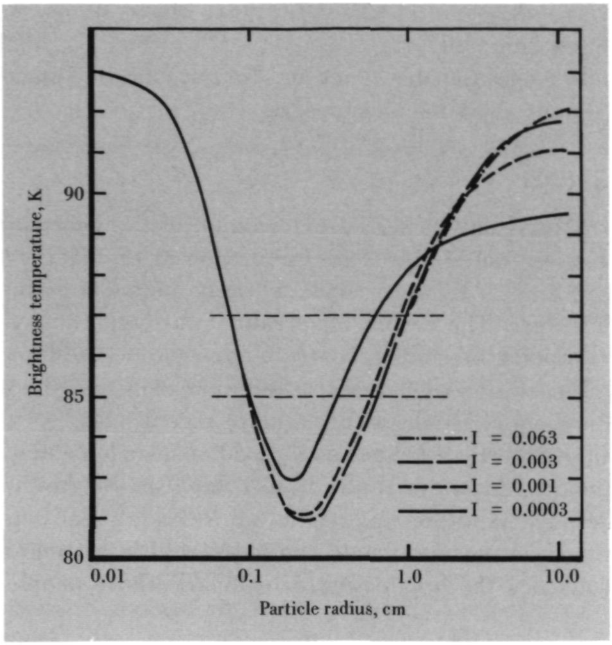

FIGURE 6.—*Apparent brightness temperatures of mid-B ring particles as a function of size and thermal inertia as seen with a 5 arc sec diameter telescope beam, 5 arc sec after exit from eclipse for a 25° tilt of the ring plane relative to the Sun and a 3° phase angle.*

group (Murphy et al., 1972) for the temperature difference between the ring entering the shadow and at a point soon after exit.

This temperature range therefore implies a particle radius on the order of a millimeter to a centimeter, regardless of inertia.

Surprisingly, even inhomogeneous particles—such as the interesting case of a dense core of high conductivity covered with a thin surface layer of low conductivity—give basically the same result. The reason is that if the surface layer has very low conductivity then the core has very little effect on the temperature. If the surface layer is very thin, then it is dominated by the effect of the thermal inertia of the core. This is a little hard to see intuitively; however, in doing a numerical study, we found that the inhomogeneous cases are basically bounded by the two limiting inertias.

I would like to conclude at this point, except I must mention that we have since been made aware that the Hawaii group has looked at their observational evidence again, and I believe the estimate for the temperature drop between entrance and exit is now 1.5±0.5 K. Once the temperature difference is less than 3°, the resolution of this method decreases because one enters a region where inertia again becomes important. With a 1.5±0.5 K estimated and a 3σ limit of 3°, one is just at the point where the bulk particle size can no longer be bounded by thermal observations. The size range indicated is greater than about 1 cm and is compatible with at least some of the radio/radar observations, which are themselves not completely compatible.

Perhaps I should leave it at this point and let the following speakers give some more conflicting details about the particle size.

DISCUSSION

David Morrison May I say in partial explanation of the contradiction in the IR results that this was not simply a reexamination of the data. The observations that give the 1.5 ± 0.5 K were made entirely independently by a different method this last year. The earlier observation was something that we unfortunately should not have presented. It was a one-sentence addition to an abstract at a meeting. Please don't leave with the impression that the earlier observations should have equal weight with the more recent ones.

Hugh Kieffer In our work on eclipse cooling curves, we have made an attempt to separate the method from the application. I think the method is fairly straightforward. It does give an interesting result that we hadn't really anticipated ahead of time—that the exit measurements can be related to particle size. This whole group should discuss the application, particularly those people who made the observations.

Von Eshleman The 1.5° difference applies to what time after exit?

Kieffer It's about 2000 s.

George Aumann As I now understand it, the 1.5° difference refers to different observations. In that case, because of the varying tilt of the ring plane, it will be a somewhat different time than for our calculations. You have to be very careful because you are looking at particles which on one end of the projected eclipse are close to the shadow, and then you come out on the other side away from the shadow, so you are averaging over roughly 1500 s to about 2500 s after eclipse. You have to really do an honest average over the beam, including the fact that there is a radial separation inside, so the right-hand beam looks closer to Saturn than the left-hand beam. So roughly that is the number, a smearing of quite a large time, unfortunately.

William Irvine Would Dave (Morrison) comment on the observational differences between the east and west ring ansa?

Morrison I am not sure it bears any relationship to the data we are talking about. There is an asymmetry in the infrared brightness of the east and west ansa of about 8 percent in our 20 μm measurements and 10 percent as reported by Allen and Murdock (1971). I would like to seek an explanation of that difference in terms of either the orientation or the albedo of the ring particles.

At the time after eclipse involved, when you are clear out on the ansae, I doubt if there is any residual effect from the thermal shock of the eclipse itself. The ansae difference of 8 ± 4 percent in flux is about 1° in temperature. I am not sure the effect is real, but it was mentioned that there has been an asymmetry reported in the photometric brightness of the rings, and it would be nice to seek a common explanation for that and the infrared asymmetry.

Irvine Does this appear to be a continuous change with position around the ring?

Morrison We made no attempt to measure it that way.

Irvine It would be interesting to see how that asymmetry might vary with position in orbit.

Lonnie Lane According to the diagram in which you display a range of particle sizes and the temperature variation relating to heating and cooling (see fig. 4), it appears that if the particles have fairly good size there really is no such thing as thermal shock to them, in the sense of something that could be disruptive. If particles are very small, you do expect a large temperature change. Is that possibly a mechanism for either driving things to accretion or to small size over a billion years or more?

Kieffer I would hazard the response that if we are talking about fairly small temperature changes, I would not anticipate thermal shock to be important for these particles. For particles that are a centimeter or larger, we are talking about fairly small temperature changes, 30° or so, and I wouldn't imagine that that would disrupt them. If the particles are really large, the temperature changes become even less. It might bear some further thought, but my initial response is that it is probably not important.

Eshleman I have some difficulty understanding the last model relative to the earlier portion of your presentation about the crystalline structure or the feathery-type structure. These are quite different subjects you are speaking to?

Kieffer Yes, they are very different subjects and completely different observations. The first is reflectance of infrared radiation and the second is emission of infrared radiation.

Eshleman If you could get a scale from the temperature change during eclipse, would this be of the small-scale surface structure? Did you incorporate your textural scale argument?

Kieffer I have not used a textural scale in the thermal calculations but the thermal inertia that would be appropriate to a fine-textured frost. This is the lowest inertia that we utilized in the thermal model, 3×10^{-4} cal cm^{-2} K^{-1} s$^{1/2}$. An interesting possibility would be to have a fairly large particle composed of such low inertia material. The strength of such material is certainly so low that you wouldn't expect any large particles to last very long. If they got clobbered once by a more solid particle, they would surely come apart because they are just very fine-grained snowballs.

However, one of the strengths of the thermal method, which was initially a surprise to us, is that even composite particles—where a center core of high conductivity is covered with a thin layer of low conductivity material as indicated by spectral reflectance data—follow the basic exit temperature dependence on radius found for homogeneous particles. The method applies to homogeneous particles and is basically equally applicable to the inhomogeneous cases for the warming part of the eclipse.

Inhomogeneous particles have very different temperature-history curves from homogeneous particles during eclipse, but to distinguish inhomogeneous from homogeneous particles during eclipse would require considerable temperature resolution. This might be a proper objective of a spacecraft observation, but it is completely impossible from Earth-based or Earth-orbiting observations.

Irvine Do you have a feeling for what the volume density would be for a frost which has that lower value of the thermal inertia? Can you make something in the lab which is that low?

Kieffer We have not measured the thermal inertia of frosts in the laboratory. Thermal inertia for such material is largely a function of the size of the point contacts. There are no measurements, to my knowledge, of frost of this type. I think some have been made for natural terrestrial snow deposits but it is so highly dependent on just how the grains connect to each other that I would not want to hazard any guess of what would happen in the Saturnian environment. With time, for the same volume density, the conductivity will tend to increase because the favored sites for molecular condensation are the little corners in between two things that meet; that tends to weld them together and increase conductivity.

I think that perhaps the strongest support of such a low inertia is the Galilean satellite spectral reflectivities, which indicate water frost of this fine nature, and the eclipse observations, which indicate extremely low inertia for the uppermost few millimeters of the Galilean satellites.

Eshleman Did I understand figure 6—with the change to 1.5 K you conclude that the particle size is greater than 1 cm?

Kieffer Yes, for 1.5 ± 0.5 K the particle size would become greater than or on the order of 1 cm and is compatible with at least some of the radio/radar observations, which are themselves not completely compatible.

Morrison In the paper by Morrison and Cruikshank (1973), there are a couple of references to some studies in the literature of the thermal conductivity of very low-density frost; they would tend to be observations made at one atmosphere of pressure. There is the difficulty of scaling to conditions at Saturn, but there are observations of very low-density frost thermal conductivities.

Kieffer The problem is that they are, in all cases, basically terrestrial environment frosts, and they are subject to this problem of metamorphism. Studies that have been made in materials like antarctic snowpacks show that you can get, in the space of a meter, a very wide range in the crystal form, the conductivity, and all the other properties of interest to us, except the chemistry.

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