N75 17246

James B. Pollack

ESTIMATES OF THE SIZE OF PARTICLES IN SATURN'S RINGS AND THEIR COSMOGONIC IMPLICATIONS

I would like to start by discussing the near-infrared ice band measurements Hugh Kieffer mentioned, then to proceed to the radar and radio observations, and finally to consider some possible implications of the size estimates, relating them to the Poynting-Robertson effect and also proposing a possible mechanism by which the size of the particles may significantly change after the initial formation of the material in the rings.

As I am sure you are all aware, Kuiper's observation showed quite clearly the ice bands that Hugh Kieffer mentioned yesterday. The first calculation that we did was to try to see how large the particles would have to be in order to produce absorption bands of this depth. A very clear distinction has to be made here on what we mean by particle size. Given the fact that Goldstein's radar results (see contribution by Morris for a discussion of the radar detection of Saturn's rings) show that the ring particles cannot be much smaller than the wavelength of observation, we have to be very careful in how we interpret the size inferred from the ice band data. In other words, it is not necessarily the size of the particles themselves, but, as you will see, it is the size of the microstructure on the surface of the particles.

In figure 1 Kuiper's observations are shown by the triangles. I particularly want you to concentrate on the very strong 2 μm absorption feature and on the very strong absorption feature at 1.5 μm. The curves themselves are the theoretical calculations obtained using a simple multiple scattering theory. The depth of these bands is a strong function of the mean particle size $\langle a \rangle$. Both the 2 μm band and the 1.5 μm band can consistently be fit with a mean particle radius on the order of about 25 μm or a mean diameter on the order of 50 μm. This number corresponds very nicely with the 100 μm number Hugh Kieffer mentioned yesterday.

For particles of this size, the optical depth at a wavelength of 12.6 cm would be extremely small, and there would essentially be no radar backscattering, contrary to Goldstein's observations. Given that fact, we can't interpret this particle size

---

Ames Research Center.

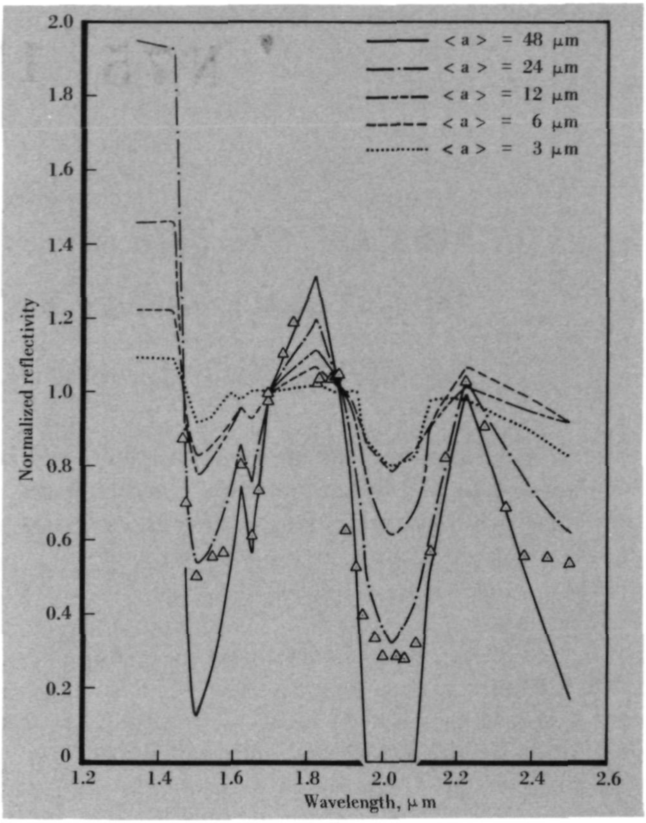

FIGURE 1.—*Spectral reflectivity of Saturn's rings. Triangles indicate observations by Kuiper et al. (1970). Theoretical curves for ice are labelled by the value of mean particle radius. Both theoretical and observed values have been normalized to unity at 1.7 μm and the calculations have used a value of 2 for the visible optical depth of the rings.*

as being the size of the gross particles themselves. This means that, as viewed at near-infrared wavelengths, the surface of the individual particles exhibits a microstructure. A particle's surface is not simply a nice, smooth, solid surface; it has texture and it is the size of the textural elements we have estimated from fitting the absorption bands.

Let's next move on to the radar and radio observations. The fact that a very significant reflectivity return was obtained from the rings at 12.6 cm immediately means that the optical depth of the rings cannot be very small at that wavelength.

Figure 2 is a plot of particle extinction efficiency as a function of the parameter x. x is the standard Mie scattering term, i.e., the ratio of the circumference of the particle to the wavelength ($2\pi a/\lambda$). By efficiency factor, I mean that if I take an ensemble of particles, a typical particle will have a cross section equal to its geometric cross section times the efficiency factor. As you can see, if the value of x is much greater than 1 (that is, the particles are much larger than the wavelength of observation), the efficiency factor asymptotically approaches a value of about 2. What is happening in this case is that there is an equal contribution from both diffraction and the other ordinary types of scattering and absorption. You also see that once we get to an x value of about 1, the efficiency factor and therefore the optical depth fall extremely rapidly with decreasing x and that behavior is almost independent of the material's makeup.

We know that the optical depth of the rings at visual wavelengths, where x is of course much greater than 1, is on the order of unity for ring B and maybe somewhat smaller for ring A. If we are going to get any radar return at all, this means that x has to be comparable to or greater than 1 at the wavelength of observation, 12.6 cm. In particular, this means the mean particle radius has to be comparable to or greater than 2 cm.

As you heard in George Morris' talk, one way of interpreting the high radar reflectivity is to invoke a gain-type effect, that is, the particles preferentially scatter in the backward direction, and you can gain, in his discussion, as much as a factor of 8/3 in terms of backscattering. Gordon Pettengill has pointed out that you might gain even more if you consider semitransparent particles (see discussion accompanying contribution by Morris).

Now, suppose you wish to invoke the gain effect; this effect arises when the particles' surfaces have a range of large tilt angles. Then, in order for the tilt angles

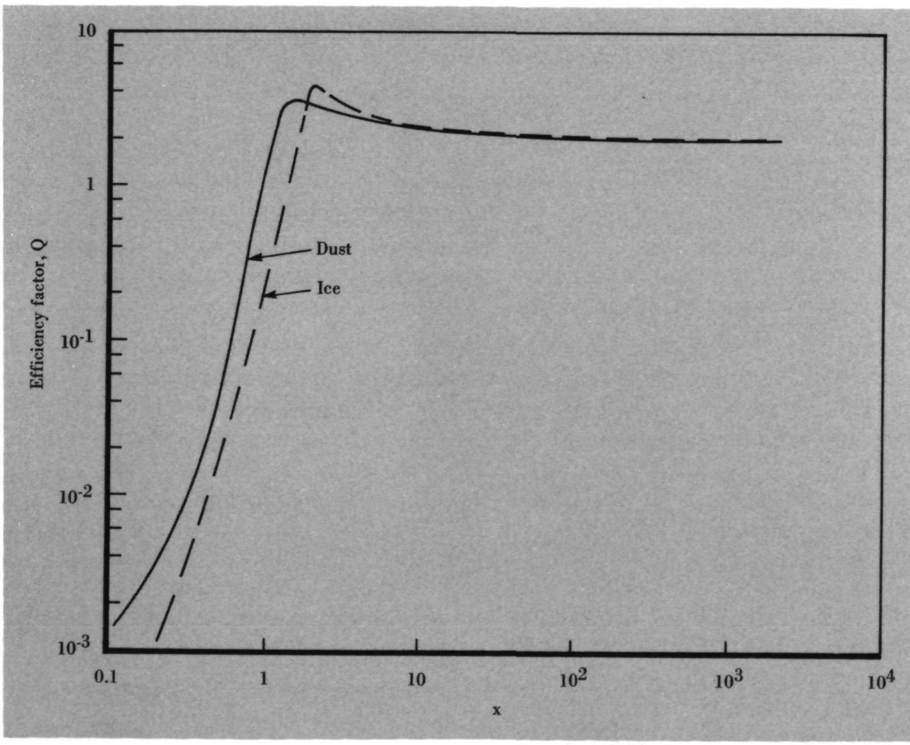

FIGURE 2.—*Extinction efficiency Q as a function of* x *for pure ice and silicate particles at radio wavelengths.* x *is the ratio of particle circumference to wavelength.*

to show up, these facets have to be on the order of a wavelength. Thus, according to the gain idea as advanced by Morris and Goldstein, one requires particles bigger than a meter.

An alternative possibility is to consider multiple scattering as a way of enhancing the reflectivity. The point is that if you consider higher-order scattering, the intensity will increase in any direction and will lead to values that are not inconsistent with the radar result. For this to be the case, we must have a reasonably high single scattering albedo at microwave wavelengths. That statement implies a restrictive range of particle sizes.

Passive radio observations have enabled us to make some fairly definitive statements on particle size as well as to interpret the radar observations. They have the general characteristic of very low upper limits to the brightness temperature throughout most of wavelength space, from several millimeters all the way to 21 cm.

If we were speaking in terms of very large particles, we would not expect this sort of behavior, because if the particles were large enough they would be completely absorbing. Furthermore, if we write the emissivity as 1 minus the reflectivity, the reflectivity that we mean is one in which we effectively average over all angles of incidence. In other words, it is not something that is equivalent to the radar reflectivity, and in particular no backscattering factor enters. If one is restricted to a particle that is very large and purely absorbing, the only way one can get reflection is off its external surface. In that case for most plausible materials, according to the Fresnel reflection laws, the reflectivities will not be comparable to unity, and therefore the emissivity should be close to 1, although not exactly 1.

William Irvine That statement is true for homogeneous particles, isn't it?

Pollack That's correct. But I think if you took a model that had a core-mantle type of construction, my remarks would be equally applicable unless you had very high indices of refraction for one of the materials. In other words, it would just be a matter of applying Fresnel's law twice instead of once.

Irvine I am thinking of the analogy of a snowball at optical wavelengths, where you have lots of reflecting facets which can increase the overall albedo. I don't know whether you can do that at centimeter wavelengths.

Pollack I think you might have problems with a model like that in two senses. One is that low emissivity seems to be a phenomenon at quite a number of different wavelengths. It is unlikely that you are going to have facets of such a size that they are going to do a lot of internal reflection at all these different wavelengths. The other point is that most materials do not have large numbers of textural elements whose size is on the order of a centimeter or larger.

Let us go back now to the bright cloud model that I spoke about as one of the models to explain the radar results. The point there was that you have a high single scattering albedo, and as a result you have a lot of multiple scattering and therefore a high reflectivity. So let's see what a model like this would predict for the emissivity of the rings at radio wavelengths.

Figure 3 shows a plot of a scaled single scattering albedo. In effect what I have done here is take the anisotropy factor out and express things in terms of an equivalent isotropic single scattering albedo. It is plotted as a function of x for two different compositions. The two curves differ because of the imaginary part of the index of refraction. Once the diameter of the particle is such that an internal ray has a reasonable probability of being absorbed, the single scattering albedo will start to drop. In the case of typical silicate particles, they have larger values for the imaginary part of the index of refraction than ice, and they start to fall at smaller values of x than in the case of ice.

You will notice that there is a certain intermediate range of x values—that is, an intermediate range of particle sizes comparable to the wavelength such that the single scattering albedo is high. The very large particles will start to fall for the reason I mentioned, and the albedo will asymptotically approach the Fresnel reflection law value. At very small values of x, absorption becomes much more efficient than scattering; hence, as you go to very small x, almost any particle becomes a perfect absorber.

To relate the scattering results to the radio brightness temperatures, I have taken a very simple scattering model. First of all, I assume that the rings are optically thick enough so that they can be considered to be infinitely thick. I will come back and comment on what influence that assumption has on the numbers I am going to give to you. Furthermore, I have reduced the problem down to an equivalent isotropic problem.

With these assumptions, remembering again that emissivity can be calculated from the appropriate average reflectivity, one can mathematically express emissivity in terms of the single scattering albedo and Chandrasekhar's H functions. The H functions in effect bring the dependence of the angle of view into the equations. You can then say, given a certain limit or given a certain value for the brightness temperature at a particular wavelength, this implies a certain range of single scattering albedo. You can then go to figure 3 and see what value of x this corresponds to. Furthermore, if there exists a set of observations at enough

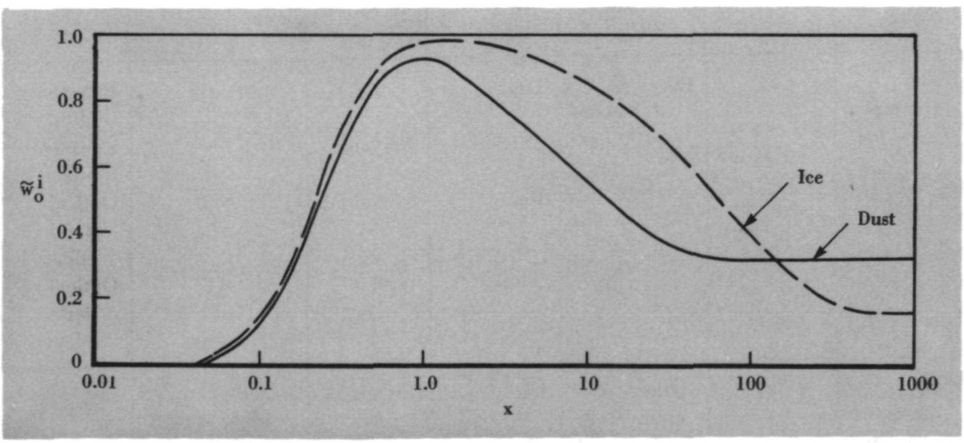

FIGURE 3.—*Isotropic single scattering albedo* $\tilde{w}_0^i$ *as a function of* x *at radio wavelengths for pure ice and silicate particles.*

different wavelengths, it is possible to obtain a good determination of the mean particle size.

Table I summarizes the particle sizes implied by the various radio observations, as well as repeating the results of the radar observations. First of all, let me again remind you of the radar result; this result is simply based on the fact that the optical depth cannot be too small. We obtained a lower limit on the order of 2 cm for the mean particle size. You will recall that yesterday Berge (see contribution by Berge) presented the very nice interferometer results that he and Muhleman got at 21 cm. These results set a very severe upper limit to the mean brightness temperature of the rings that was approximately a factor of 10 below the brightness temperature that's measured at infrared wavelengths. This enormous discrepancy is very hard to explain by simply juggling certain factors.

Consider the bright cloud model I spoke about earlier. Remembering that the 21-cm observations are slightly longward of the 12-cm radar observations, there are two ways in which the low brightness temperature at this wavelength can be interpreted: you can invoke either an optical depth effect or an emissivity effect.

If it is an optical depth effect, then you get for an upper bound something very close to the lower bound you got for the radar result. If you say that it is an emissivity effect, then there is no place on the curves for the silicates where you can get a high enough single scattering albedo. In the case of ice particles, it is only those values very close to the peak on the curve that give you a consistent result, and you get a mean particle size on the order of 2.5 cm.

TABLE I.—*Size estimates of the ring particles.*

Type of observation	Wavelength, cm	Observer	Assumed composition	x	$\langle a \rangle$, cm	Comments
Radar back-scattering	13	Goldstein & Morris (1973)	Ice	>1	≥ 2	
			Silicate	>1	≥ 2	
Radio thermal emission	21	Berge & Muhleman (1973)	Ice	~ 1.5	~ 1.5	Emissivity effect
			Silicate	No value	—	Emissivity effect
			Ice	≤ 0.7	≤ 2	Optical depth effect
			Silicate	≤ 0.7	≤ 2	Optical depth effect
Radio thermal emission	0.1	Rather et al. (1973)	Ice	$3 \leq x \leq 15$	$0.005 \leq \langle a \rangle \leq 0.25$	
			Silicate	$1 \leq x \leq 3$	$0.002 \leq \langle a \rangle \leq 0.005$	

In addition, let me now go to a much shorter wavelength. In the discussion of the short wavelength observations (see contribution by Janssen), there was an observation near 1 mm by John Rather at Kitt Peak. The important thing about his observation, remembering that observations at this wavelength are very difficult to calibrate absolutely, is that he observed both Jupiter and Saturn. The important point is that Saturn had a significantly higher brightness temperature whereas, on the basis of most atmosphere models, as well as on the basis of results at longer wavelengths, you would expect the brightness temperatures to be approximately equal. If one interprets the excess as due to thermal emission from the rings, this would say that the brightness temperature of the rings at 1 mm was not up to the infrared temperature by any stretch of the imagination. It would be something like a factor of 2 to 4 below the brightness temperature measured at infrared wavelengths. If one makes use of the emissivity formula for isotropic scattering, this result implies a certain range of single scattering albedo. The plot of single scattering albedo versus x then implies a certain range of particle sizes. In the case of the silicates, this millimeter observation would imply extremely small particles, whereas in the case of the ice particles you could have particles up to a quarter of a centimeter—not too different from our inferences on particle size made from the longer wavelength radio and radar observations.

I think you can get a feeling from everything I've said that, very crudely speaking, particles that are on the order of a centimeter would fit all the observations. In going through table I, we had a very hard time obtaining consistent results between the different wavelengths with silicates. In the case of ice, we still didn't get complete consistency, but the discrepancy was not nearly as large as for the silicate particles.

If one goes through more detailed calculations, as I am presently planning to do, I think one can in fact reconcile all the different wavelengths of observation. There are two principal things that I have not done in the present calculations. First, I have assumed that the rings are very optically thick. Now, we know that the optical thickness for ring B is on the order of about unity at visual wavelengths and about one-half for ring A. The effect of allowing for finite optical thickness is to make the required single scattering albedo lower than in the infinite case. This would lead to bigger particles for the millimeter observations of Rather. Second, the values used for the indices of refraction at microwave wavelengths must be revised. Simply because of the paucity of data in the literature, I used values of the measured index of refraction at room temperature for both the silicates and ice particles.

In the case of silicate particles, there are some measurements which show that as you go to lower temperature the imaginary part of the index of refraction drops, and evidently there are some similar data for ice. This would have the effect of permitting larger particles to be consistent with the 1-mm observations of Rather. I think the combination of these two factors will yield a range of particle sizes for the 1-mm observations that will be consistent with the range of sizes deduced from the longer wavelength observations. On the other hand, I don't think the correction factors are going to be so large that it is going to be possible to speak of particles much larger than a few centimeters. My feeling at the present time is that when you take both the radar and radio observations together, you are probably speaking of particle sizes on the order of a few centimeters.

One rather intriguing consequence of this analysis is the fact that these sizes are close to the size for which the Poynting-Robertson effect would tend to eliminate the particles in the rings of Saturn over the 4.5-billion-year age of the solar system. As you remember in the case of the Poynting-Robertson effect, what is happening is that you have solar energy incident on the particle, being absorbed, and then being reradiated. Because the particle is traveling in an orbit, the result of the doppler shift of the emissions leads to a net drag on the particle, which causes its orbital radius to decay. Furthermore, the magnitude of the rate of decrease depends on the reciprocal of particle size, so that very small particles tend to be eliminated rather quickly from the Saturn ring system.

Suppose one asked the question, "For what particle radius would I have a situation where a particle started from the outer edge of ring A at the beginning of the solar system and 4.5 billion years later (now) ended up at the position of the inner part of ring B?" The answer would be somewhat larger than a few centimeters in size. The exact value is difficult in the sense that you must consider corrections due to scattering and corrections due to finite optical depth, but it's on the order of a few centimeters.

One intriguing possibility that comes out of the size estimates above is that, as a result of the Poynting-Robertson effect, the rings may have been considerably broadened in their extent. That is, at early epochs of the solar system, the rings might have been a lot narrower than they are at the present time, and then, because of the Poynting-Robertson effect, they were considerably broadened.

Another theoretical consideration related to particle size has to do with the origin of the ring particles. One needs to factor Roche considerations of the tidal instability of particles into any consideration of the origin of the ring particles. The question here is whether anything of interest happens after the initial formation of the ring particles. As a result of meteoroid bombardment, there may in effect be a significant reduction in the mean particle size from what you start out with originally. The idea is very simple. By mean particle size we mean some sort of weighting by the cross section area of the particles. If we just peel off a very thin layer from the big particles by meteoroid bombardment, and if this layer contains many smaller particles, we have a situation where the ejected fragments would have a comparable total area to that of the parent particle. If you peel off more than a single thin layer, then you have a situation where the ejecta have a larger cross section area, and it is not too difficult to convince yourself that there are many situations under which the cross section area of the ejecta can be enormously larger than that of the parent particles, even though the majority of the mass would still reside in the parent particles.

In fact this would partially answer Fred Franklin's question. We were having difficulty yesterday—with the particle sizes I am speaking about—in trying to get Fred enough mass so that Cassini's division is in the right place. In these terms we really have two populations: one population that determines the mean size when we are speaking about things like radar, radio, and optical observations, essentially a cross section average; and a second population when we are speaking about adding up all the contributions to get the mass.

I might also add that when a meteoroid impacts a small body, the ejecta in general have a velocity significantly larger than the escape velocity because the parent particle is relatively small. On the other hand, the ejection velocities will be significantly less than those needed to escape from the Saturn system. So the particles will go into orbit about Saturn. Now, if nothing else happened in the meantime, they would be recaptured eventually as their orbit crossed that of the parent particle. However, there are a number of factors that may cause a particle not to be recaptured. One of them is the Poynting-Robertson effect, which can decrease the orbital radius so it doesn't intersect the parent particle. Second, if there is enough meteoroid bombardment—and it doesn't have to be a fantastic amount—you build up enough density of ejecta initially that the ejecta have a greater probability of colliding with themselves than they do with the parent body.

You could go through second and third generations of this sort of process where the ejecta themselves are subjected to bombardment. But if the second or third generations are very small particles, the Poynting-Robertson effect will tend to sweep them out very quickly. So with this sort of model, it is possible that if you start with a few relatively large particles, you may well end up with particles whose size is comparable to the critical Poynting-Robertson size in the sense that much smaller particles have been eliminated by the Poynting-Robertson effect and much bigger ones have been ground up.

Let me conclude by speaking about some observations that could help give us more information and tests to see if some of these ideas really make sense.

We had mentioned the possibility of looking at radio sources that are occulted by the rings, making possible a direct transmission measurement. This is a very attractive type of observation because the only thing that is being measured is the optical depth. Furthermore, the optical depth has a very simple type of dependence on the mean particle size. In particular, when x is greater than 1 it has a large constant value, and when x is smaller than 1 it starts to drop radically. Therefore, we have a very direct way of getting at the particle size.

Yesterday we discussed the possibility of doing such an observation with the extremely nice radio telescope at Westerbork; as I understand it, this telescope is operating at 6 and 21 cm. Observation at 6 cm would afford us the opportunity to map the optical depth of the rings at radio wavelengths. If you make measurements at wavelengths longer than 12 cm, such as a 21-cm wavelength, you supplement the radar observation in terms of particle size information. You would probably also want to go up into the meter range (say, for example, 70 cm) and even longward of that, if possible. You should start reaching a point where the optical depth of the rings becomes very small very quickly, and the rings become transparent. This is a direct way of getting some further information to make sure my interpretation really is correct.

DISCUSSION

Fred Franklin What a nice paper this is; seeing everything brought together is particularly nice. The first thing I want to mention is the question of the meteoroidal bombardment. It is something that Allen Cook and I (Cook and Franklin, 1970) worried about for a while, too. We also did the hypervelocity impact calculations, and our conclusion was like yours: after an impact the material is sprayed out into orbit about Saturn primarily, you do lose some into the disk, and perhaps some goes into a hyperbolic orbit. But much of it is in fact returned to the ring. Since the optical thickness of the ring is of the order of unity, it would seem very clear that you certainly return the bulk of the material quite promptly to the ring. If you tried to take the best numbers, it was not clear whether the rings were gaining mass or losing mass. You might, in fact, slowly accrete material by this means, or you might actually lose it. Any change had to be terribly slow, and it would seem to me quite conceivable that you might remove material from small particles, but it probably would be redeposited somewhere else. I think we can also show that secondary impacts are not so important. So, essentially, you peel material off of one particle but redeposit it on another one. I would emphasize that things do happen slowly, and the Poynting-Robertson effect would not materially change the orbit of a particle before it is recaptured because of the high collisional thickness of the ring.

William Irvine If such secondary material collides with particles in the ring, does it stick?

Franklin Sure, why not? I won't say that it necessarily does, but you can't keep building up a halo forever.

James Pollack What would actually happen is that each time it gets back to the parent body, you would have a somewhat inelastic collision in which the relative velocity is reduced by maybe a factor of one-half or so. If you go through this several times, you will finally get back to a point where the relative velocity has become less than the escape velocity, and the particle will be recaptured. My point concerning meteoroid bombardment was not so much that the mass would change as that the cross section would become dominated by the smaller ejecta particles. Also, one should not necessarily confuse the present optical depth with the rings' past value.

Franklin Sticking probabilities for dirty ice are not terribly low.

Robert Soberman If you are talking about a considerable amount of material and you don't allow for the sticking, then the Poynting-Robertson effect should smear out features like the Cassini division and fill the region closer to the planet than ring C. These particles would be smaller and therefore better visual scatterers. You should see a lot more light in close to the planet as the Poynting-Robertson effect brings it in. So either the material is sticking back in the same regions from which it came, or there is very little material being ejected in the first place.

Franklin It might be nice to work out a theory of the brightness distribution of ring C based on a model of this sort, whereby you do feed in material by one mechanism or another.

Pollack I thing you have to be a little careful in the sense that the transit time through the Cassini division might be rapid because of the satellite perturbations.

Soberman What of the region in closer than ring C? You would have a buildup by the Poynting-Robertson effect in this region.

Pollack I think one really has to consider satellite perturbations. That is, if we do not put satellite perturbations in, what you say is true. But it might be that the transit time, once material gets beyond ring C, is very small.

Soberman I was thinking of the zodiacal light. As dust particles spiral into the Sun, you have a buildup in concentration closer to the Sun.

Pollack Yes, but there you don't have the significant perturbations found in the case of Saturn's inner satellites.

Brad Smith In any case, the brightness distribution in ring C goes the wrong way. The brightness falls off almost linearly from the inner edge of ring B.

Pollack Right. You have to work out the effect of the satellite perturbations to see whether that observation would be consistent or inconsistent with meteoroid bombardment and the Poynting-Robertson effect.

Franklin I would like to emphasize your remark, Jim (Pollack), that perturbation resonances are particularly important when you come to discussing Poynting-Robertson lifetime. If you do have a particle in ring B that tries, because of its Poynting-Robertson effect, to move closer to the planet, it inevitably encounters the one-third resonance. This is a higher order of resonance than the one-half resonance of the Cassini division, but it still would induce the oscillation in a and e that I mentioned yesterday (see contribution by Franklin). In this case the oscillation in and out would not be of the order of a few hundred km but would still be of the order of 10 or 20 km. If you try to feed particles in, they encounter the resonance, start to librate, and will essentially be returned to the ring. If the density of the ring material is sufficiently high, it seems to me you can make a strong case that the particle will collide with main ring particles and simply be returned to the ring.

I would like to make the point that resonances, when you also have a situation of a large amount of material on each side, are difficult for microscopic particles to cross. I would like to speculate a bit further that this may in some sense provide an explanation for the very existence of Saturn's rings. Saturn has an inner satellite, Mimas, which has important resonances within the Roche limit. Uncondensed material there is essentially boxed in or held in this region, first by the presence of a sufficient amount of absorbing material and second by resonant barriers on either side.

If you look at the case of Uranus, this situation does not occur. The innermost satellite of Uranus has no resonances within the Roche region. Even one-third the period of the innermost satellite is well outside the Roche limit. In the Uranus case, you have no natural barriers, and a hypothetical ring might, in time, be dissipated by the Poynting-Robertson effect or other dissipating mechanisms.

These resonances are particularly important in the presence of a sufficient amount of absorbing material near the border and may divide and maintain the rings over long periods of time.

Pollack That is a fascinating remark. What I was starting with in terms of meteoroid evolution was an extreme situation in which you might have had just a few parent bodies to begin with. Under that sort of situation, the probability of recapture may be very small. The second remark that I would like to make, which is a key feature of this type of meteoroid model, is that this gives you a way of generating an enormous number of particles such that they will have fairly frequent collisions and the whole system will collapse down to a thin structure.

The final point I would like to make concerns the location of the Cassini division. As you remember in Fred's talk yesterday, he said that he would like to have a certain amount of mass in order to move the Cassini division to the right location. As I understood it, he wants to look at this number in a bit more detail. But if you take the mean particle size I have estimated, the optical depth of the rings, and their known radial extent, it is possible to derive how much mass you have in a very simple calculation. You evidently fall a couple of orders of magnitude below what Fred would like to have. However, if this meteoroid hypothesis makes sense, there might be a few bodies of much larger mass than you get by adding up all the mass in the smaller particles. That could well get Fred enough mass. Or, if you accept Fred's mass, and if you accept my mean particle radius, you may almost be driven to the conclusion that much of the mass is tied up in a few much larger objects.

Hugh Kieffer One independent piece of evidence that relates directly to this question is the fact that the Martian satellites have very low inertia surfaces and they are covered with some type of dust-like debris. Their gravitational field is so small that if they were not recapturing their own particles or capturing some particles, you would expect them to be cleaned by meteoroid bombardment and have a solid surface. At least in one case, there are satellites that are recovering—or have acquired from some other source—additional material. The net effect of meteoroid bombardment is to add mass, which is not what you expect for the first-order interaction.

Irvine What is the evidence for that in the case of Martian satellites?

Kieffer Thermal measurements taken of the satellites going in and out of eclipse. The Martian satellites have very low emission surfaces.

Pollack There is also some meteorite evidence from polarization and photometry studies of Mariner 9 TV images of Phobos and Deimos.

Kieffer Can the Poynting-Robertson effect pull particles across a resonance gap if they were initially in low-eccentricity orbits?

Franklin I think probably not, because these oscillations have characteristic periods of the order of a year. That is small compared to Poynting-Robertson lifetimes.

Kieffer It seems a little incongruous, in light of the theory of the evolution of the solar system, that we might have a ring which is very bimodal, that is, a large number of small particles and a few much more massive objects. I feel that that is probably unstable for any of a variety of reasons, and the effect of the major particles—the large objects—would be to perturb the density distribution in the ring or to just sweep things up slowly. It seems a little incongruous that you could have a few large particles immersed in this sea of much smaller objects, and I would encourage somebody to look into the question of whether that is stable and which way the mass flow would go.

Pollack It was really the exercise of trying to understand the regolith on Phobos and Deimos that led me to consider this possibility for the rings of Saturn. The two are related both genetically and in terms of how things developed historically.

On the question of whether you add mass or you don't add mass, as I tried to indicate, if nothing else happens, the ejecta are eventually going to be captured by the parent body. So the question comes down to considering what else might happen. There are two ways in which you get a situation where recapture, at least directly, is not going to occur. Those are the Poynting-Robertson effect and the buildup of a large enough density of material such that the particles collide with one another before they get recaptured. Furthermore, I think it is important to think in terms of the three-dimensional situation; imagine the parent bodies being above and below the ring plane and the buildup of a large enough density initially so that they can start colliding with one another. What is going to happen then is that the small ejecta will tend to collapse down to a ring structure with the parent bodies in general agreement above and below it so that you are not going to notice the parent bodies.

Kieffer I don't think that sounds too reasonable, Jim, in that it forces a large number of collisions between the parent bodies and this ring of smaller material. Putting the major bodies in orbits of different inclinations forces a large number of collisions, and I think you would rapidly sweep out the small particles and transfer momentum at quite a high rate.

Pollack Why is having many collisions undesirable?

Kieffer The rings at the moment seem to be extremely stable in terms of the particles possessing nonintersecting orbits. They are quite planar. There is no evidence that there are any number of high-inclination particles. Yet, if you have a large number of collisions between a major object, which is penetrating this ring twice a day, either the material sticks to the parent body, in which case it is swept up, or you generate a large number of high-inclination orbits and start perturbing the whole ring out of its stable, flat configuration.

Franklin That is a very good point, and it is terribly difficult to decipher which event has actually happened. I think it is very central to understanding the possible vertical extent of the ring or its collapse.

Kieffer If you allow the particles to impact and bounce off again, you nonetheless transfer momentum, and this would force the particles back into a planar configuration. While the rings may be stable against gravitational collapse, I would seriously doubt that they are stable against collapse in the sense of collisions with bodies of appreciable mass.

Pollack No, I am not sure that the parent bodies really could prevent the collapse, in that the collapse is mostly caused by momentum exchanged between the small colliding particles. In that sense I don't think the parent bodies, unless they were extremely massive, would have that big an influence. But you may be right in the sense that perhaps I need to go to the situation of having just one parent body at the edge of ring B and one at the edge of ring A.

There are two other important points. First, I am not talking about an isolated system, at least initially. There is an important input of energy from meteoroid bombardment. Second, collisions between small particles can become more important than their occasional collision with a parent body.

Charles Lillie What role do electrostatic forces play?

Pollack One classical problem Fred (Franklin) has treated has been the question, "Is there any way to prevent collapse to a monolayer?" That is a serious problem because of the frequency of the collisions. If we didn't have the opposition effect, which does present a strong case that it is not a monolayer, then I think, a priori, one would very likely postulate the ring as a monolayer. If you are willing to accept the opposition effect and say it isn't a monolayer, then you have to come up with some mechanism by which it doesn't become a monolayer.

Franklin In order to get much distention from electrostatic forces you have to make the particles awfully small.

Pollack How small do you have to make them?

Franklin If the particles are on the order of a millimeter or smaller, you can float them to a degree, and even that is pushing things very hard.

Pollack Do you think you could push it to a couple of centimeters?

Franklin I hate to speculate now, but my feeling is no. You can't charge the ring particles to any great degree.

Let me also say I am still a little mystified by the interpretation of the opposition effect. I wonder if it isn't associated with the particles themselves rather than with the interparticle medium.

Pollack Let me repeat the logic on this particular question; it is contained in Bobrov's (1970) review paper. It is true that individual particle surfaces will show an opposition effect, but the real question is, given the very high albedo of the rings of Saturn, do you expect as large an opposition effect as is observed? If you use a Galilean satellite as a standard to try to answer this question—in the sense of providing a surface that has a comparably high albedo to the rings of Saturn—you simply don't get anywhere near as large an opposition effect as you do for the rings of Saturn. One could play with this in the sense of finding some other surface, such as Iapetus or a laboratory sample, and see if the inferences drawn from the Galilean satellites still hold up.

Franklin It is a good game to play.

Pollack Very definitely, and this has an impact on Bill Irvine's model (see contribution by Irvine) in the sense that if we say that some of the opposition effect, though maybe not all of it, is due to the individual particle opposition effect, then we have to revise the derived spatial density numbers.

Irvine Basically, even with multiple scattering, it seems to me very difficult to make the rings cold enough and the reflectivity high enough to reconcile the radio and radar data. As I understand it, in your computations, for example, you took sort of a lower limit on the radar data (see contribution by Morris): 30 percent of a comparable isotropic scattering surface. If you took the value of 60 percent instead, that would push the albedos way up. The result seems to be that you have an extremely narrow size range in which to put all your particles if you are going to get the high albedos you need from the curve of the single scattering albedo versus particle size (fig. 3). I am wondering if you don't run into problems considering the wide range of the radio wavelengths over which the reflectivity has to be very high. You spoke about the 21-cm measurements, but it seems to me that the 3-cm measurements indicate temperatures whose upper limit is comparable; if you try to match that you have a large size range over which the rings have to be very reflective, and it seems to me that it might be bigger than the hump in your curve.

Pollack Let me comment on your two remarks. First, on the radar reflectivity, I took 30 percent rather than 60 percent in the sense of a lower bound, not the actual value. I did that not because I felt the radar observations were that bad, but because I felt the theory was that bad. When you are speaking about specific intensities, particularly in the backscattering direction, it is not that easy to calculate things exactly. Particularly since I was using an isotropic scattering theory, I was very hesitant to try to place precise numbers on the radar reflectivity. I felt a little bit better with the emissivity calculations because they involved integration over all angles of incidence, and one can generally get better answers under situations like that.

In the second-generation calculation I am working on now, I certainly intend to try to do justice to the observations of radar reflectivity. At the stage of the present theory, I thought it was premature to take the radar numbers too literally.

The low brightness temperatures throughout the entire millimeter and centimeter range tend to force you to a restrictive set of x values, which means, in effect, that you don't get consistent particle size information when you consider a range of different wavelengths. To emphasize this point I picked the 1-mm point to contrast with the 21-cm one. There are two important factors that I think will help to resolve this type of conflict. First, I think I used too high a value for the imaginary part of the index of refraction for ice; it should be smaller because I used room-temperature data. A second factor that will help is the finite optical thickness of the rings.

Irvine What do you think of the quality of the available information on the refractive index for ice? From our experience in the infrared, I have, a priori—with no knowledge of the actual data—some skepticism as to how good the data really are with respect to the imaginary part of the index at microwave frequencies.

Michael Janssen I did find some data for ice. I found that the value for the imaginary index of refraction for ice is a little larger than 10^{-4}. It is a lot lower than the value used by Jim, so it is going in the right direction. I found this information in an article by Whalley and Labbé (1969).

Irvine Is there any evidence on the wavelength dependence?

Janssen On a theoretical basis, a frequency-squared dependence is expected. When you take into account the frequency-squared dependence, the 1-mm disk temperature measurements of Rather at Kitt Peak are consistent with ice particles on the order of several centimeters.

Irvine Do you have any idea of the effect of contaminants?

Janssen I suspect, although I haven't looked into it in any detail, that contaminants will introduce greater loss.

Pollack In any case, just to illustrate this point, the curve in figure 3 for ice has a fairly narrow range for which x is close to unity. If we make an order-of-magnitude decrease in the imaginary index of refraction, the curve will be quite broad as far as its extent in x space. I think we still may be in a situation where we don't know the exact value of the imaginary part of the index of refraction. The point is, though, that there doesn't look as if there is any inconsistency.

In the case of silicate particles, we had a really difficult time getting consistent particle sizes between the 21-cm and 1-mm observations because the imaginary part of the index of refraction was too high. In this case also, the imaginary part of the index will go down with temperature. One of the intriguing possibilities suggested by this current work, which I can make a little more solid in a more refined calculation, is the possibility of getting compositional information for the particles as a whole. If you require a very low imaginary part of the index, this may be consistent only with ice particles and not with silicate particles. At this point it would be premature to draw any conclusion like that for sure.

Franklin Jim, just to summarize, would this revision increase the range of particle size and, in particular, allow you to have larger particles than you indicated here?

Pollack It would mean that at any particular wavelength there would be a larger range of sizes that would be consistent. What we are actually after is the intersection when we put all the different wavelengths together. Right now the 1-mm and 21-cm results don't intersect. They are off by a factor of about 5. When we lower the imaginary part of the index, I think they will, in fact, overlap, but their region of overlap still won't be that large. It will indicate sizes on the order of a few centimeters. I think it is going to be awfully difficult to drive it up beyond that.

Janssen The 1-mm observation was a positive result and gives you an estimate for the brightness temperature. The other measurements are all upper limits. On the basis of the 1-mm measurement, if you have an index of refraction for ice and assume a model, you can calculate the particle size. It then becomes a question of what is the right value for ice.

Pettengill What was the approximate size?

Janssen I was just working it out here, and I got an estimate on the order of a few centimeters.

Irvine Jim (Pollack), on another topic, it might not be quite as simple to get particle size information out of the radio occultation data as you indicated. For an optical thickness of 1, there will be a scattering that will require some additional assumptions.

Pollack That is not a problem. In effect, except for possibly the diffraction component, all scattered radiation would be completely scattered out of your beam. Anything that gets scattered will have a very small chance of going off in the direction of the Earth. So it is a question of the angular size of the source, and, to be meaningful in general, the source would have to be fairly small so that almost all the photons that have been scattered will not be seen.

Kieffer In any event, the amount of energy that is scattered into your beam just before there is any attenuation has to be comparable to the same effect during the period of occulation. If you are worried about small angular scattering, then immediately prior to the source going behind the rings you will get an initial contribution from radiation that is coming slightly off axis. The whole question is whether or not you have to worry about anything other than straight geometric cross section.

Pollack I think the answer is no.

Kieffer That's right, I think it is no.

Irvine Well, another way to put it would be to ask how much multiple secondary scattering you get back into the beam.

Pollack Yes, but that's the point. Let's take the case where you have a lot of multiple scattering and you have more or less an isotropic intensity pattern that results. The amount of radiation that you will have and that you will see as a result of this redistribution will be something like the ratio of the angular size of the source to 2π steradians. This will be a very small number.

REFERENCES

BERGE, G. L.; AND MUHLEMAN, D. O.: High Resolution Observations of Saturn at 21.1 cm Wavelength. Astrophys. J., vol. 185, 1973, pp. 373–381.

BOBROV, M. S.: Physical Properties of Saturn's Rings. In Surfaces and Interiors of Planets and Satellites, A. Dollfus, ed., Academic Press, 1970, pp. 377–458.

COOK, A. F.; AND FRANKLIN, F. A.: The Effect of Meteoroidal Bombardment on Saturn's Rings. Astron. J., vol. 75, 1970, pp. 195–205.

GOLDSTEIN, R. M.; AND MORRIS, G.: Radar Observations of the Rings of Saturn. Icarus, vol. 20, 1973, pp. 260–262.

KUIPER, G. P.; CRUIKSHANK, D. P.; AND FINK, U.: The Composition of Saturn's Rings. Sky and Telescope, vol. 39, 1970, p. 14.

RATHER, J.; ADE, P.; AND CLEGG, P.: Brightness Temperature Measurements of Saturn, Jupiter, Mars and Venus at 1 mm Wavelength. To be published.

WHALLEY, E.; AND LABBÉ, H. J.: Optical Spectra of Orientationally Disordered Crystals III. J. Chem. Phys., vol. 51, 1969, pp. 3120–3127.