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THE RINGS OF SATURN



*Proceedings of the
Saturn's Rings Workshop*



NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

THE RINGS OF SATURN

COVER PHOTOGRAPH

This striking photograph of Saturn, showing the shadow of the globe on the rings, was taken at 0430 UT on January 19, 1973, by A. S. Murrell of the New Mexico State University Astronomy Department. The phase angle is $4^{\circ}4'$, and the southern hemisphere of the planet is tipped $26^{\circ}4'$ toward the observer. The print from which this cover was reproduced was a color reconstruction produced by R. L. Fritz from three separate black and white photographic plates exposed through red, green, and blue filters. The telescope has an aperture of 61 cm (24 in.) and a focal length of 45.5 m (150 ft.).

THE RINGS OF SATURN

Proceedings of the
SATURN'S RINGS WORKSHOP
held at the
Jet Propulsion Laboratory
Pasadena, California
July 31 and August 1, 1973

EDITED BY

Frank Don Palluconi
Gordon H. Pettengill

Scientific and Technical Information Office

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

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FOREWORD

The Saturn's rings workshop was held in the summer of 1973 in response to the recent upsurge of observational interest in the Saturn system coupled with the prospect of in situ outer planet exploration, including the 1977 Mariner Jupiter/Saturn mission.

During the winter of 1972-1973, Saturn's rings were detected by radar at a wavelength of 12.6 cm. This detection and the surprising strength of the signal necessitate a rethinking of the properties of the ring particles and of previous interpretations of the passive radio data. In addition, there have been several brightness temperature measurements of the rings in the infrared following spectroscopic identification of water ice as a ring constituent. These measurements constitute an important supplement to the more traditional observations at visual wavelengths. In short, the quality and breadth of Saturn ring data now place severe and challenging constraints on ring models.

Saturn's rings not only constitute a prime scientific objective for space missions, but they may, as well, represent a danger to such spacecraft. There is some evidence that particulate matter may exist beyond the bright rings. The extent or even the existence of such material is not adequately established, and it has an important influence on planning for future missions to Saturn.

This workshop was organized to consider these several topics:

1. The nature of Saturn's rings
2. The hazard to spacecraft from ring particles
3. Identification of fruitful ground-based and near-earth observations and analyses
4. Recommendations for future ring observations from spacecraft, with emphasis on the Mariner Jupiter/Saturn 77 project

Participants, including six members of the Mariner Jupiter/Saturn science teams, were invited to address these topics at a workshop held at the Jet Pro-

pulsion Laboratory on July 31 and August 1, 1973. This workshop was initiated at the request of the Planetary Programs Office of NASA Headquarters and supported by the Advanced Technical Studies Office of the Jet Propulsion Laboratory. Dr. Gordon H. Pettengill of the Massachusetts Institute of Technology organized the scientific program and served as chairman.

These proceedings contain the contributions given at the workshop, along with the discussion that accompanied each presentation. The proceedings have been organized in the same sequence as the workshop agenda. Special effort was made to capture the spirit and enthusiasm of the workshop by including the extensive discussions that took place. It is hoped that many of the ideas germinated at this workshop will eventually become full-fledged papers in the open literature.

*Ichtiague Rasool
Planetary Programs Office
National Aeronautics and
Space Administration*

Editors' Note

From the inception of the Saturn's rings workshop through publication of these proceedings, numerous individuals have contributed to organizing and conducting the workshop and preparing the contributions for publication. The editors would like to express their appreciation to all who contributed to the success of the workshop and especially to the participants who so readily agreed to attend and contribute.

In publishing these proceedings, it was decided that early release had highest priority. To accomplish this goal it was necessary to limit participation in editing the full workshop transcript. As a result, authors were given the opportunity to review their own contributions and the accompanying discussions but were not able to review their own comments made throughout the entire workshop. While this process did speed publication, it may have introduced error, for which the editors assume full responsibility.

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OPENING REMARKS

The stated purpose of this workshop is to provide NASA with the best available current information on the hazard to space vehicles presented by Saturn's rings, with the information to be used as inputs in targeting considerations for Saturn-dedicated and swingby missions.

I would like to interpret and expand on this stated purpose in the form of what I would call the desired output from the point of view of the Mariner Jupiter/Saturn (MJS) mission. I can summarize it in three specific comments.

First of all, we would very much like to get what I call a "best" model of Saturn's rings, to use for mission-design purposes. At the present time we are in the throes of mission-design activities. This breaks down into three areas, and somewhat different information is needed for each.

The first is trajectory design. Where do you fly by the planet to be both safe and productive?

The second area is the spacecraft design itself. What kind of protection should there be on it?

The third is the question of the instrument design. What ranges of parameters should one be able to detect in order to get information on the rings?

The second desired output comprises ideas that you may develop on how this model might be improved during the rest of the mission-design phase, which I consider going on at least until launch. The variables certainly get firmed up as we go along in the process, but one thing that continues as a variable, in fact past Jupiter encounter, is the trajectory (in other words, where you fly by the planet). So I think future information can and will be set into the mission planning.

Lastly, desired output will include any ideas you may develop on how the MJS mission could be improved with respect to obtaining Saturn ring information. This really relates to the values of various kinds of measurements and how they complement one another and fit together.

Project manager, 1977 Mariner Jupiter/Saturn Mission.

This mission is designed as a two-flight mission. We are trying to set it up such that we can make use of an adaptive mode at Saturn. The encounters of the two spacecraft will be separated by 1 to 4 months, depending on the final targeting. We would like to keep this separation reasonable so we can retarget the second spacecraft on the basis of the information we receive from the first and thereby maximize the return from the overall mission.

These may be parochial comments, but I think they are some things to shoot for, and they would be very important and valuable to the MJS mission.

Gordon Pettengill I think that we all realize the importance of understanding the properties of Saturn's rings from the standpoint of mission requirements, but I think many of us also feel the importance to science, in general, of having a workshop of this type at this time.

STRUCTURE OF SATURN'S RINGS: OPTICAL AND DYNAMICAL CONSIDERATIONS

What I would like to do is to consider some properties of Saturn's rings from two rather distinct aspects. First, I would like to talk a little about some of the photometry—specifically, photometry in the visual region—leading to photometric phase curves of Saturn's rings. Then I should like to point out to you some of the past interpretations that people have brought forth from these curves to indicate what information is available and try to be somewhat critical.

Second, I want to speak on what information can be provided by certain dynamical arguments. It has been a long-standing conviction, I think, that the perturbations of the inner satellites on the ring particles are to a large extent responsible for many of the features of the radial profile of the ring. I would like, therefore, to rediscuss with you briefly some of the dynamical arguments and indicate what seems to me one area of conflict between dynamical and photometric models of the rings and then, in the last few minutes, try to resolve aspects of this discrepancy by presenting to you a new observation which may indirectly aid in the resolution of this discrepancy.

What I am mainly going to be concerned with is ring B, the bright interior ring, and the outer one, ring A. As you know, there are other rings closer to Saturn that introduce quite exciting new problems, but I am going to ignore them until discussion later. What I would like to call your attention to is the Cassini's division, a conspicuous gap, which is situated very close to a distance from Saturn corresponding to one-half the period of Mimas. The Cassini's division is some 4000 km across, and it presents a real challenge that any comprehensive dynamical ring model must explain.

I want to speak only for the moment about the dependence of ring brightness of angular separation of the Earth and Sun as viewed from Saturn; that is, upon the solar phase angle α . Figure 1 is a photometric scan along a portion of the major axis of the Saturn system taken from a photograph exposed near quadrature when

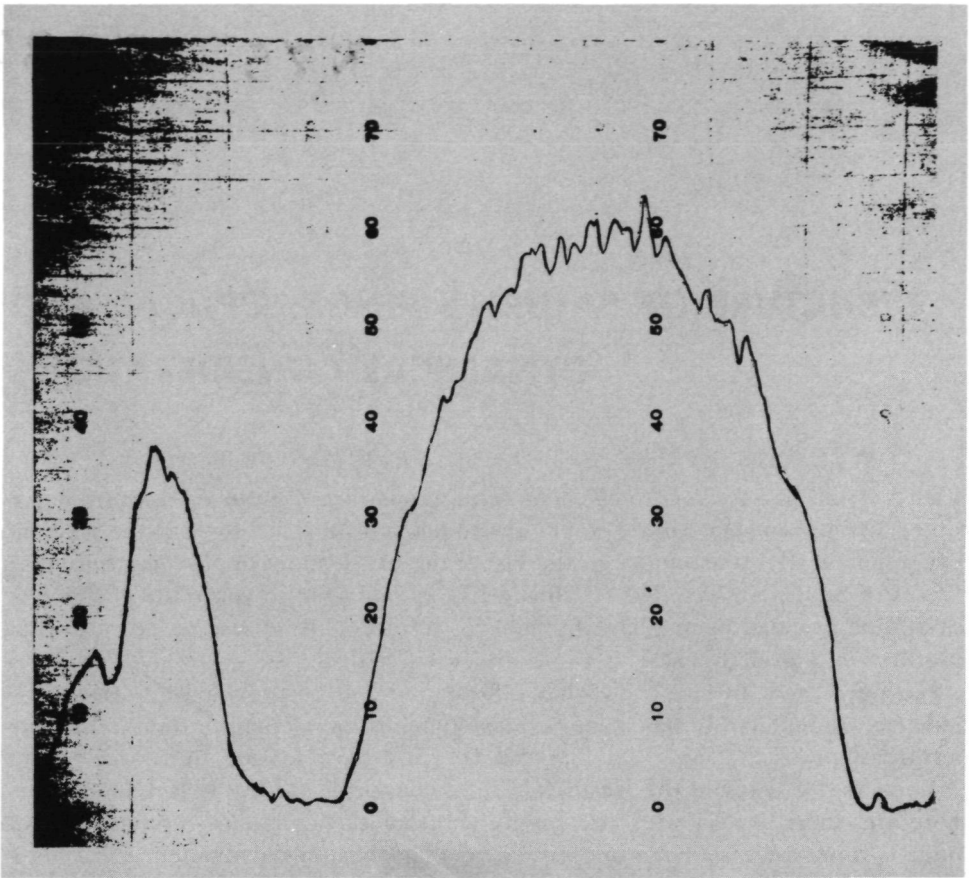


FIGURE 1.—An unrectified tracing of one ring ansa and of the disk of Saturn obtained from a photograph (in V) taken near quadrature, $\alpha = 6^\circ$.

$\alpha \cong 6^\circ$. On this unrectified tracing, in which only the disk and one ansa are shown, it is clear that ring B is much fainter than the brightest part of the disk and that ring A is fainter still. At opposition—that is, when the phase angle has fallen to zero so that the Earth and the Sun are in a direct line with Saturn—a similar photographic scan, figure 2, shows that ring B and ring A have both brightened enormously with respect to the disk. This behavior has been known for some time and has been measured reasonably accurately. Measurements, not just at quadrature or opposition but throughout the entire range $0^\circ < \alpha < 6^\circ$, give results similar to those of figure 3, which is a plot of magnitude in the visual (that is, the broad band V of the Johnson-Morgan System) against phase angle α . It is unquestionable that the ring does change its brightness with time, depending on solar phase angle, in this way.

The curve of figure 3 has two distinct portions. From about 1° or 1.5° to 6° or somewhat more, it is quite linear. But shortwards of about 1.5° , it becomes quite steep and nonlinear. This latter portion is the one of particular interest. The right-

hand part ($\sim 1.5^\circ < \alpha < 6^\circ$), usually labeled in the literature as the “phase variation,” can be accounted for by the scattering of individual particles. One knows, for instance, that many objects in the solar system—the Moon at large phase angles, asteroids, and some satellites—show a behavior similar to this one. A steep slope of the sort shown in figure 3 for the particles of Saturn’s ring, that is, of about 0.030 or 0.035 magnitude per degree, is characteristic of an object with rough and pitted surfaces.

The nonlinear portion or “pip” in the region $0^\circ < \alpha \lesssim 1.5^\circ$ is referred to in the literature as the opposition effect and is quite marked in the case of Saturn’s rings. To my knowledge, it was the first object in the solar system to show such behavior. Most of the planets tend to show linear or nearly linear phase curves, all the way to $\alpha=0^\circ$. Now it is well known that both the Moon and most of the asteroids do show an opposition effect. But Saturn’s ring remains, I think, some-

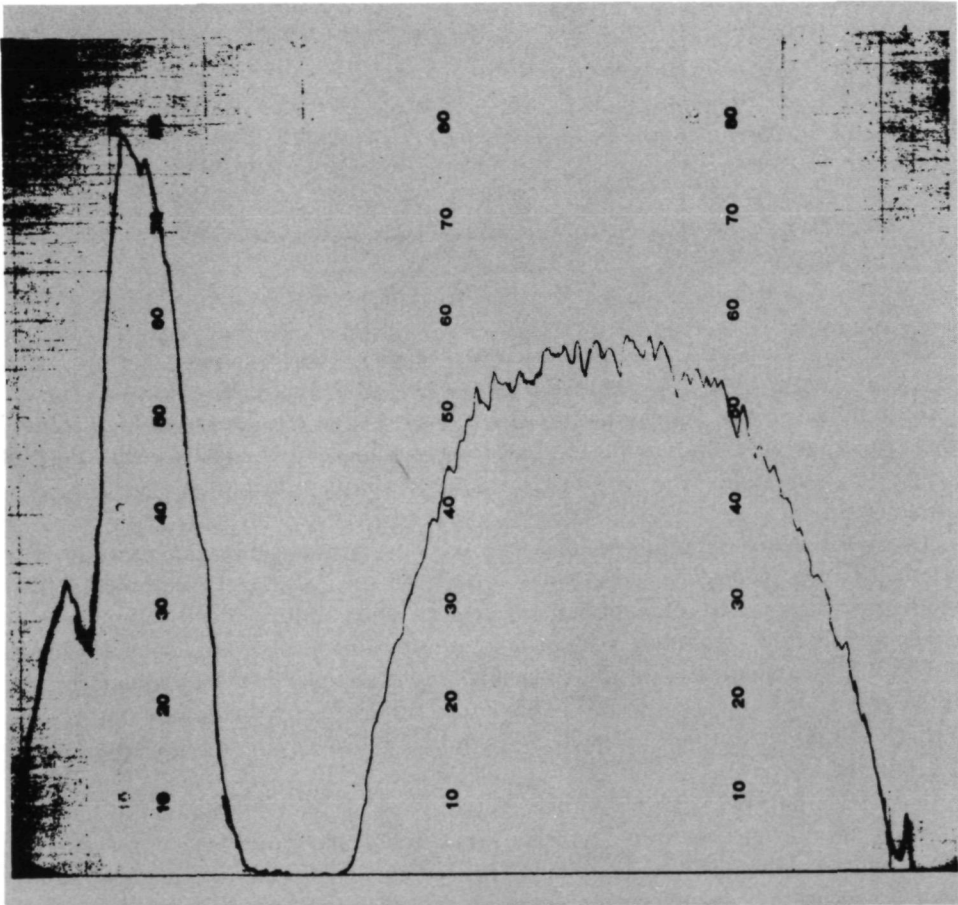


FIGURE 2.—Similar to figure 1 except that photograph was taken near opposition, $\alpha=0.1^\circ$. Note the brightness increase of rings A and B relative to the disk of Saturn.

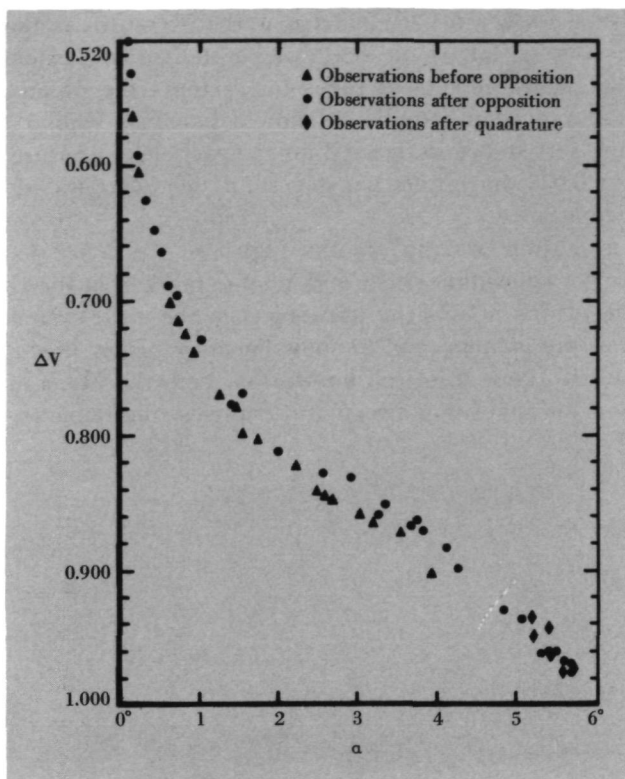


FIGURE 3.—Visual (V) phase curve of Saturn's B ring in a plot of magnitude vs solar phase angle.

what unusual, in that it exhibits this effect even though it possesses a high albedo. One would expect that multiple scattering would tend to destroy this type of brightness surge, but the ring unquestionably shows a brightness surge that is pronounced.

The interpretation of this nonlinear brightness surge, which amounts in V to about 0.2 magnitude, has led to what I shall call the "classical ring model." This model provides a ready explanation for the opposition effect; indeed, the model was suggested by it. To elaborate very briefly: at opposition, when $\alpha = 0^\circ$, each particle covers its own shadow, and the ring as a whole appears correspondingly bright. However, as α increases toward its maximum value of $\sim 6^\circ$, shadows of foreground particles impinge on those in the background, and the ring consequently undergoes a brightness decrease.

It is clear that the brightness changes will depend upon the amount of material in the ring. Various workers, some in great detail, have derived expressions for this brightness dependence upon α , and, I believe, Dr. Irvine (see contribution by Irvine) will present some very nice recent work along this line. In general, one finds that it is possible to fit a theoretical curve to the observed opposition effect and thereby derive one parameter of particular interest, the so-called volume density, D . This parameter is the fraction of the total ring volume occupied by particles,

and one finds in the literature values of D that range between 10^{-2} and 10^{-3} , with perhaps a few that are even a bit smaller. Inasmuch as $D < 10^{-2}$ appears to be well established observationally, the view that the ring is a medium many-particle radii in vertical extent has found wide support. This conception of the ring is what I refer to as the classical ring model. In some sense, the observational work that supports this model also suggests that it may require at least a slight revision. It seems quite well established, for example, that the opposition effect is wavelength-dependent in a rather complex fashion—a fact that might be accounted for by the albedos of the ring particles. Photometry also provides other parameters for the ring system: geometric albedos, Bond albedos (by inference), and optical thicknesses.

The basic point I wish to stress here is that the only observational support for the classical ring model is the opposition effect. It seems, therefore, important to ask whether the opposition brightening could result, at least in part, from any other mechanism. Studies by Oetking (1966) and Hapke (1966) indicate that this may indeed be the case. Their laboratory studies have shown that a wide variety of different substances do exhibit a brightness surge when the phase angle drops below about 1° . This behavior appears to be largely independent of particle size, roughness, and, to some extent, even albedo. One would expect that as the single scattering albedo rises, an opposition effect would diminish, owing to the increasing role of multiple scattering. To some extent laboratory samples did show this anticipated behavior, but even from albedos of 0.7 to 0.8 a brightness surge of ~ 10 percent was measured. Because the laboratory measures indicated that this phenomenon is very common (although ice particles were not examined), one is strongly led to the suggestion that a part of (and maybe the entire) opposition effect presented by the rings can be traced to the particles themselves and therefore is not the result of interparticle shadowing. Should this prove completely true, then the main observational prop of the classical model is removed, and the rings might, therefore, be simply a medium that is (nearly) a particle diameter in vertical thickness.

Let me leave the subject of photometry and survey for you some work that three colleagues—Drs. G. Colombo, M. Lecar, and A. Cook—and I have been doing on the dynamics of the ring system and related topics. There are clearly certain approximations involved in the course of this work that I don't mean to obscure, and please feel free to criticize if you are unhappy. I have to confess that I began with the naive hope that all one had to do was to take a planet, put a large number of massless ring particles around it, introduce the inner satellites with their appropriate masses (they are quite well known but might be increased so that things would happen faster in a sample calculation), and one just turned on a machine and after a while discovered that the satellite perturbations, particularly ones near resonance, had sculptured the ring, so that from an initially featureless uniform ring Saturn's ring was formed. I was set to lease the movie rights so that one could watch a nice uniform ring quickly being sculptured into Saturn's ring. Well, the movie has yet to be made and I am afraid my hopes were terribly naive. The problem is simply that things happen on an enormously slow time scale, not

slow cosmologically, but very slow when it comes to the allowances made by administrators of computing facilities.

I would like to digress for just a moment to introduce some quantitative calculations we have carried out for bodies in and near the present asteroidal belt. These calculations include the planets Mars, Jupiter, and Saturn—with their observed masses—and a swarm of massless bodies, with the objective of seeing whether planetary perturbations alone could produce the presently observed distribution from a uniform belt of asteroids extending from Mars to Saturn. Results (Lecar and Franklin, 1973) show that if gravitational perturbations by the planets are responsible for the details of the asteroidal distribution—particularly in the region between the resonances at $1/2$ and $2/3$ of the period of Jupiter—then they require times much greater than $\sim 10^3$ Jovian periods to be effective. By inference, the inner satellite Mimas, with a mass $\sim 7 \times 10^{-8}$ that of Saturn, would require, to produce ring features, a time much too long to be accessible by direct computational methods. One has therefore to examine this problem indirectly.

Before doing so, however, I would like to call attention to figure 4 which shows, for a primary to secondary mass ratio, μ of 10^{-3} , the region near the 1:2 resonance. Here we have the oscillations in the semimajor axis a of a body near the 1:2 resonance plotted as a function of time measured in periods of the perturber (secondary). There are, it seems to me, two features to be mentioned in the present context. First, it is clear that the oscillations in a are substantially

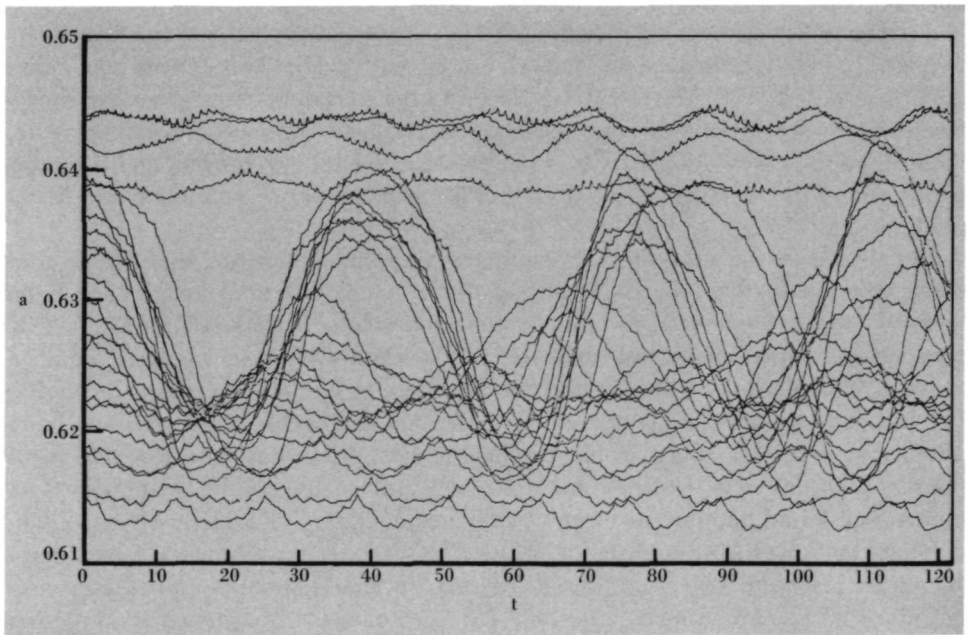


FIGURE 4.—Semimajor axis a vs time t measured in periods of Jupiter for bodies at and near the 1:2 resonance (1:2 being the ratio of the periods of an asteroid and Jupiter).

greater inside than outside of resonance. This is also true of the similar oscillations in the eccentricity e ; both are of the order $\mu^{1/2}$. The second point is the absence of coherence in this type of motion: one is not dealing with a running density wave that follows the perturber around, but rather a longer period oscillation for which, at a point in space and time, two particles may have different velocity vectors.

With these remarks in mind, let us return to the specific dynamical problems presented by the rings. In view of the difficulty of carrying out a complete dynamical calculation, with realistic values for the satellite masses and with the inclusion of collisions, we have tried to begin to understand whether the perturbations of the satellites on ring particles could be sufficient to have a marked effect on the ring—to produce, for example, the Cassini division or to define the outer boundary of ring A—by a simple and approximate argument. Please have patience here, because I do not want to stress the argument itself so much as to apply it gently a little later. What we have done is to suppose that the ring has evolved, owing probably to particle collisions, to a state such that ring particles are now separated from one another by just such an amount that mutual collisions no longer occur. It is then a straightforward matter to calculate, from the formalism of the restricted three-body problem, a set of periodic orbits (in the rotating frame) that fulfills this condition, i.e., that are so spaced that objects moving in such orbits do not collide. Effectively, then, we have calculated the area, in this planar model, of the epicycle executed by a particle, with a given semimajor axis, owing to the perturbations of the inner satellite, Mimas. We then make the assumption of a collisionless ring (at the present time) so that the area of these epicycles must be inversely proportional to the surface density of the particles. We make the further reasonable assumption that the observed brightness is proportional to the surface density, and we are therefore able to plot a profile of the radial structure of the ring under the influence of a perturbing satellite. Near a resonance, for example, the epicyclic areas increase rapidly, leading to a much reduced brightness.

Results are shown in figure 5 for the case in which the perturber, Mimas, with a mass 6.7×10^{-8} that of Saturn, moves in a circular orbit. In this figure, pairs of vertical lines mark the resonances; the right-hand one of any set includes the effect of the oblateness of Saturn. It effectively shifts the position of a resonance slightly outwards. Also marked near the bottom of this figure are measures of the width of the Cassini division, where the upper set of arrows defines the most probable value. The inner boundary of ring B, the outer boundary of ring A, and the Encke “division” are also indicated by vertical arrows. The horizontal scale of this figure plots distance from Saturn, where 1.0 corresponds to the semimajor axis of Mimas’ orbit. The vertical scale is the inverse of the epicyclic area; or, in other words, ΔR is the radial excursion executed by a particle in a periodic orbit with a given semimajor axis owing to the perturbations of Mimas. The quantity $R\Delta\theta$ is the corresponding tangential excursion.

There are, it seems, both encouraging and discouraging features to the approach depicted in figure 5. The first encouraging one is that ring B is predicted to be the brighter of the two major rings. Secondly, it is clear that, although this

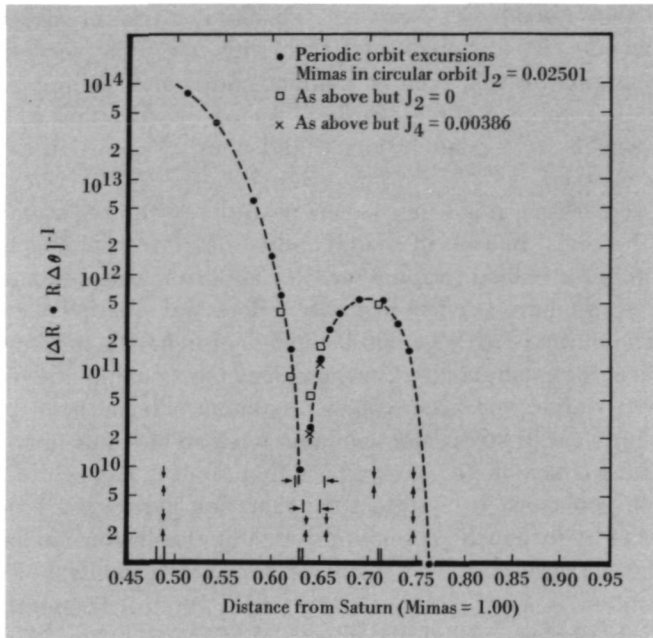


FIGURE 5.—*Predicted density profile of the ring obtained with the perturber, Mimas, in a circular orbit. Arrows mark measured ring features. Both scales are drawn such that $R_{\text{mimas}} = 1.0$. The quantity ΔR , for example, is the minimum radial separation of adjacent particles such that collisions will not occur.*

model does not offer a precise definition for the extent of the Cassini division width, it has produced, in a region whose size is comparable to the observed Cassini division, a surface density that is low relative to its value in rings A and B. Finally, note that the outer boundary of ring A has a close association with the 2:3 resonance with Mimas.

There are, however, discouraging features associated with this model as well. First, no inner boundary to ring B is predicted while a sharp one is observed. The Encke division, which apparently lies near the 3:5 resonance, is also absent. Finally, the position of the predicted and observed locations of the Cassini division do not agree; the gap predicted by this model lies closer to Saturn than the observed one by about 0.2 second of arc. This latter point has been used to support the claim that resonances with Mimas are insufficient to account for the details of the ring structure. The first two discouraging features can to some extent be removed by including in the calculations the eccentricity of Mimas' orbit. This serves to excite other (higher-order) resonances, e.g., at 1:3 and 3:5. It also serves to broaden slightly the predicted Cassini division width. These results are presented in figure 6.

I do wish again to direct your attention to the displacement of the "predicted" location of the Cassini division from the observed one. Now to suggest a possible way to account for this shift: if ring B is sufficiently massive, such a displacement

must occur. To put it briefly, if the mass of ring B is large enough, then the local orbital frequency of a particle near or beyond its outer boundary will be markedly increased. Thus, the distance from Saturn corresponding to the 1:2 resonance with Mimas is displaced outwards (when compared to the case of negligible mass in ring B). This approach leads us to a revision of the work presented in figures 5 and 6, for now we wish to recalculate those curves but to include as a parameter the mass of ring B. Franklin, Colombo, and Cook (1971) have examined these ideas quantitatively and have shown that the required shift of the resonance can be produced if the mass of ring B lies near 6×10^{-6} that of Saturn, give or take 30 percent. If we adopt a vertical ring thickness of 2 km, this value of the mass leads to a mean ring density of about 0.1 gm/cm^3 . Although this value is rather large, it does not conflict with stability criteria; densities larger by a factor of 2 or 3 would still be stable against self-gravitating condensations.

These results conflict with our earlier remarks concerning the volume density derived from the classical ring model. From dynamical considerations, we require the mean density for ring B to lie near 0.1 gm/cm^3 . From the classical model, we obtained a volume density less than 10^{-2} . This figure, of course, coupled with a particle density of $\sim 1 \text{ gm/cm}^3$, leads to average densities that are at least an order of magnitude smaller.

It is this dilemma that needs to be resolved. Perhaps I did drop a hint at the

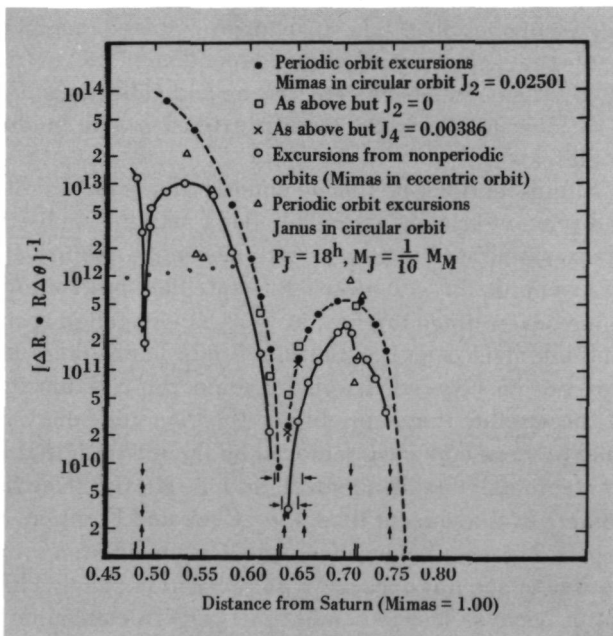


FIGURE 6.—Predicted density profile obtained with Mimas in an eccentric orbit ($e = 0.02$). Some features resulting from the recently discovered satellite, Janus, are also indicated. For further details, see Franklin and Colombo (1970).

outset as to a possible resolution; in any event, I would like to suggest that the trouble lies in the rather tacit assumption that the opposition surge is produced solely (or largely) by interparticle shadowing. Inasmuch as Oetking (1966) and Hapke (1966) have both suggested that a nonlinear opposition surge is a phenomenon presented by numerous laboratory samples, I would like now to suggest that this phenomenon is also presented by the particles of Saturn's ring. To test this suggestion we require a determination of the phase curve of an individual ring particle—something that is not, alas, even possible for the MJS mission. However, if we broaden our requirement slightly and ask for the phase curve of a single ice-covered body in the Saturnian environment, the prospect is not so bleak. Saturn is, after all, attended by an abundance of satellites, and the evidence is rapidly growing that particularly the inner ones are ice coated if not solid ice. By way of evidence, let me just remind you that it is impossible to reconcile the brightnesses with the densities of the inner satellites, unless their albedos lie near 0.6 and their densities are near unity. My hope, then, is to substitute the phase curve of a satellite for the phase curve of a ring particle by making the claim that an icy surface structure is common to both. Unfortunately, this substitution is not quite so easy or immediate as one might wish. The problem here is twofold. First, as any observer will attest, the scattered light from the disk and rings of Saturn makes photometry of the inner satellites very, very difficult. Second, existing photometry suggests rather decidedly that the brightness of the inner satellites depends upon orbital phase in a pronounced manner. This makes a determination of the solar phase angle dependence more complex and time consuming. Dr. D. Morrison informs me that he and colleagues are hard at work in the fine skies of Hawaii to provide us with extensive high-quality photometry of these inner satellites.

Although it sounds curious at first encounter, the best candidate for a single ice-covered and photometrically accessible body may be the bright hemisphere of the more distant satellite, Iapetus, which lies at 60 Saturnian radii from the planet. As you are no doubt well aware, this satellite shows enormous light variations, being some seven times brighter at western elongation than at eastern. This very large amplitude itself suggests that the bright hemisphere has a high albedo, i.e., that it may be ice covered. I would remind you too that it is the "trailing" hemisphere of the satellite that is bright and the "leading" one dark. This suggests that an erosional process may have removed all the ice from that hemisphere upon which, say, meteoroidal flux is greatest and is slowly removing ice from the trailing hemisphere at the present time. (See Cook and Franklin, 1970, for details.)

During the past 2 years, I have been making observations of the bright face of Iapetus in order to see if it does show an opposition effect. This project seemed especially fruitful because last December the western elongation of Iapetus (when the satellite is at maximum light) nearly coincided with opposition. Since I began these observations, Drs. D. Morrison and R. Murphy have carried out infrared measurements of Iapetus that argue for an albedo of ~ 0.3 for the bright hemisphere—rather lower than what one might expect for ice. Thus it may well be that only a fraction of the entire hemisphere is ice coated. With these caveats and

reservations in mind, let me still proceed with the tentative claim that a phase curve of Iapetus might have a resemblance to the phase curves of individual ring particles. Figure 7 is a plot of the variation of the brightness (maximum) of Iapetus at western elongation (vertical scale) versus solar phase angle α (horizontal scale). It is at once clear that Iapetus does brighten substantially at opposition—by some 0.3 magnitude over its brightness at $\alpha \cong 6^\circ$. However, it is also apparent that figure 7 is as yet incomplete; one requires a knowledge of the satellite's brightness in the range $1^\circ < \alpha < 3^\circ$. I would add too that the point at 3° rests upon too few observations to be too much relied upon. Note the date at the top of the figure; on January 10, 1974, a brightness maximum falls at $\alpha \cong 2^\circ$, and observations at that time will be crucial in deciding whether the slope of the phase curve is steep but linear or whether it is less steep but shows an opposition surge.

Let me just conclude by saying that I think there begins to be some evidence that the chief underpinnings of the classical ring model requiring that the rings be a medium many-particle radii in vertical thickness may be somewhat less than secure. It seems to me, therefore, that a model satisfying both dynamical and

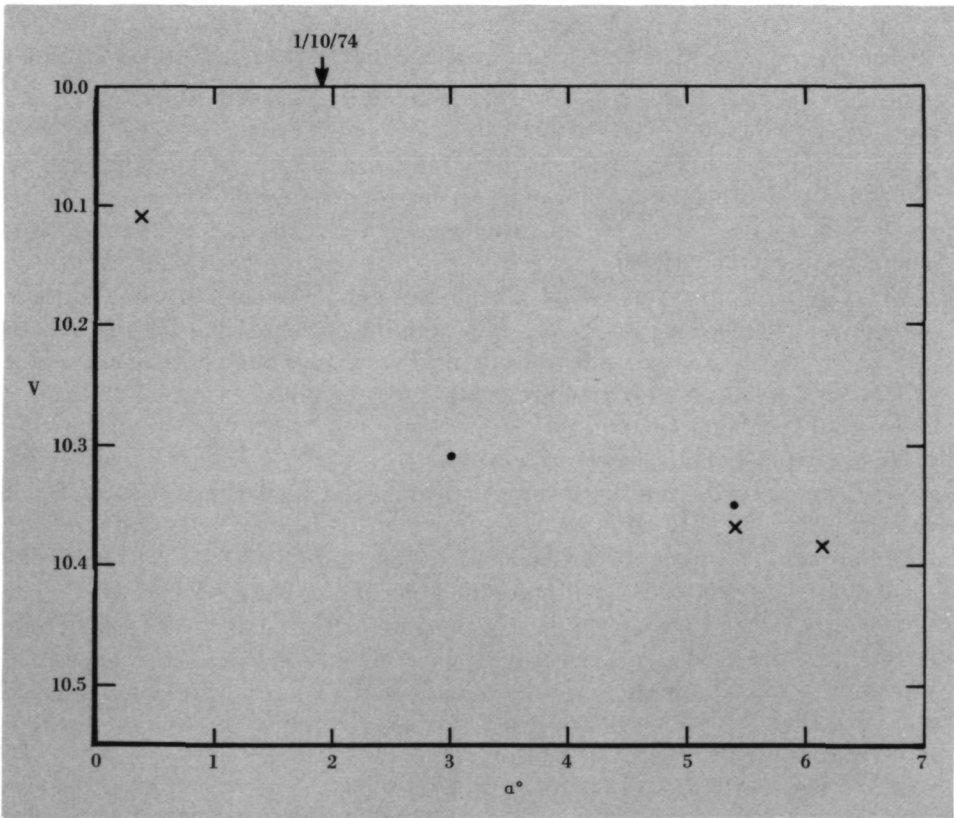


FIGURE 7.—Brightness (in V) of Iapetus at its western (or bright) elongation as a function of solar phase angle, indicating that this satellite displays either an unusually steep phase variation or an opposition effect. Dots are observations of R. Millis; crosses are those of the author.

photometric constraints is one in which the rings are nearly a monolayer of large bodies.

DISCUSSION

James Pollack My question relates to the type of opposition effect you might expect from an ice particle. In Bobrov's review article (1970) he addresses himself to this question and he points out that the Galilean satellites, for which there is some evidence that they are ice covered and quite bright, do not show as big an opposition effect. What is your reaction to that sort of evidence?

Fred Franklin Well, one very trivial reaction. The Jovian environment is a very strange one. In fact, even the models that suggest Jovian satellites have ice also often introduce an overlying layer of something else. I am somewhat happier doing observations of this sort in the Saturnian environment, but what you say is a very good point.

William Irvine Isn't it also true that the strongest opposition effect, or perhaps the only real indication of an opposition effect, is for the darkest Galilean satellites?

Pollack That's right. Bobrov's point was that if you take something of comparable albedo to the rings it doesn't show the same large opposition effect.

Robert Murphy In my work with Cruikshank and Morrison we also observed Rhea, which definitely has an albedo on the order of 0.5 or 0.6. I think Rhea would be an ideal satellite to look at for an opposition effect.

Franklin Rhea does show an orbital phase angle variation of a rather variable character; isn't that right?

Murphy Morrison has derived an albedo and radius for Dione, using infrared radiometry and visual photometry. His preprint gives a value for the density of 1.2 g cm^{-3} , and a visual albedo, with an average over the entire visual region of 0.6. That would suggest we have another icy satellite.

Irvine That is a Bond albedo?

Murphy That is a visual geometric albedo.

Gordon Pettengill Do you think you can actually get a solar phase curve, David (Morrison)?

David Morrison Yes, and in particular I have one here for Rhea. We have had a photometric project this last opposition at Mauna Kea Observatory. The photometry is good enough to clearly separate orbital phase from solar phase effects. It is not good enough to make a clear distinction between an opposition pip and a gradual increase, but the average for Rhea has a wavelength dependence and runs from about 0.025 magnitude per degree in the blue up to about 0.040 magnitude per degree at 8000 \AA .

Franklin The increase with phase angle is fairly linear.

Morrison A linear fit was consistent with the data which, as I say, is not good enough to make a clear distinction.

Franklin But you will look for opposition effects, in time, or could you put limits on any opposition effect at the present time?

Morrison I don't have a plot here that would let me put limits on it, but I think we can.

Franklin That would be very nice.

Murphy In deriving a phase curve for Iapetus, we have to bear in mind that the surface has some very dark material and some very bright material. The observing geometry changes with time, and we may get very strong variations in the fraction of the observed area covered by each material type.

Franklin I can only defend myself by saying I was aware of that and did measure successive western elongations to minimize any change, but it certainly is there.

Hugh Kieffer I would like to change the subject a bit and question you about the dynamical model. In deriving these reasonably good fits of brightness versus radius from the planet, you assumed that each little epicycle was basically clean except for one particle.

Franklin That's right.

Kieffer It seems to me as though that in itself immediately places very strong constraints on particle size, which must be kilometers.

Franklin I would never use any argument like this to get a particle size. In fact, I dwelt on it today to such an extent only to have a "zero-order" model in which I could estimate how much mass would have to be put into ring B to shift the Cassini division. I can only say that the problem is difficult, and one really has to consider collisions and one has to introduce an elasticity parameter. For the present I don't know how to handle the problem except in the rather simple way that I have outlined. The model is therefore incomplete, certainly, but I do not see that it is inconsistent. The real question that it addressed was, "How particles would arrange themselves, given the perturbing force of Mimas (and Titan), if no mutual collisions were allowed?"

Kieffer I don't know what the right answer is. I just wondered what your thoughts were.

Franklin The minimum epicycle diameter is, by the way, of the order of 200 meters. This value would apply near the center of ring B, at large distances from any strong resonance. In this model, then, a figure of this order would formally correspond to an average particle diameter.

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William M. Irvine

MODEL OF SATURN'S RINGS THAT SATISFIES THE OBSERVED PHASE CURVE FOR OPTICAL SCATTERING

I would like to begin by stressing that this is a preliminary report, and any numbers quoted are subject to revision. If the procedure we are using is of interest to anyone, I hope they will consult with me. I wouldn't have presented this material at this time if it were not for the workshop nature of our discussion. The results are principally the work of Y. Kawata, a graduate student at the University of Massachusetts, and in essence, they are a refinement of the classical model to which Dr. Franklin has referred (see preceding contribution by Franklin).

Several workers in the classical tradition have stressed the importance of refining previous calculations to include rigorously the effect of multiple anisotropic scattering, and that is what we have done, including the effect of the solar penumbra, which becomes important in shadowing computations of this kind. Our basic idea has been to see if we could take the classical model and match the observations, including the wavelength dependence, simply by varying the particle albedo as a function of wavelength, and thus obviate the need to consider diffraction by the particles. This provides a model different from that Franklin and Cook (1965) proposed in the past.

Let me fairly briefly go over the procedure, which is well known in the literature. The basic data that we have been attempting to fit include, first of all, the phase curve, which Dr. Franklin has shown. This phase curve has three characteristic features and is shown in figure 1. We have normalized the visual and blue curves at the smallest phase angle observed by Franklin and Cook (1965). There is the opposition effect or surge in brightness near opposition, the linearly increasing portion at larger phase angles, and the very important wavelength dependence. I might stress at this point the very great desirability of obtaining phase curves that are this complete and of this quality at other wavelengths, both longer and shorter. The other critically important information is the absolute surface brightness of the rings. It is matching the shape of the phase curve over its entire range

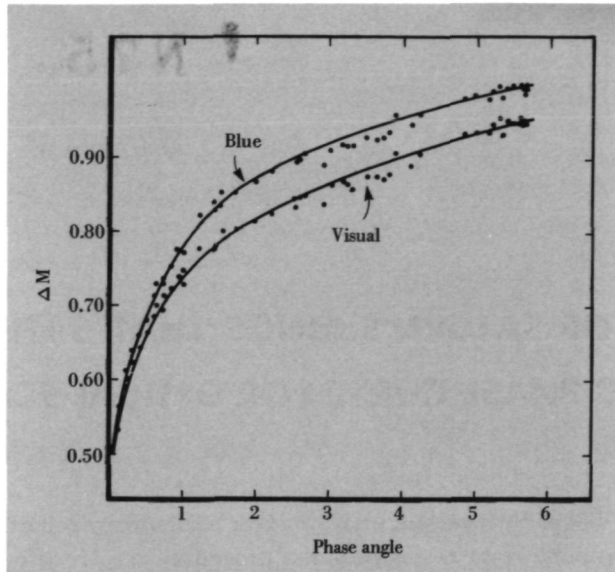


FIGURE 1.—Phase curve for Saturn's B ring showing relative brightness as a function of phase angle in degrees.

and matching the absolute brightness of the rings, which we have required of our model.

There are also data on the variation in the brightness of the rings as a function of the elevation angle of the Sun and Earth above the ring plane. I have found the treatment of those data in the literature somewhat confusing and have been unable to make a very good judgment as to how reliable they are. Different observers seem to get different results when they reduce the data because of seeing and various photographic effects on the plates.

As Dr. Franklin has pointed out, the principal diagnostic feature of the phase curves is the opposition effect. In the classical tradition, we have treated this as a result of mutual shadowing in a layer that is many particles thick.

We have in our model taken the rings to be homogeneous, so we have not looked at the effect of possible variations in particle properties with altitude in the ring, something proposed by some authors. Initially we took the ring particles to be monodispersed (characterized by a single particle size) and treated them as spheres for the purpose of the shadowing calculation. That doesn't, I think, significantly affect our results, because it can be shown that the magnitude of the shadowing effect at opposition is independent of the particle shape. We did not assume that the particles scattered like Mie particles. A spherical particle was used simply to give the geometry of the shadow. I will return later to the question of a distribution of particle sizes.

What we have done is to take this homogeneous layer with randomly distributed particles, solve the radiative transfer problem for radiation incident on this ring including the multiple scattered intensity for arbitrary phase functions,

and include in the first-order scattering a correction for shadowing, which can be done in a straightforward manner. In this way we can account for both mutual shadowing of the particles, which Dr. Franklin has described, and anisotropic multiple scattering. We have, unlike Bobrov (1970a), for example, taken our results for parallel incident solar radiation and integrated over the solar disk to provide the range of angles of incidence that are important. If you haven't been familiar with this problem, it may seem somewhat surprising that the angular size of the Sun at Saturn can significantly influence the shape of a computed phase curve when the size of the Sun is something like 3 minutes of arc. But, in fact, that is a very important effect. It significantly reduces the magnitude of the opposition surge.

Figure 2 shows the magnitude of the shadowing effect with first-order scattering only, in stellar magnitudes between a phase angle of 0° and 6° , for models with a point Sun or models that integrate over the finite Sun. The effect is much reduced for a finite solar disk as a function of the volume density parameter, D , that Dr. Franklin spoke about. It is interesting to see that, when you account for the finite size of the Sun, you get a maximum phase effect at a particular volume density. If the volume density gets too large, then the rings effectively get filled up with matter and the shadowing becomes less important, essentially because the peak produced by the shadowing becomes very broad. Likewise, if the volume density becomes very small, with parallel radiation incident on the ring all your shadows would be cylindrical and would go on to infinity, so you reach an asymptotic situation. In the real world where the Sun has a finite size, you get radiation coming into the shadowed zone, so if you put the particles far enough apart you lose the shadowing effect.

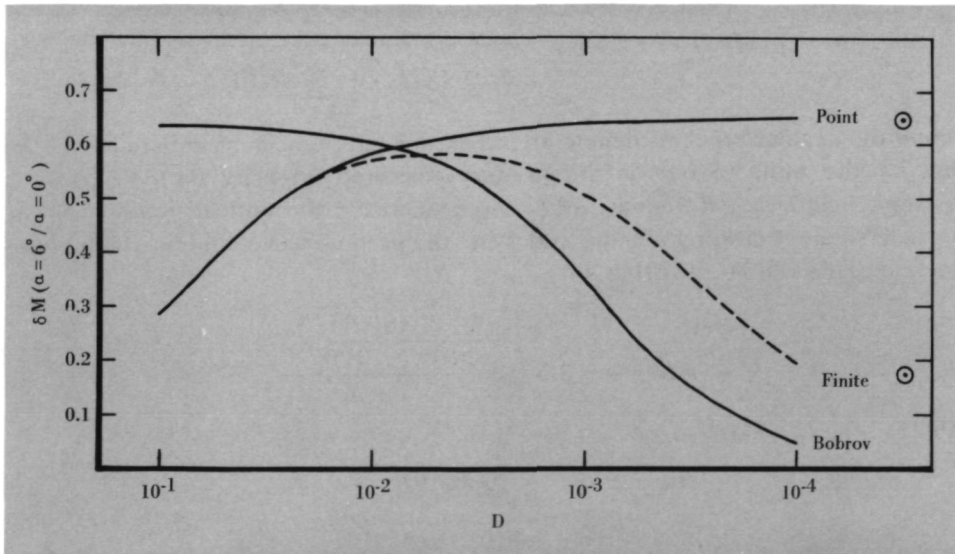


FIGURE 2.—Magnitude of the opposition effect resulting from particle shadowing for three cases as a function of volume density D , considering only single scattering.

It turns out that it is the volume density D which is the principal determinant of the shape of the opposition effect. As Dr. Franklin pointed out, we do get a pretty good handle on that and it turns out to be about 0.01 in these kinds of models.

However, we do find that, with that value of D which correctly reproduces the opposition effect, we cannot get the correct slope to the phase curve at larger phase angles by simply taking into account the primary scattering. We therefore have to go on and include the effects of multiple scattering and the effect of the phase function of the particles.

As I have said, the multiple-scattering computation is now a straightforward thing to do. The reflectivity of the rings will be given by the first-order scattering including the shadowing effect, plus the sum over higher-order scattering contributions where the shadowing effect is negligible. We find that the phase curve then depends on such parameters as the single scattering albedo of the ring particles, the optical thickness of the ring, the phase function of the ring particles, and volume density D , as well as geometric factors like the phase angle and the tilt of the ring with respect to the Sun and the Earth.

The specific intensity I of radiation reflected by the rings may be expressed as a sum of successive orders of scattering:

$$I = I_1^S + \sum_{n=2}^{\infty} I_n a^n \quad (1)$$

where I_1^S is the contribution from radiation scattered once, including the shadowing correction, and $a^n I_n$ is the contribution from radiation scattered n times. The predicted phase curve $M(\alpha)$ can be written as:

$$M(\alpha) = -2.5 \log \frac{a\Phi(\alpha) \langle S(\alpha) \rangle + \sum_{n=2}^{\infty} R_n(\alpha)}{a\Phi(0) \langle S(0) \rangle + \sum_{n=2}^{\infty} R_n(0)} \quad (2)$$

where the angular brackets denote an integration of the incident radiation over the disk of the Sun; $\langle S(\alpha) \rangle$ is the primary scattered intensity for conservative, isotropic scattering; R_n equals $a^n I_n$ for $\tau=0$ with τ the optical depth; a is the particle single scattering albedo; and $\Phi(\alpha)$ the particle phase function with phase angle α . This can be rewritten as

$$m(\alpha) = -2.5 \log \frac{\frac{\Phi(\alpha) \langle S(\alpha) \rangle}{\Phi(0) \langle S(0) \rangle} + X}{1 + X} \quad (3)$$

where

$$X = \frac{\sum_{n=2}^{\infty} R_n(0)}{a\Phi(0) \langle S(0) \rangle} \quad (4)$$

This is so because the higher-order scattering component of the intensity does not change rapidly with angle. The maximum phase angle observable for Saturn

is 6° , so, to a good approximation,

$$\sum_{n=2}^{\infty} R_n(0) \sim \sum_{n=2}^{\infty} R_n(\alpha) \quad (5)$$

The phase curve depends theoretically on the quantity X , which is the fraction of multiple scattering at opposition; that is, the brightness we see at opposition will include once-scattered light and multiply scattered light. Equation (3) turns out to be simple to deal with. We see that the shape of the phase curve will depend on the fraction of multiple scattering, the shape of the individual particle phase function, and the parameters that go into determining the shadowing effect in first-order scattering (principally the volume density and optical depth).

Those are the parameters which we will try to determine with this model, and, of course, there are a lot of parameters to play around with. Fortunately, we do have some other data, and particularly important in that regard are the data on the absolute brightness at opposition. I have been referring to this as the absolute brightness; in effect, it is the ratio of the brightness of the rings to the brightness of the disk.

There have been observations of the spectrum of the rings by Franklin and Cook (1965); Lebofsky et al. (1970); and Irvine and Lane (1973); and there have been some recent data by the Russian group, Kharitonova and Teifel' (1973). The spectral data are in pretty good agreement for wavelengths less than about 6000 \AA , particularly if we bear in mind the color dependence of the opposition effect. But strangely enough, the data are rather discordant in the red. In the red the observations of Irvine and Lane (1973), Lebofsky et al. (1970), and Kharitonova and Teifel' (1973) are all rather different from each other. I don't know what the explanation for that is; some of it may be due to differences in tilt during the observations, or there may be actual temporal variations due to such things as difference in the insolation or differences in the brightness of the rings on the east and west side, as has been reported consistently back through the literature. I don't know what modern observations there are, but it would be interesting to see if there is any spectral difference from one side of the ring to the other.

Let me go on and show you how we can try to match both the shape of the phase curve and the observed absolute brightness with our theoretical models. We can conveniently do that with the aid of figure 3. The vertical axis represents the primary scattered radiation, including the shadowing effect. The horizontal axis in figure 3 represents the sum of the higher-order scattering, which according to the model is $\sum_2^n R_n$. The dashed curves designated R , V , and B are the loci of points that satisfy the observed absolute brightness in the red, visual, and blue, respectively. An uncertainty of ± 0.05 has been shown for B to illustrate the effect of possible uncertainty in these measurements.

For a given D and τ , the shape of the phase curve $M(\alpha)$ depends principally on the fraction of multiple scattering X . By experimenting with a wide choice of values for these parameters, for the phase function Φ , and for single scattering

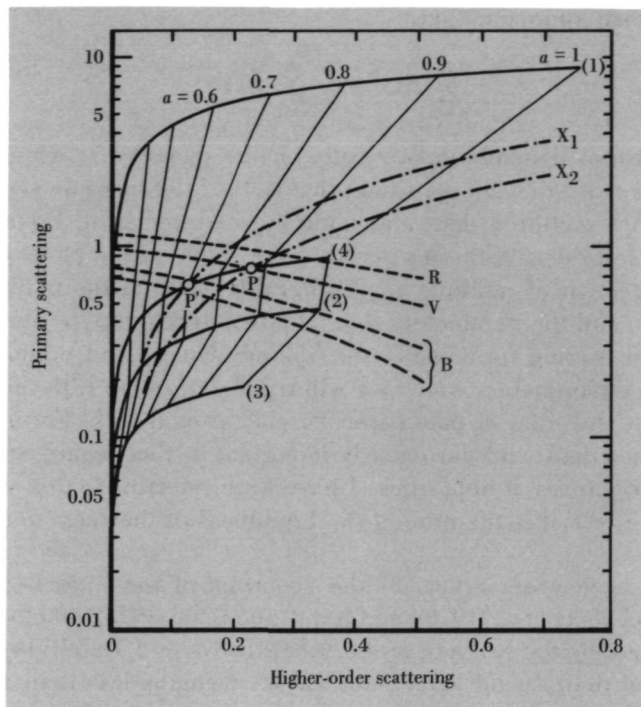


FIGURE 3.—Scattering diagram showing the relative amount of primary and higher-order scattering necessary to match the B ring brightness and the shape of the phase curve.

albedo a , we find that the sharp peak in the opposition effect depends primarily on the value of D , and that the observations restrict D to a narrow range around the value 0.01. Using this value of D , a τ of 1 on the basis of the observations of stellar occultations by the rings as discussed by Cook et al. (1973), and four assumed phase functions, the solid curves in figure 3 were constructed.

These phase functions, labeled (1) to (4), are shown in figure 4. They represent phase functions with the following characteristics: (1) a very strong backward peak with no forward scattering, (2) isotropic scattering, (3) a very strong forward peak with a small backward peak (reminiscent of the phase function for terrestrial clouds), and (4) a more slowly varying backward scattering phase function with a slight peak near 180° .

The straight lines in figure 3 connect points of constant single scattering albedo a and are labeled at the top of the figure. Once you have chosen an optical thickness for the rings and a volume density to more or less match the initial peak in the opposition effect, the parameter that determines the phase curve is the contribution of multiple scattering. The dot-dashed curves in figure 3 represent a match to the observed phase curves, X_1 in the blue and X_2 in the visual. The dot-dashed curves are the loci of points for which the fraction of multiple scattering is $X_1 = 0.17$ and $X_2 = 0.29$.

If we now call the intersection between curves V and X_2 in figure 3 a point P and the intersection between the middle of the range B and the curve X_1 a point P' , then the theoretically computed brightness curve for the rings that passes through both P and P' will match the observed absolute brightness and also the observed phase curves.

James Pollack What is the meaning of the numbers on the vertical and horizontal axes of figure 3?

Irvine The units are arbitrary because we are ultimately talking about a ratio of ring intensity to disk intensity.

Pollack How can the primary scattering be greater than 1?

Irvine Because it isn't strictly a fraction. There is a scaling by the geometric albedo of Saturn's disk.

The power of this procedure is illustrated by the large differences between the curves in figure 3. The requirement that the model match both the shape of the

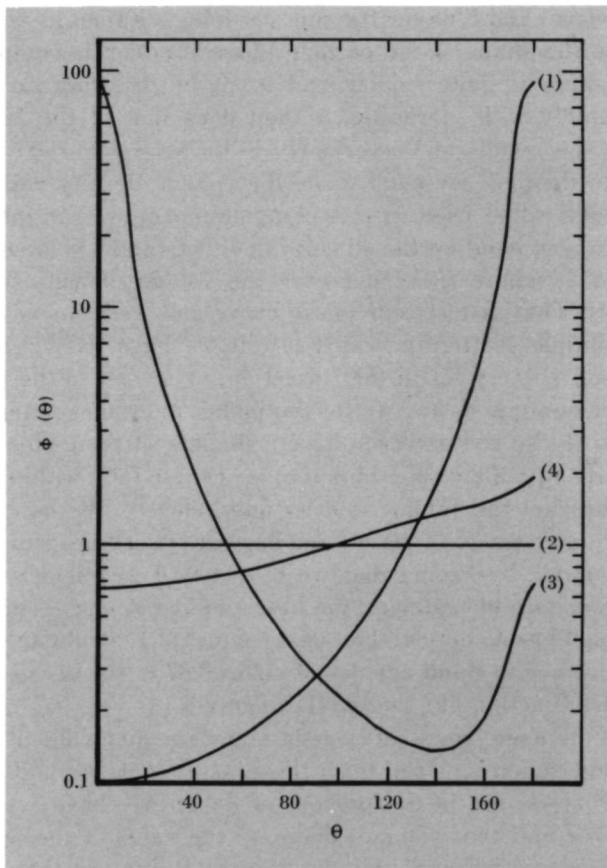


FIGURE 4.—Particle phase functions used to generate curves labeled (1)-(4) in figure 3. $\theta=0$ is the forward direction.

phase curve and the absolute brightness clearly puts significant restrictions on the form of the phase function. In particular, it is quite evident that neither the phase function with a very strong backward peak nor that with a very strong forward peak can match the observations. Some degree of backscatter is required to match the phase curve, so that the phase curve must be similar in shape to curve (4) in figure 3. Although the shape of the phase curve (apart from the opposition peak) depends principally on the values of Φ near 180° (corresponding to the small phase angles observable for Saturn), an appropriate phase function cannot be very different from curve (4). If it decreased much more sharply with decreasing α , it would not satisfy the normalization condition. The addition of a shallow forward peak to the phase function would be possible and would require a lower backward peak; that is, the phase function would become more isotropic.

We may now determine the single scattering albedo from the position of the points P and P' on curve (4) in figure 3. We find $a_V = 0.87$ and $a_B = 0.70$. By normalizing the phase function (4) to unity at $\alpha = 180^\circ$ and integrating, we may obtain the phase integral q for the ring particles as $q = 2.1$. The resulting geometric albedos in the visual and blue for the ring particles are then $p_V = a_V/q = 0.41$ and $p_B = 0.33$. When the shape of the particle phase function is compared to that of the Moon, the shape is quite similar near $\alpha = 0$, but the ring particle brightness falls off less rapidly with increasing α than does that of the Moon. This is in agreement with the results of Ververka (1973) for snow-covered objects.

Going back to figure 3, we can't make the volume density very much smaller than the particular value used in this computation or we run into the following problem: we are at a point on the shadowing effect versus volume density curve, shown in figure 2, where if we decrease the volume density we increase the shadowing effect. That distorts our phase curve and we have to modify it by increasing the multiple scattering, which means we have to increase the particle albedos. We can't go very far in that direction, or we get to the point where the new curves representing X_1 and X_2 for the higher multiple scattering don't cross the observations in the red until you have a single scattering albedo greater than 1, which is clearly a problem. We think that we can, in fact, within the parameters of this model, bracket the volume density quite closely. We can't go very far in the other direction or we don't get a large enough opposition peak.

Let me summarize by saying that we have found a satisfactory model using the classical procedure of matching the blue and visual phase curves and brightnesses. This model has an optical thickness τ equal to 1, a volume density of about 0.010, ring particles with Bond albedos of about 0.87 in the visual and 0.70 in the blue, and a phase function like curve (4) in figure 4.

None of that in itself necessarily tells you very much about Saturn's rings until you see how much you can push these parameters around and still fit the data. That is what we are in the process of doing. We have gone a little bit in this direction. We find that you can decrease the value of the optical thickness of the B ring (I am treating here only data for the B ring, which is most complete) to about a value of 0.7. That brings the volume density down a bit. It doesn't change the albedo very much, and it makes the phase function somewhat more

backward-scattering. It turns out that you can increase the optical thickness by a considerable amount, but the model is not sensitive to larger values of τ . It is hard to get an upper limit on the optical thickness using a model of this nature.

Let me make a final remark about particle size. If we took the monodispersed model literally, we could, given the optical thickness and the volume density, determine the mean particle size if we knew the physical thickness of the rings. If we take a physical thickness of the rings of 2 km, we come out with a particle size of about 15 m.

That may be misleading for reasons associated with what that physical thickness means, as well as with the limitations of the monodispersed model. So we modified our computation to include a distribution of particle sizes. Of course, once you allow for that, there are an infinity of possible particle size distributions you can use. We have looked at the distributions that Bobrov (1970) considered where the number of particles with radii between ρ and $\rho + d\rho$ is proportional to $K\rho^{-s}$. This is a distribution, according to Bobrov, which is used in meteor astronomy. We have looked at distributions that increased sharply toward the small particle end and also the uniform distribution, where s equals 0. The latter case doesn't change our results very much, at least in our initial experimentation. However, as you go to distributions that have more and more small particles in them, we find that if we take a distribution as steep as ρ^{-3} , we don't seem to be able to fit the data. With ρ^{-2} we can fit the data, and the mean particle size turns out to be considerably reduced.

In the case where you have a steep distribution of particle sizes, you must differentiate between the mean size, the root mean square size, the cube root mean cube size, and so on, all of which would enter a shadowing computation of this kind. Probably the most relevant is the root mean square particle size. We have matched the data with a model that has a volume density of the order 10^{-2} (5×10^{-3} up to 2×10^{-2}) and a root mean square particle size of around 50 cm. Whether we can push that down lower as some other considerations (see contribution by Pollack) would suggest is not entirely clear at this point. I think it may be difficult to push the root mean square particle size much lower than that. For the albedos, we get a somewhat greater range, but, in any case, it is clear that the particle albedo is high and wavelength-dependent.

In agreement with what Dr. Franklin said, it would be very desirable to get additional information on the phase curve and opposition effect of big, bright particles. I would think that, as Dr. Pollack pointed out, the Jovian satellites are potentially good examples, as are, perhaps, Iapetus and Rhea. It may be possible to do something in the laboratory with ice surfaces. It is very important to try to find the effect of albedo on the shadowing effect (the effect of the complex part of the index of a refraction). Perhaps one could do some experiments with ice, putting in varying amounts of dye to reduce the transparency of the ice, and see how the opposition effect is changed.

As I said in the beginning, I also think that a critical type of observation in distinguishing between models of the kind I considered here would be complete phase curves at the extreme wavelengths available from the ground in the red or

near-infrared and the ultraviolet. It is important to see whether the shape of the phase curve changes and just what the opposition effect is at those wavelengths.

Since I didn't bring it out in this discussion up to this point, perhaps I should emphasize that in the models we considered we have been assuming that the phase function for the particles was independent of wavelength; that is, as you change the albedo of the particle you change the reflectivity at all wavelengths by a constant factor. I believe, from looking at the Fresnel coefficients and from multiple scattering computations, that that will be approximately true for big, rough, bright particles as long as the absorption doesn't get too large, and it doesn't have to get too large to knock the albedo down considerably. It would be of interest as well to see if that assumption could be verified, perhaps in the laboratory.

DISCUSSION

James Pollack How much can we trust the measurements of the thickness of the rings? The reason I mention this is that in looking at the data as presented in Bobrov (1970b), you must extrapolate to the exact point of ring passage. There is no measurement per se at that exact moment. The two independent people who did this seem to get extrapolations that differ by a factor of 2, which makes me wonder whether there is any reality to what they did at all.

Hugh Kieffer I would like to complicate the issue, Jim (Pollack), a little bit. A prerequisite for this thickness measurement is the assumption that the edge-on outermost ring particles have properties that are somewhat similar to the central ring properties. That could be a terrible assumption if there is any kind of separation going on. If you throw in lunar-type material on the outside of the A ring for some type of screwy reason, you will be off by a factor of 10 in the ring thickness.

Pollack Fred (Franklin), do you think the measurements are real? Do you think we should believe them?

Fred Franklin I agree with what you say, but it seems to me it is hard to make them much thicker than the upper limit given of about 4 km. I have the impression that you could make them a good deal thinner.

Pollack Yes, that is really the point. I have no doubt that the upper limit is real. The point is, say in Bill's (Irvine) interpretation, if you make the rings a lot thinner then the particle size could be a lot smaller. That is why I asked the question. You think that is a possibility?

Franklin I certainly do.

William Irvine The parameter, which is the best determined by this approach, is the volume density. You can't push that around very much.

Robert Murphy Bill (Irvine), the albedos you are coming up with are awfully high. I will talk about that in my presentation. With those albedos there is no way to get the ring brightness temperature above 80 K which is in conflict with a number of observations in the infrared.

Irvine Including yours?

Murphy Yes.

Irvine Well, the original brightness temperature measurements gave values of about 80 K.

Murphy Allen and Murdock (1971) got about 83 K.

Irvine Okay, what do you want? Will you quote a value for the temperature at this point?

Murphy Well, I will talk about it this afternoon. In the context of this particular paper, I would like to point out that these albedos are very difficult to handle in the context of other existing data. They have to be somewhat lower.

Irvine How low do you feel they would have to be?

Murphy The values that Cook et al. (1973) have talked about, on the order of a Bond albedo of 0.6.¹

Irvine Well, of course, that is a bolometric Bond albedo.

Murphy Right.

Irvine We would have to do the appropriate integration to get that from the values of Bond albedo I have given. But even 0.6 is fairly high in the scale of the solar system.

Dennis Matson This is in regard to Bob's (Murphy) remarks on temperature. There is an emissivity parameter that enters into these models. The temperature you get is also dependent on what you are assigned for that. I do not view the infrared data, at its face value, as being inconsistent with higher albedos. There are a number of model parameters still free at this time.

Franklin If you did have a ring model in which you had essentially a monolayer of large particles in contact, or however you would like to visualize it, do you feel that you could match the observed photometry? You have parameters to play with—surface structure, phase function, and so on. Does a monolayer have a photometric interpretation that is reasonable, or do you think that is essentially ruled out?

Irvine Well, as you know, there isn't at this point any really accurate theoretical way to treat the opposition effect in a single particle, so it is difficult to answer your question. I don't think one can answer your question theoretically. One would have to try to look at laboratory data. As you pointed out, the data seem to indicate that you don't get as large an opposition effect, as you observe in the rings, with bright samples from the laboratory. I don't think the data published so far are very complete. It would be useful to have a more systematic investigation.

Brad Smith Bill (Irvine), is there any way of using the changing ring tilt that will take place in the next half-dozen years or so to distinguish between the monolayer and your model?

Irvine Well, we can certainly make predictions for what you ought to see with changing ring tilt with our model. I think to get good observations of that effect would be very valuable. I don't know how you would predict what you ought

¹Editors' note: Murphy's view has changed, somewhat softening his objection to Bond albedos greater than 0.6. See footnote in discussion following his contribution.

to see for the monolayer. I haven't given that enough theoretical thought and I don't know that anyone else has, either.

Franklin But you have no arguments against it?

Irvine What worries me about a monolayer is that it is difficult for me to visualize a monolayer that has an optical thickness of unity and anything like the thicknesses that we have been talking about for the rings, for then you have got great big particles that are essentially, as Dr. Franklin said, rolling over each other, at least in the thicker parts of the B ring. Maybe it doesn't appeal to me esthetically. It is a little hard to visualize. I think it might be interesting to try to look at a model like that dynamically. You will certainly have collisions which would tend to reduce the angular momentum of the individual particles, and I would think you would eventually get them spiraling inwards. It might be interesting to see if you could establish the time scale.

Smith But you eliminate those collisions which are required by the particles passing through the ring place twice every revolution.

Irvine Right.

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Frank Don Palluconi

SATURN'S RING PARTICLES AND SPACE VEHICLE DESIGN

I will discuss two primary areas: design, as Saturn's rings affect the mission and spacecraft, and ring particles, with particular emphasis on material outside of ring A and the hazard this might imply to spacecraft crossing the ring plane beyond ring A.

MISSION AND SPACECRAFT DESIGN

There are a number of factors that affect mission design: the scientific objectives, the capabilities that can be built into the scientific instruments and built into the spacecraft, and the natural environment. In some cases potential hazard from the natural environment has an effect on trajectory selection. In the case of the MJS project, at least in the initial specification, the trajectories have been constrained to pass no closer to the planet nor to cross the ring plane more closely than 4 Saturn radii. This points up why a description of particulate matter beyond ring A is important. The opportunities that are available as a function of trajectory will be discussed by Dr. Penzo (see following contribution).

In spacecraft design, there are three areas where Saturn's rings may have some influence: thermal control, celestial sensors, and particle shielding.

With respect to thermal control, the existing information we have about the amount of radiation emitted at thermal wavelengths from the rings, the scattered light from the rings, and, of course, the dimensions of the rings makes it possible to decide what the thermal input to the spacecraft from the rings will be. Preliminary calculation indicates the thermal input will be small compared to the heat budget of the spacecraft itself, and so it is very likely that Saturn's rings themselves, at least from a thermal standpoint, will not have a direct impact on thermal control. In particular, the spacecraft is to be designed so that it operates independent, from a thermal-control standpoint, of the environment. The thermal

design is really set by the extremes in the natural environment, which occur near Earth just after launch and near Saturn prior to encounter.

Scattered light from Saturn's rings is a problem for celestial sensors. Two sensors are used for providing attitude reference: a solar tracker and a Canopus tracker. The solar tracker will not be affected by scattered light from the rings. It will only be affected by occultation of the Sun by the planet or by the rings, and in this case an inertial reference will be provided. The Canopus tracker is more sensitive with respect to scattered light. The information that exists from ground-based observations is sufficient for design of this tracker. In this case, as well, if the rings intrude too far into the field of view, inertial reference will be provided.

With respect to both of these areas, thermal control and celestial sensors, information about the basic properties of the bright rings, which already exists, is probably sufficient to come up with a good design.

At this time there is no plan to burden the spacecraft with shielding, which is primarily intended to provide protection against Saturn ring particles. Our understanding and modeling of material beyond ring A is sufficiently poor that this kind of exercise would not be profitable. Rather, the emphasis has been placed on selecting a judicious trajectory, which minimizes in itself the hazard to the spacecraft. This doesn't mean, however, that the spacecraft won't be provided with some shielding that would be effective against particles, including any encountered near Saturn.

There are a number of ways in which this will come about. The first and most obvious is the basic structure of the spacecraft, which will provide some shielding both for subsystems and science instruments. In addition, two natural environments, interplanetary meteoroids and charged particle radiation from the Jovian radiation belts, solar-type protons, and onboard radiation sources may require shielding, which would also be effective against particles. There are also several additional problems—producing an electromagnetically clean spacecraft and accounting in some way for the possible effects of differential electrostatic charging—that may require design features which would be effective in producing some kind of shielding.

PARTICLES BEYOND RING A

Turning to Saturn's rings themselves, table I presents the dimensions of the system. I would like to use this table not so much to discuss the dimensions of the ring system, which come from a compilation by Cook et al. (1973), but rather to discuss the regions of Saturn's rings where there are thought to be relatively few particles and which are possible candidates for crossing by a spacecraft. The three regions that one might initially consider are the ring D region, named following a suggestion by Guerin (1970), Cassini's division, and the entire region outside of ring A, which I have labeled the D' region in order to maintain some connection with previous terminology.

A number of estimates for optical thickness have been made for these regions. One I would like to mention in particular was based on an analysis by Cook

TABLE I.—Saturn's ring dimensions.

Ring	Feature/ring boundary	Distance from center of planet, km	Nominal distance in equatorial radii, R/R_s
Ring D	{ Equatorial radius	59 800 ± 350	1.00
Ring C	{ Inner C boundary	72 000 ± 3500	1.21
Ring B	{ Inner B boundary	91 400 ± 700	1.53
	{ Outer B boundary	116 700 ± 700	1.95
	Width of Cassini's division	4 800 + 1400 - 2800	
Ring A	{ Inner A boundary	121 600 ± 700	2.03
	{ Outer A boundary	137 100 ± 700	2.29
D' region	{ Outer limit to D'ring region	239 200 — 358 800	4-6

and Franklin (1958) of observations made by Barnard of the shadowing of Iapetus by ring C. This led to a deduction for the optical thickness of ring C, including its inner boundary, closer to the planet than any condensations have been noted during ring plane passage. At the inner edge of ring C, the optical thickness was felt to be small, on the order of 10^{-2} or so. This certainly represents an upper limit to the optical thickness of the material within the ring D region and very likely is a conservative upper limit for the optical thickness of any material outside ring A.

So far as I know, there has never been any serious consideration of trajectories that pass either through the Cassini division or the ring D region, apart from some suggestions made at the time the Grand Tour missions were considered. The prime purpose of passages through Cassini's division or ring D was to reduce the transit time to planets beyond Saturn.

The primary interest with respect to the MJS project is in the region outside ring A extending to $4 R_s$ or $6 R_s$ or beyond, where there is some evidence for material.

I would like to discuss essentially the totality of information that exists with respect to material beyond ring A. There are a number of visual sightings of a narrow ring just outside of ring A; two sequences of photographs that show the presence of a line representing, perhaps, extended ring material; and the recent radar detection of Saturn's rings.

The visual sightings were reported primarily during two periods of time. The first, in the period 1907-1909, followed the 1907 ring plane passage of the Earth across the ring plane (Alexander, 1962, chapter 28). The second followed the 1950 passage of the Earth across the ring plane in the years 1952 and 1954. The earliest

reports indicated a narrow ring just outside of ring A. It was seen both in front or near the planet and in the ansae. There were some anomalies with respect to these observations. The ring width deduced from observations near the planet did not correspond to the width in the ansae. The suggestion made at that time was that perhaps this was evidence for extraplanar particles.

The second set of observations refers to the period following the 1950 ring plane passage, during which a number of observers reported seeing material just beyond ring A. For the most part, these observations and the earlier ones were made with small telescopes less than 16 in. in diameter, although during 1952 there was one observation by Cragg (1954) using the 60-in. reflector at Mt. Wilson. Cragg reported a narrow ring immediately adjacent to ring A, with a width of about 6000 to 10 000 km and an overall brightness about one-half the brightness of ring C.

There have been negative findings during both these time periods as well. Barnard, aware of the reports of a narrow ring outside of ring A, used a 40-in. refractor to carefully scrutinize the region just outside ring A in about 1909 and concluded that he could find no evidence whatsoever for this external ring. A similar occurrence happened in 1954 when—I think within about a month of one of Cragg's observations—Kuiper (1973) made observations with an 82-in. reflector and with the 200-in. reflector, again carefully scrutinizing the region just outside ring A. He was able to find no evidence for an exterior ring and felt from at least the 82-in. observations that the brightness of any material had to be less than $1/40$ the brightness of ring C.

There is also a point I would like to make with reference to work done by Franklin et al. (1971). They looked at the properties of the rings from a dynamic standpoint, considering the perturbing effect of Saturn's satellites. In this work they were able to show that it is possible for relatively stable particle orbits of small eccentricity to exist in the region just outside of ring A. I would like to emphasize, however, that the existence of such stable orbits does not imply anything about their being populated by particles.

The most extensive evidence for material beyond ring A consists of photographs. I would like to defer discussion of those for a moment and make one point about the recent radar detection of Saturn's rings, which will be considered in more detail by Dr. Morris (see contribution by Morris). There is a low doppler shift portion to the radar return. Among several explanations for this low doppler shift portion, and probably not the most likely, is that it refers to reflections from particles that are orbiting outside of ring A. The point I would like to make is that those observations don't in themselves imply or place any requirement that this material be in the same plane as the principal rings. I think it is useful in the context of this workshop to at least entertain the suggestion that there may be particles with nonzero inclination. This aspect is particularly relevant in determining the hazard from external ring material.

So far as I know, there are two sets of observations in which the photographs taken show a narrow spike extending outside the bright rings. These observations were made by Feibelman (1967) and Kuiper (1973 and 1972). A brief summary of this material is given in table II. Both sets of observations refer to the same

TABLE II.—Summary of photographs of extended ring material.

Observer	Telescope	Observing period	Exposure time	Radial extent	Brightness, mag/sq sec of arc	Effective reflecting area
W. A. Feibelman	30-in. refractor	October, December, January 1966–1967	5–30 min	$> 4 R_S$	≈ 15	—
G. P. Kuiper	61-in. reflector	October, December, January 1966–1967	10–60 s	$\approx 6.3 R_S$	≈ 15 Definite decrease with increasing radial distance	$3 \times 10^5 \text{ km}^2$

time period—the 1966 passage of the Earth across the ring plane—and in particular to the observing period of October, December, and January. Between about October 29 and December 17, the Earth and the Sun were on opposite sides of the ring plane, and on December 17 the Sun and the Earth returned to the same side of the ring plane.

Feibelman designed his program specifically to search for the existence of external ring material. He used a 30-in. refractor and very long exposure times, ranging from 5 to 30 min. He did not publish or attempt to publish reproductions of his photographs. Rather, he made photodensitometer tracings across the ring plane on several photographs and felt that he could see a definite indication of material or an increase in density of the negatives as the ring plane was crossed. He felt that this material extended as far as 4 Saturn radii, and perhaps beyond. He also estimated the brightness of this faint line at something like 15 magnitudes per square second of arc. He made no estimate of the reflecting area that could be inferred from this type of observation.

Dr. Kuiper's observations were made at the same time using a 61-in. reflector. He used much shorter exposures, ranging from 10 to 60 s, and felt he could detect the presence of this ring somewhat farther out than Feibelman, perhaps to the orbit of Dione or to about 6.3 Saturn radii. His estimate of the brightness of the faint extension was essentially the same as Feibelman's. In addition, he was able to detect a definite decrease in brightness with radial distance from the planet and a definite thickening in this faint extension as the inclination of the observer above the ring plane increased from 0.1° to 0.5° . He also estimated the total line brightness and, based on an assumption of the albedo, deduced an effective reflecting area for this material of something like $3 \times 10^5 \text{ km}^2$. This material is essentially in the same plane as the principal rings of Saturn.

Brad Smith How does this brightness compare with the brightness, say, of ring B or ring A?

Palluconi It is appreciably fainter.

I would also like to mention at least one negative photographic observation. Rosino and Stagni (1969) made extensive observations of the Saturn ring system at the time of ring plane crossing in October, December, and January 1966–1967. They were aware of Feibelman's report and made photodensitometer tracings normal to the ring plane at about the same distance that Feibelman did, between 3 and 4 Saturn radii. They were unable to detect the presence of any external ring material. In addition, Focas and Dollfus (1969) and Sekiguchi (1968) have taken an extensive number of photographs at this time, and, so far as I know, they have not reported detecting any exterior ring material.

Smith I would like to add another negative observation. This was made in New Mexico in 1966 at the time of the crossing through the ring plane, and, although I haven't yet worked this out in terms of magnitude per square second of arc, it must be something on the order of 15.

Dr. Kuiper has kindly supplied me with a copy of the manuscript which contains reproductions of his photographs and in addition a copy of a "Lunar and Planetary Laboratory" publication which also contains some reproductions, which I will make available to the participants of this workshop.

I would also like to mention a brief program carried on at JPL for the purpose of trying to place some kind of limit on the amount of material that may exist outside ring A at the time when the rings are fairly wide open. This is a much more difficult observation, and for this purpose a silicon-imaging photometer was used. This instrument has several advantages. In particular, its large dynamic range and linearity make calibration with respect to the planet and bright rings relatively easy. The digital form of the output makes it susceptible to computer manipulation and makes possible an attempt to eliminate the effects of scattered light from Saturn and the bright rings.

One thing which I don't think the photographic evidence makes particularly clear is the radial distribution of material. That is, it is possible that the photographic observations could be adequately represented by one or more narrow rings with, perhaps, one extending to 6 or so Saturn radii. If in fact the density in narrow regions were appreciably above that implied by these observations on the average, then making observations when the ring plane is fairly wide open might have some value. I think it would be appropriate in the discussion session to be held tomorrow to consider continuing or extending these observations.

THE HAZARD FROM PARTICLES OUTSIDE RING A

What I would like to do next, particularly with respect to the estimate of reflecting area made by Dr. Kuiper, is to consider a simple framework that enables one to make a rough estimate of what the hazard might be in crossing the ring plane with this amount of material.

I have constructed a very simple framework that enables one to estimate the hazard:

α \equiv fractional area occupied by particles

πr^2 \equiv area of particle

A \equiv effective area of spacecraft

θ \equiv inclination of trajectory to ring plane

$$\frac{A \cdot \alpha}{\sin \theta \pi r^2} = \text{number of impacts}$$

The first quantity is α , which I have taken to be the fractional area occupied by the particles in a planar geometry when you view the ring normally. It can effectively be equated with the optical thickness. Next, I have assumed that the particle area can be represented by πr^2 , where I have taken the particles to be a single size for any particular consideration. This assumption, of course, is wrong. One would expect that there would be a dispersion in particle size, in which case it would be important to know what that dispersion was in order to use the appropriate moment. In addition, I have identified A as the effective area of the spacecraft and θ as the inclination of the trajectory to the ring plane, measured from the ring plane. With those four quantities, one can very simply estimate the number of impacts. The quantity I have indicated by $\alpha/\pi r^2$ represents the surface density of particles, and A divided by $\sin \theta$ simply gives the effective area swept out by the spacecraft in crossing the ring plane.

Figure 1 utilizes this expression and gives the number of impacts as a function of particle radius for a number of values of α . The number of impacts is displayed on the ordinate. The abscissa indicates the particle radius, ranging from a millimeter to a meter. For the purpose of being explicit with respect to numbers, I have made two choices, one with respect to the area of the spacecraft, which I have taken at 10 m², and the other the angle of inclination, which I have taken to be 90°. If, for example, one had a trajectory that was inclined only 1° with respect to the ring plane, the number of impacts would increase by approximately a factor of 55 over those shown.

I have also indicated by the horizontal dashed line the probability of no impact, which is constant for a given number of impacts. It is essentially the exponent of the number of impacts. If you have a description of a potentially hazardous natural environment, it is common to decide on some other basis what constitutes an acceptable level of risk, perhaps expressed as a probability of no impact, and then look at the environment and develop a strategy based on what the probability of no impact is for a particular set of assumptions.

What I would like to do is take the estimate which Dr. Kuiper made of 3×10^5 km² as the effective reflecting area represented by this observation and uniformly distribute that material from the outer edge of the A ring to 6 Saturn radii. If we do that, the value of α , or the optical thickness, is on the order of 10^{-6} . As can be seen from figure 1, there may or may not be a significant hazard depending on the dominant particle size. The point I would like to make with this figure is that

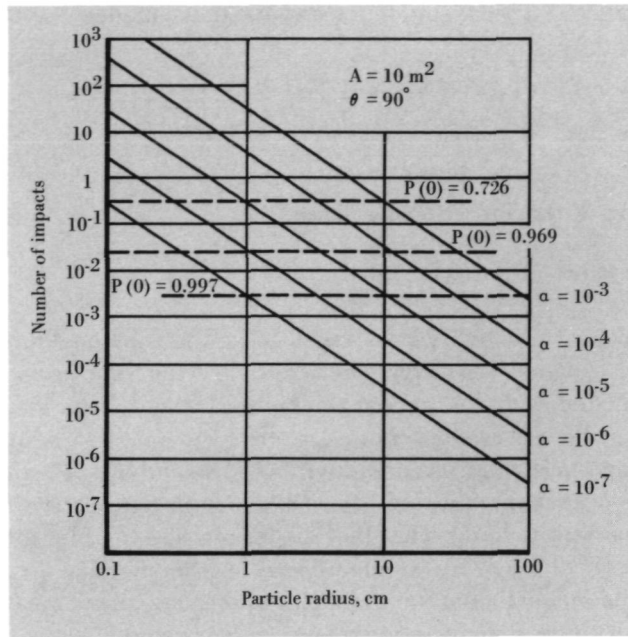


FIGURE 1.—Number of impacts vs particle radius. Solid lines are labeled by fractional area occupied by particles, α ; dashed lines are labeled by probability of no impact, $P(0)$.

it is not sufficient in itself to have a very small optical thickness; one also needs to know or make some argument about the particle size.

Three kinds of descriptions of the particles outside ring A can be made, any one of which would be of appreciable help in making an assessment of the hazard.

The first one is an assessment that there is no material there or an insignificant amount of material. For purposes of this workshop, I think that it would be well to consider the possibility that the whole of the existing observational information could in fact have some alternate explanation which does not require material or particles outside of ring A.

A second kind of description would be partial. This is a less detailed solution but nevertheless one which presents some important information. It could take, for example, the form of regions to be avoided. The region immediately outside of ring A, for perhaps $0.1 R_S$, might be one such region. The converse would be regions that are safe. In that regard, crossing at the orbit of one of the inner satellites of Saturn might be such a possibility. Mimas, which has the largest eccentricity of the five inner satellites—excluding Janus—is a candidate.

The third case would be a full model or a full description of the particulate matter found in this region. This would basically consist of an estimate of the spatial density of particles, and, if it were felt that there was a possibility that not all the particles were in ring plane, this would include the spatial density as a function of inclination. In addition, it should contain the dispersion in particle size and an estimate of particle density.

The final point I would like to make is that, with respect to the whole problem of the existence and form of material beyond ring A, any contribution this workshop can make in one form or another to a description of this material will be helpful not only to outer planet projects in general but of particular importance to the MJS project at this time.

DISCUSSION

Brad Smith Did you say that you have made or are going to make vidicon observations?

Frank Palluconi I think the word is "have made." We had three nights in the winter of 1973. These observations were made under the direction of Dr. A. Goetz from Mt. Wilson using the 60-in. telescope and an instrument that had been put together at JPL. We had a significant problem: interference from the many transmitters on Mt. Wilson produced records that were unusable for the purposes that we had intended. This problem has been corrected by readjusting grounding points, and hopefully this is now a relatively insignificant problem.

Smith So you can't give any upper limits of the brightness of this D' ring relative to ring C or ring B at this time?

Palluconi Not based on those observations.

William Irvine Do I understand, Brad, that your observations are not necessarily inconsistent with Kuiper's—that they are at about the same level?

Smith Well, I would have to see what our limiting magnitude is, but I would estimate that it is of that order. But, of course, a magnitude one way or the other could make a lot of difference. We found no evidence whatsoever of any extension beyond the edge of ring A. Furthermore, we have done some preliminary photometry on photographs that were taken last year. Photographs being so nonlinear and having such a small dynamic range, it is difficult to put upper limits on the brightness of the so-called D' ring. We are still working on it, and our first guess is that the brightness cannot be any greater than 1 percent of the brightness of ring C at the distance of Mimas.

Robert Murphy Don't you have to work at about 20 magnitudes per square second of arc or less in order to do this properly now because of the tilt change?

Palluconi Yes, as I mentioned, it is very much more difficult. If the observations in fact represent uniformly distributed material, it would be extremely difficult if not impossible to set a useful upper limit. I think the value of observations at present is in answering the question, "Are there any narrow rings of increased spatial density which would represent regions to be avoided?" In that regard the observations might be quite helpful. If one could really effectively handle the scattered light problem it would be possible to make observations that refer to brightness levels limited only by the sky background. That's where we would like to be able to work. The problem with many of the photographs already in existence is that scattered light from Saturn's rings and from Saturn itself precludes setting very restrictive limits on the brightness from this exterior region.

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Paul A. Penzo

IMPACT OF SATURN'S RINGS ON MISSION ANALYSIS FOR MJS 77

Figure 1, which is drawn to scale, shows a heliocentric plot of three possible MJS 77 trajectories as seen from the ecliptic North Pole. This figure is presented to explain the spacecraft approach direction to Saturn. Although a single launch date is shown, there is a 32-day period from August 17 to September 17, 1977, during which the two Mariners can be launched. Varying the launch date will not significantly change figure 1. All trajectories currently acceptable to MJS 77 lie between the extreme trajectories shown, essentially in the ecliptic plane, with Jupiter arrival dates between February 21 and July 24, 1979, and Saturn arrival dates between October 28, 1980, and September 21, 1981. Thus, the possible arrival dates at Saturn span almost a year.

During this time the view from Earth shows the rings tilted downward from 5° to 10° . Also, at large distances the approach of the spacecraft to Saturn for the range of arrival dates is essentially from the direction of the Sun, and this direction remains fairly constant. Thus, the spacecraft, on approach to Saturn, will see the planet and rings fully lit with the rings tilted down about 10° . The characteristics of the near encounter of the Saturn flyby—that is, altitude and latitude passage at closest approach—depend upon mission constraints and scientific objectives. Thus, it is possible to pass over or under or to the left or right of the rings. Analyses are currently in progress to relate these flyby characteristics to specific science objectives and thus determine what specific trajectories should be flown. It should be noted, however, that it will be possible to alter the Saturn flyby to some extent, such as from passing close to the rings to passing further out, up to 2 or 3 months before the spacecraft arrives at Saturn. Some of the mission objectives at Saturn currently being considered—which affect the trajectory design and in particular the aim point at Saturn—are shown on the following page:

Earth, Sun occultation of Saturn, rings, and satellites

Close as possible to surface

Close as possible to rings

Close encounter with Titan (~20 000 km)

Close encounter with Iapetus

Multiple satellite encounters

Post-Saturn trajectory to Uranus

Post-Saturn trajectory toward solar apex

Going over these objectives briefly, Earth and Sun occultation by Saturn is easily obtainable and does not greatly restrict the selection of the Saturn aim-point. The best Earth occultation by the rings, however, requires a passage slightly above the ring plane, assuming direct passage of Saturn. This is somewhat incompatible with flying as close as possible to the surface and the rings,

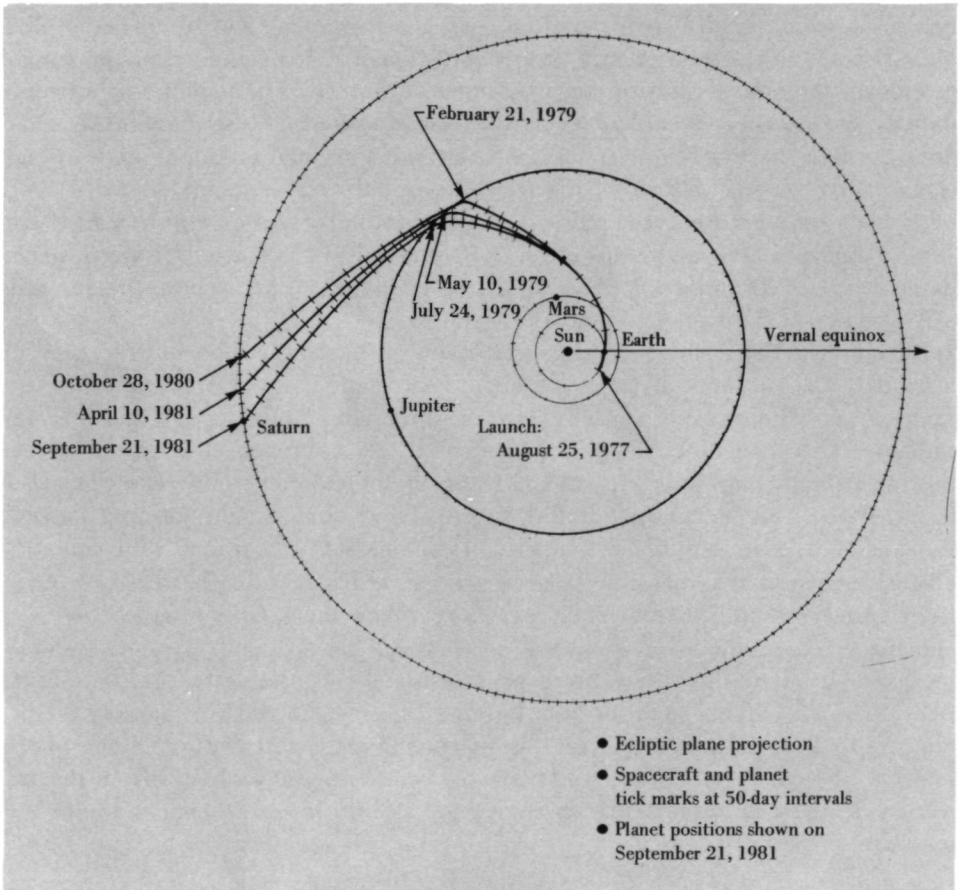


FIGURE 1.—Heliocentric plan view of transfer trajectories.

which is best accomplished by flying slightly below the rings. This latter requirement assumes a ring plane crossing-distance constraint (i.e., that the spacecraft must cross the ring plane no closer than some specified distance, now taken as 4 Saturn radii).

Another objective would be to pass close to Titan or Iapetus, say within 20 000 km, which would place restrictions on the Saturn aimpoint. There is some freedom here, since the encounter may take place with Titan anywhere in its orbit, within limits. If, in addition, it is desired to come close to Saturn, then the Titan encounter occurring before Saturn passage must pass far underneath the rings, whereas the Titan encounters after Saturn passage may pass closer, but still underneath the rings. The desire for multiple satellite encounters, say to get within 20 000 km of Titan and within 50 000 or 100 000 km of an inner satellite such as Dione, would further constrain the aimpoint and the Saturn arrival time. Requiring the spacecraft to continue on to Uranus after the Saturn encounter places a very strict requirement on the Saturn aimpoint. Here, it is necessary to remain in the ecliptic and fly very close to Saturn, perhaps through the visible rings. The final objective listed here is a post-Saturn trajectory aimed toward the solar apex, requiring the spacecraft to come up out of the ecliptic to an ecliptic latitude of about 25° to 50° and a longitude of 270°.

Relative to these objectives, the general regions of the aimpoints are shown in figure 2. The dashed line represents the ring plane, and the location of each region is about equal to the distance of closest approach to Saturn. The number

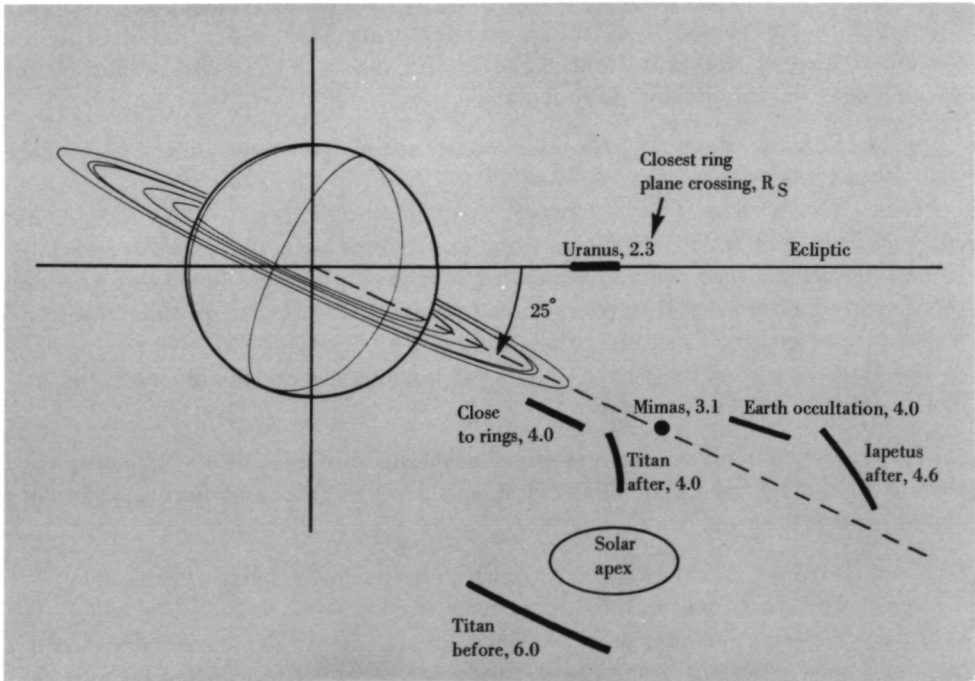


FIGURE 2.—Mission objectives at Saturn and relation to rings.

shown next to the stated objective is the closest ring plane crossing distance. For example, if we wish to come close to the rings, we have to aim the trajectory underneath the ring plane. This would be the ideal way of getting close to the rings and remaining outside the $4.0 R_S$ distance at ring plane crossing. If we wished to continue on to Uranus, we would have to pass Saturn well above the rings and remain essentially in the ecliptic. It is necessary to remain in the ecliptic in order to continue on to Uranus, which is very near the ecliptic. Also, it would be necessary to violate our current $4.0 R_S$ ring plane crossing constraint and cross the ring plane at about $2.3 R_S$, just at the edge of the visible rings. This close approach to Saturn is necessary to cause a sufficient amount of bending by Saturn in order for the trajectory to continue in the right direction to get to Uranus.

Another objective shown in this figure is Earth occultation. This means that, as viewed from the Earth, we wish to have the spacecraft pass behind Saturn and each ring in the ring system. Data about the rings may then be obtained from the received radio signal. The best ring occultation results when the spacecraft passes Saturn slightly above the ring plane. It is interesting to note here that the closest approach distance to Saturn that will satisfy the $4.0 R_S$ ring plane crossing constraint is further out from Saturn than when passing underneath the rings. This is a general property of the Saturn flyby trajectories—that you can get closer to Saturn by flying under the rings than flying over them.

The “Titan after” label simply means that this is the general aim region required to obtain a close encounter with Titan after the Saturn flyby has occurred. The time of Titan encounter is about 18 hr after Saturn passage. Encountering Titan before the Saturn flyby means encountering Titan when Titan is in the portion of its orbit that is in front of Saturn. As shown in the figure, this requires passing considerably below the ring plane.

Von Eshleman Paul (Penzo), this chart seems to make things look more mutually exclusive than they really are.

Penzo That’s right. This is more of a schematic diagram to show the general regions. I did not try to show the complete regions with the possible overlaps, since this can get to be very complicated. For example, there is overlap between the Earth occultation and encountering Iapetus after the Saturn encounter and also between getting Titan after the encounter and passing as close to the rings as possible. However, the further apart two regions are on this diagram, the less likely it is that an overlap exists.

Finally, the aim region for passing out in the direction of the solar apex is shown. Flying by Saturn in this region would result in a post-Saturn trajectory aimed toward the solar apex.

Robert Murphy What is the advantage of going to the solar apex?

Penzo The particles and fields people are interested in seeing where the boundary between the solar particle field ends and where the interstellar medium begins. There are measurements that they wish to take in that region to define that boundary.

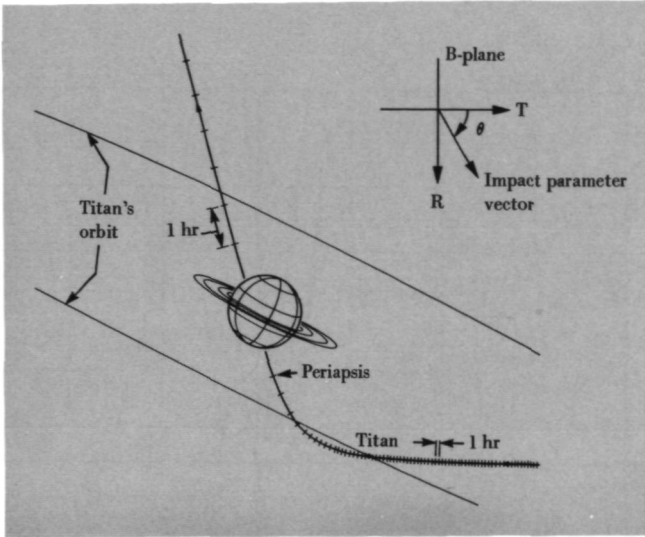


FIGURE 3.—Sample flight path by Saturn.

Figure 3 simply shows one trajectory satisfying a particular objective, which is to encounter Titan. In this case the trajectory begins at the lower right. The tick marks indicate fixed time intervals, so that, as you tick off each hour, the trajectory approaches Titan, then encounters Titan. Near the Titan encounter, it crosses the ring plane at $19 R_S$ and then continues its passage underneath the ring plane. Saturn occultation occurs just about 1 hr past periapsis, with exit from occultation occurring about 2 hr later. The second ring plane crossing, from below to above, might be at 6 or $8 R_S$.

I want to point out what is called the impact parameter plane. This is simply a plane that is perpendicular to the incoming velocity vector, with Saturn at the center of the coordinate system. Neglecting Saturn's mass, the velocity vector from a very large distance is just extended along a straight line until it passes through this plane. The vector from the center of Saturn to this intersection is called the impact parameter. The T axis represents the ecliptic direction—that is, the intersection of the impact parameter plane with the ecliptic plane. The angle θ is measured from this line to the impact parameter vector in a clockwise direction and is somewhat equivalent to an inclination. That is, as you move clockwise from the T axis, you measure θ from 0° to 360° as indicated in the small sketch in figure 3.

Figure 4 shows a multiple satellite encounter analysis, with the X axis indicating the Saturn arrival date resulting in a Titan encounter at various points on its trajectory. There will be certain regions—certain locations of Titan on its orbit—where the trajectory will encounter not only Titan but also other satellites. The Titan closest approach altitude for this complete figure is 25 000 km. So in addition to Titan, you can also, for example, get close to Dione as shown. The distance of Dione will minimize at about 75 000 km when you arrive at

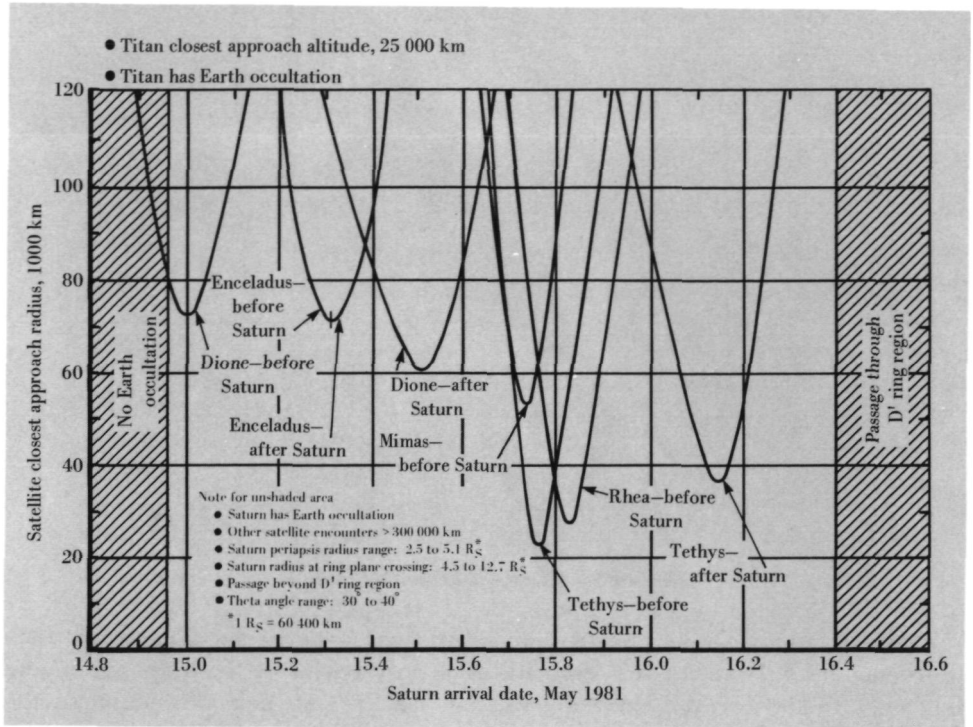


FIGURE 4.—Saturn satellite encounter opportunities.

Saturn on May 15, 1981. Here, you will encounter Dione before the closest approach to Saturn. As you change the date of arrival at Saturn and maintain the Titan encounter, other satellites come into the picture, and some of them bunch up at certain arrival dates. For example, on May 15.8 you can get within 40 000 km of Tethys and Rhea and 90 000 km of Mimas. Therefore, you can actually come close to three of the inner satellites in addition to coming close to Titan. We wish also to have an Earth occultation of Saturn, and that implies you do not want to arrive at Saturn prior to about May 15. If you encounter Titan before that date, as shown in this figure, you do not get Earth occultation.

As the arrival date increases, the closest approach altitude at Saturn decreases. So that as the date gets beyond 16.4 in May, you must pass through what we call the D' region, which extends to 4 R_S in the ring plane. You can see that the D' ring constraint plays a definite part in selecting trajectories that encounter satellites. Figure 4 applies to a single revolution of Titan. Previous or succeeding revolutions will have different characteristics relative to the secondary satellite encounters.

Another science objective is obtaining an Earth occultation of Saturn's rings. Figure 5 is a view of Saturn as seen from the Earth. In this case the equatorial plane is horizontal, and, as seen from the Earth, the spacecraft will pass behind the planet and the rings from right to left as shown. Periastron would be close to the right edge of the figure. The hours past periastron are marked. Here you can

get occultations of rings A, B, and C and the gap between ring C and Saturn. As I mentioned in an earlier figure, this requires approaching Saturn from above the ring plane, whereas the Titan encounter requires approaching Saturn from below the ring plane, at least for the case where you get to Titan before you arrive at Saturn.

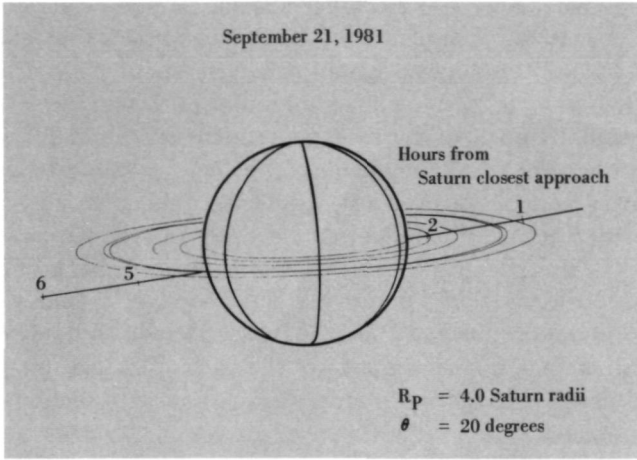


FIGURE 5. — Saturn trajectory for good ring occultation.

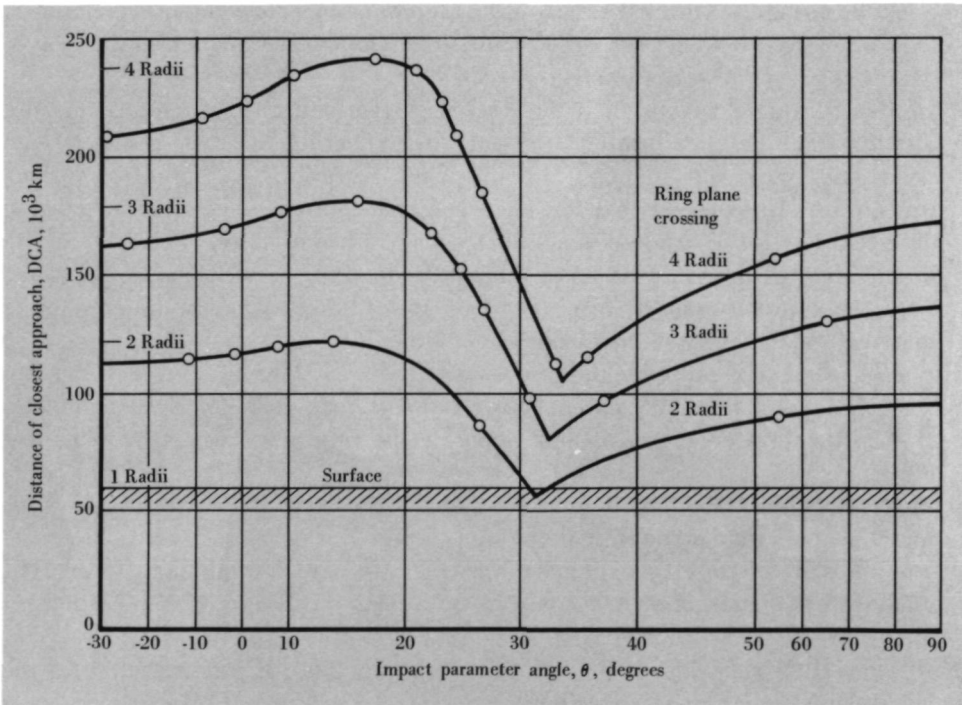


FIGURE 6. — Minimum DCA at Saturn for late arrival ring plane crossing.

Now, how close can you get to Saturn and still maintain a certain ring plane crossing distance? That is shown in figure 6. Ring plane crossing distances are the contours that are shown here, and they represent distances from 2 to 4 radii. So if we have trajectories with different values of θ , which is related to inclination, then if the ring plane crossing distance is 2 radii the closest approach distance to Saturn will range between 1 and 2 radii as shown. Therefore, for a θ angle of 32° , it is possible to cross the ring plane at 2 radii and, in fact, impact Saturn. Going to a larger ring plane crossing of 3 radii, which is about the location of Mimas, you could get, for example, within one-half a radius of the surface of Saturn. If you had the constraint that we currently have, which is 4 radii for the ring plane crossing, or in the vicinity of Enceladus' orbit, it is possible to get as close as within 1.8 radii of Saturn. As shown in the figure, this occurs at a certain value of θ and, as I said, θ starts off parallel to the ecliptic and then increases clockwise, or downward. The ring plane is located near a θ value of about 28° . Therefore, at 30° to 40° , you would pass just under the rings and get as close as possible to Saturn and still maintain the ring plane crossing at 4 radii. A decrease or increase in this angle will change how close you get to Saturn. This figure could be used to locate the best targeting at Saturn in order to get as close to the planet as possible.

DISCUSSION

Fred Franklin Have you done simulation studies to see what mass determinations might be possible? I think the mass of Rhea, for instance, is completely unknown.

One wonders if it would not be possible to get, say, close enough to the ring to detect its mass if it were sufficiently great.

Paul Penzo Just flying through the Saturn system will give you some mass information for practically all of the satellites. There is a study going on that determines how well you can find the mass of various satellites for various trajectories. It turns out that for Titan you need not get very close in order to determine its mass to more accuracy than we presently have. For some of the inner satellites, it would be more difficult to do, and we may have to approach Saturn much more closely in order to separate the effect of the inner satellite masses from the effect of the planet's oblateness.

Franklin Can you determine the ring mass?

Penzo It is very difficult. An analysis has been done recently which indicates that to the first order you cannot separate the ring mass from the oblateness effect.

Franklin I think, though, if you went above the ring plane, you might have better success than if you were in the ring plane.

Penzo Yes, intuitively this seems to be the case, and I think that's true. The analysis I saw did not say this, but I would think so. We haven't gone that far into it.

Eshleman It is very difficult to determine the ring mass. If you went through the Cassini division, there is a possibility.

Franklin You might go into the Cassini division and not come out.

Sam Gulkis You did all of this work independent of anything that was going on at Jupiter. I would like to know which of these constraints are actually closed out by putting similar constraints on the Jupiter flyby.

Penzo Sam (Gulkis), Jupiter will affect the trajectory analysis or mission analysis in the sense that currently we allow the trajectory to pass within about 4 Jupiter radii of Jupiter, and that occurs for early arrivals at Jupiter and, correspondingly, early arrivals at Saturn. If that safe distance increases, say to 6 R_J , then you would have to remove the section of early arrival dates. Then we could not get there for the first few months because the distance of close approach to Jupiter is determined by the fact that you must go to Saturn, and the early arrivals at Jupiter require getting close to Jupiter. However, you could still accomplish all of the other mission goals.

Gulkis The targeting is still the same?

Penzo Yes. You simply narrow down the trajectory space that you will get, but you can still accomplish all of your goals.

Gordon Pettengill In figure 5, was Titan also assumed to be approached closely?

Penzo The trajectory that results in the best Earth occultation by Saturn's rings does not contain the Titan before or the Titan after encounter. It is closer to the region of the Titan after encounter, however, than it is to the Titan before encounter.

Pettengill What I am asking is, if one were to select one of those as desirable, are you then throwing away a close Titan approach?

Penzo Yes, that is true. You would certainly be throwing away a Titan before encounter because it requires staying further away from Saturn to satisfy the ring plane crossing constraint. There is a narrow region that occurs directly underneath the ring plane where you can get very close to Saturn and still maintain a certain distance for ring plane crossing.

Pettengill You say "close to Saturn." Do you mean close to Titan?

Penzo No, close to Saturn. The Titan after encounter, however, is close to that region, and the overlap, if any, has yet to be investigated.

Brad Smith Well, Gordon, we are going to launch two spacecraft and hopefully two will arrive at Saturn.

Pettengill And one will be used for Titan?

Smith That is a possibility.

Robert Soberman: Paul (Penzo), have you had a chance to give any consideration to Al Cook's¹ suggestion that in fact a safe place to cross the ring plane inside of 4 R_S might be through the orbit of Mimas?

Penzo Yes, that was implied on the last figure, where you could maintain the spacecraft at a fixed ring plane crossing distance, such as 3.1 R_S , which is the orbit distance of Mimas. You would have to stay on essentially the curve marked 3 radii in figure 6. So as you selected different values of θ , your distance of close approach to Saturn would change. For Titan before passage, for example, a θ angle of around 60° to 70° would be required, so you would have to pass

¹A. F. Cook, Smithsonian Astrophysical Observatory, a member of the MJS 77 imaging team.

Saturn at about 2 radii and cross the ring plane at Mimas' orbit. That would result in a Titan before close approach. The Titan after encounter is located near a θ angle of about 35° to 40° , so your closest approach to Saturn would have to be lower. This suggestion puts an added constraint on the trajectory selection in the sense that previously you could encounter Titan with any distance of close approach to Saturn or, equivalently, any ring plane crossing distance. If you insist on also passing through the orbit of Mimas, then you would be forced to fix the ring plane crossing distance at Saturn and also fix the arrival date. Hence, whereas figure 4 shows conditions for a time variation of 2 days, you now would have a specific date on which to both pass through Mimas' orbit and encounter Titan.

William Irvine: The choice of Jovian satellites you might wish to encounter doesn't particularly affect these computations?

Penzo: Not strongly. The time at which you arrive at Jupiter and the time at which you arrive at Saturn are tied together in the sense that the earlier you get to Jupiter, the earlier you will get to Saturn. However, utilizing midcourse corrections, you can vary the relation between the two arrival dates within a couple of days and that is sufficient to allow you to perform the mission analysis at Jupiter and find a Galilean satellite encounter separate from the Saturn analysis.

Smith: That, in fact, is a strong constraint, because the period of Titan is something like 16 days, so if you have only a few days to play around with, that definitely controls the satellite opportunities at Jupiter.

Penzo: At Jupiter, that's right. However, as it happens, because there are four major satellites of Jupiter, when we do pick a date we are within 1 or 2 days of encountering a satellite at Jupiter.

Smith: But the Jovian satellites are not all of equal importance.

Penzo: Yes, that's right. Perhaps I should say something about the Jupiter trajectories. The Jupiter analysis is quite different from Saturn's because the Jupiter flyby is constrained to continue on to Saturn, and you fly by Jupiter essentially in the equatorial plane of Jupiter. The close-approach range is from inside Io's orbit out to about halfway between the orbits of Europa and Ganymede. The Io approach occurs early in arrival dates and then disappears. So you can only get Io on early Jupiter encounters—which implies early Saturn encounters—and that amounts to about one-sixth of the total trajectory space that you have available. For the remaining arrival dates, the trajectory past Jupiter increases in closest approach distance and remains outside of Io's orbit.

Irvine: How much does your lack of knowledge of the mass of these satellites affect your trajectories?

Penzo: At Jupiter it is very important because we must maintain the trajectory to get to Saturn. But once we are at Saturn, the trajectory requirements past Saturn are very free. At Saturn, we are interested in the postencounter orbit determination problem, where we can try to determine the masses, but we are not so interested in the effect of uncertainties in those masses as far as where the trajectory goes past Saturn. If we had to go on to Uranus, of course that adds

a tight constraint, which increases the importance of knowing the mass of any Saturn satellites that are closely approached.

Irvine That implies it is not reasonable to aim for Uranus.

Penzo Not necessarily. There are missions being studied. The Saturn accuracy problem I have stated, however, could very well imply that the spacecraft presently being designed for MJS 77 could not carry enough midcourse fuel onboard to get to Uranus.

Dick Wallace There is a long time for small course changes to act. The flight time to Uranus is twice as long as to Saturn.

Penzo The fuel requirements are proportional to the time you are on the trajectory, and you require a couple of years to get to Uranus past Saturn. There are missions being investigated for the post-1977 period to the outer planets, and one particular mission is a Pioneer Saturn/Uranus probe. Figure 7 shows essentially the same impact parameter plane that I showed earlier for the MJS 77 mission, in that the approach direction is from the perpendicular to the plane of the figure. Plotted are the ring plane edges and lines of constant ring plane crossing distance. The points at which you would aim in this plane to get a Saturn probe are marked. You would want to get as close to Saturn as possible in order to minimize a ΔV necessary for deflecting a probe into Saturn, and, if you are interested in the spacecraft continuing on past Saturn, you would want to maintain passage beyond the visible rings as shown. If you want it to continue on to Uranus, then, as I said earlier, for a Pioneer Saturn/Uranus probe, the aimpoint regions for continuing on to Uranus are essentially in the ecliptic plane. The horizontal is the ring plane location. The ecliptic plane passes essentially through the points marked 1979 through 1982, which are the

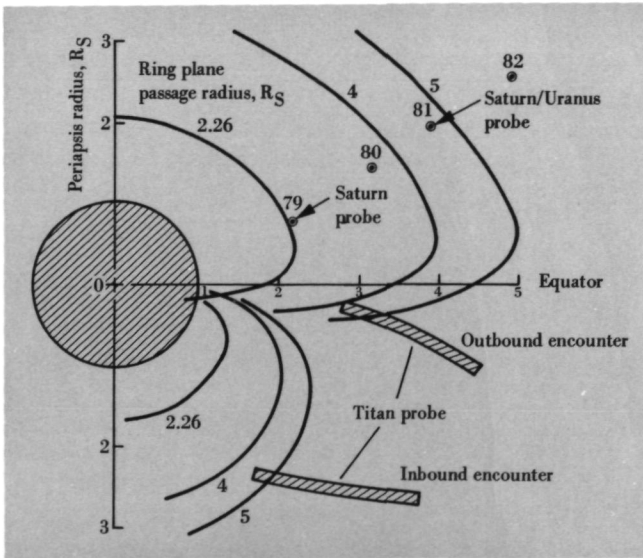


FIGURE 7.—Aiming diagram at Saturn—1981.

Saturn-Uranus probe aimpoints. So in 1981, for example, you would have to aim at the point shown.

Eshleman Is that a launch in 1981 you are talking about?

Penzo Yes, a launch in 1981. A launch in 1980 would actually require that the aimpoint be within or on the $4 R_S$ boundary, and in 1979 it would move in even closer. This indicates that just as you have desirable aimpoints for the Mariner Jupiter/Saturn mission, you have similar aiming regions for missions that go beyond the 1977 Jupiter/Saturn launch. This figure, which is similar to figure 2, was given to me by John Niehoff of Science Applications, Inc., Chicago, who determined the aim region for the Titan 1981 encounters, which are very similar to those of the MJS 77 launch.

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 $1.6 \mu\text{m}$, 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 $200 \mu\text{m}$, perhaps a millimeter.

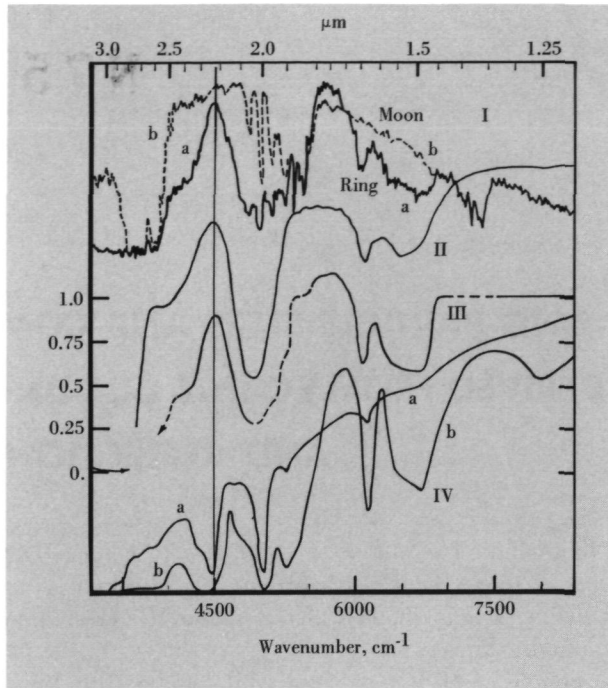


FIGURE 1.—Comparison of reflectance spectra for H₂O and NH₃ frosts and Saturn's rings. Ia—The Saturn ring spectrum from Kuiper et al. (1970). Ib—Lunar comparison spectra. II—Fine-grained H₂O frost spectrum of Kieffer (1970). III—Normalized Saturn ring spectrum (Ia divided by Ib). IV—Kieffer's preliminary NH₃ 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

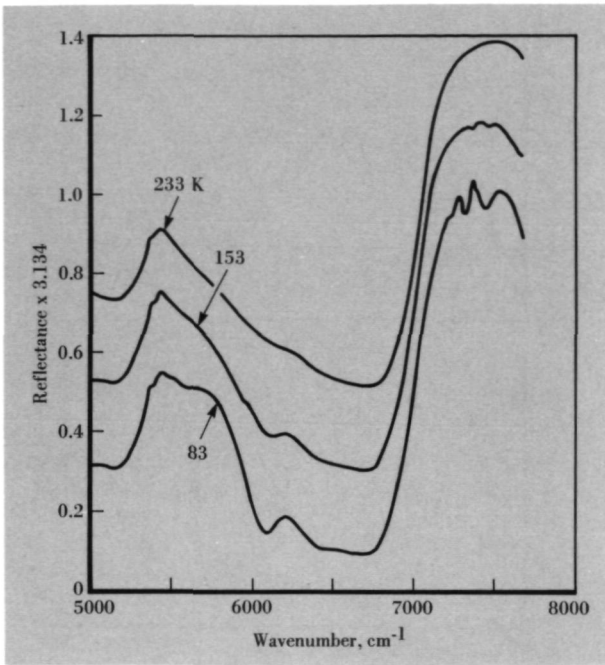


FIGURE 2.—*Temperature dependence of 1.6 μm (6100 cm^{-1}) water frost feature.*

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

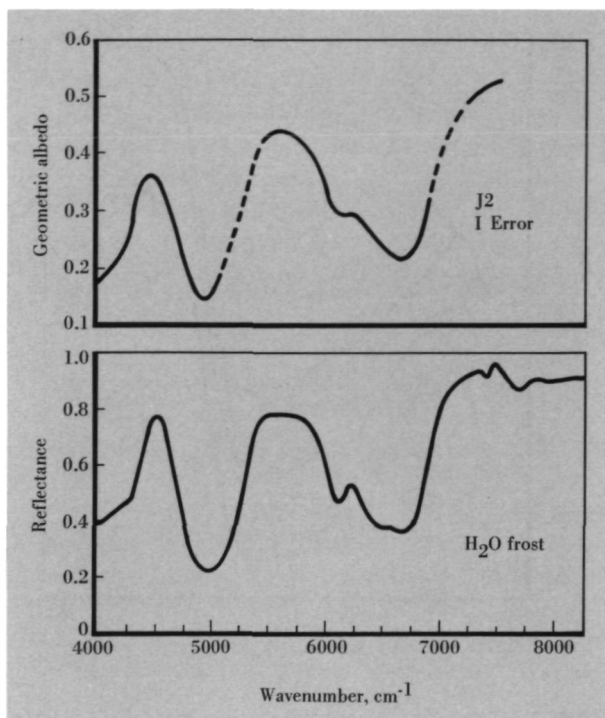


FIGURE 3.—Reflection spectra of Galilean satellite J2 and water frost.

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, I₀ 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 $100\ \mu\text{m}$ 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 $100\ \mu\text{m}$ 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, $100\ \mu\text{m}$ 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 $100\ \mu\text{m}$ 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 $100\ \mu\text{m}$ 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:

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

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

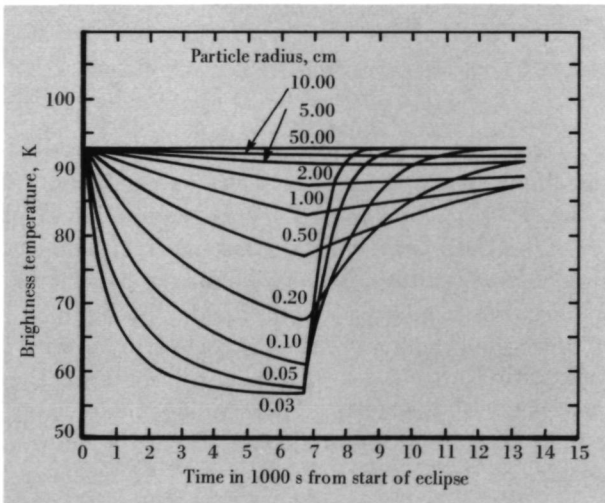


FIGURE 4.—Brightness temperature of solid ice-like mid-B ring particles as a function of particle radius and time from entering Saturn's shadow.

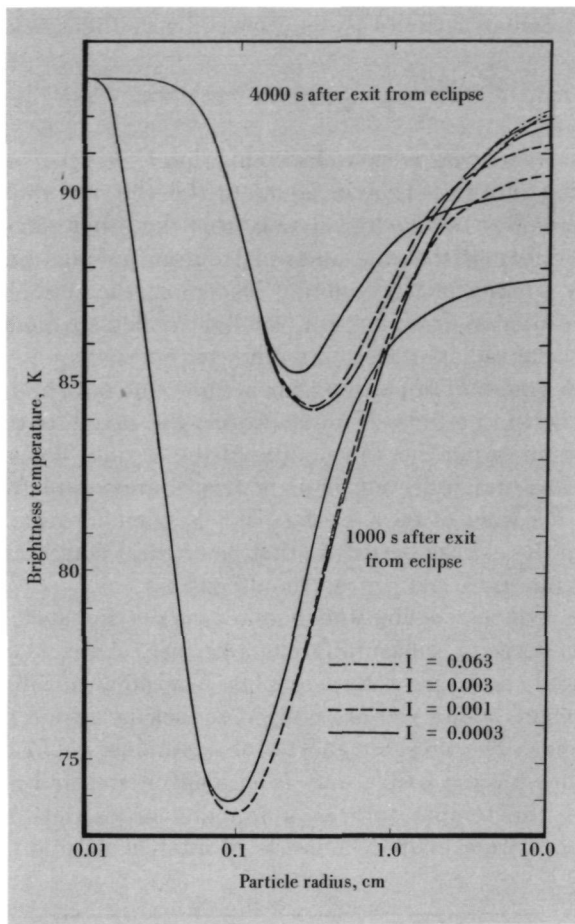


FIGURE 5.—Particle temperature 4000 s and 1000 s after exit from eclipse.

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

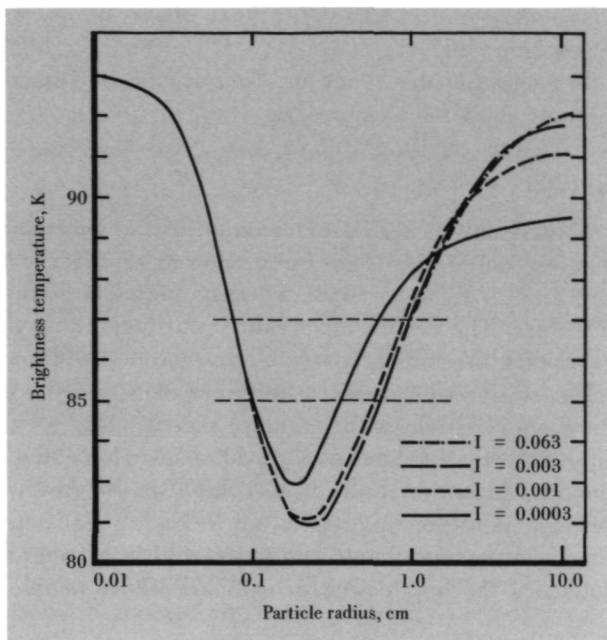


FIGURE 6.—*Apparent brightness temperatures of mid-B ring particles as a function of size and thermal inertia as seen with a 5 arc sec diameter telescope beam, 5 arc sec after exit from eclipse for a 25° tilt of the ring plane relative to the Sun and a 3° phase angle.*

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 $20 \mu\text{m}$ 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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Robert E. Murphy

VARIATIONS IN THE INFRARED BRIGHTNESS TEMPERATURE OF SATURN'S RINGS

The rings of Saturn were first detected in the thermal infrared by Allen and Murdock (1971). They determined $T_B = 83 \pm 3$ K at $12 \mu\text{m}$ in late 1969. Before that time the rings were thought to be very cold due to their apparently high albedos. Low had set an upper limit of $T_B = 60$ K at $20 \mu\text{m}$ in 1965 (Low, 1966). Six years later, Murphy, Cruikshank, and Morrison (1972) reported still higher temperatures at $11 \mu\text{m}$ and $20 \mu\text{m}$, and they suggested that the ring temperature may be varying with the Saturnocentric declination of the Sun, B' . This hypothesis was apparently confirmed by Murphy in 1972, when he measured the $20 \mu\text{m}$ brightness temperatures of the A, B, and C rings, finding $T_B = 94 \pm 2$ K for the B ring (Murphy, 1973). Additional data were provided by the independent observations of Morrison (1974), who found $T_B = 96 \pm 3$ K at $20 \mu\text{m}$ and $T_B = 90 \pm 3$ K at $11 \mu\text{m}$.

Thus, it appears that the brightness temperature of the rings does vary with time. Before the observed temperatures may be compared with any model predictions, some adjustments must be made. When the adjustments have been made, the evidence for variations in T_B with B' are less pronounced but still apparently real. The observed variations appear to contradict the hypothesis that the rings consist of a multilayer assemblage of particles which are small compared to the thickness of the rings.

SUMMARY OF BRIGHTNESS TEMPERATURES AND ADJUSTMENTS TO THE DATA

The first attempt to measure the infrared brightness temperature was made by Low in 1964. He found $T_B < 80$ K at $10 \mu\text{m}$ at a time when $B' \sim +9^\circ$ (Low, 1965). This upper limit, seemingly of little consequence at that time, severely constrains any model since it lies in a critical range of B' not covered by more recent data. The measurement was made at a time when Saturn was 9.8 A.U. from the Sun.

University of Hawaii.

To compare to the 1972 data when Saturn was 9.0 A.U. from the Sun, we increase the observed upper limit by the factor $(9.8/9.0)^{1/2} = 1.04$.

Next, we adjust for the difference between the brightness temperature of the entire ansa and that for the B ring alone. Murphy (1973) found $T_B = 89 \pm 3$ K for the A ring and $T_B = 94 \pm 2$ K for the B ring. From these data we estimate a +2 K correction is needed to relate Low's measurement of the ansa to the later measurements of the B ring alone. We note that the difference in the brightness temperatures of the ring components may be a function of the volume density of the rings and of the Sun's declination B' , and the 2 K correction may be valid only near $B' = 26^\circ$ as when Murphy's measurements were made. The original observational upper limit to the brightness temperature is listed in table I, and the adjusted B ring brightness temperature is listed in table II and plotted in figure 1.

In 1965 Low set an upper limit to the brightness temperature at $20 \mu\text{m}$ of

TABLE I.—*Infrared brightness temperatures of Saturn's rings.*

Observation year	Observers	$\lambda, \mu\text{m}$	Position	T, °K
1964	Low	10	Ansa	< 80
1965	Low	20	Ansa	< 60
1969	Allen & Murdock	12	Ansa	83 ± 3
1971	Murphy et al.	20	Peak at ansa	89 ± 3
1972	Murphy	20	B ring at ansa	94 ± 2
1972	Murphy	20	A ring at ansa	89 ± 3
1973	Morrison	11	B ring at ansa	90 ± 3 ¹
1973	Morrison	20	B ring at ansa	96 ± 3 ¹
1973	Nolt et al.	35	Ansa	90–95

¹ Temperature Difference $T_{20} - T_{11}$ accurate to $\pm 2^\circ$ K.

TABLE II.—*Adjusted B-ring brightness temperatures.*

Observation year	Observers	$\lambda, \mu\text{m}$	T, °K
1964	Low	10	85
1965	Low	20	64
1969	Allen & Murdock	12	86 ± 3
1971	Murphy et al.	20	90 ± 3
1972	Murphy	20	94 ± 2
1973	Morrison	11	90 ± 3
1973	Morrison	20	96 ± 3
1973	Nolt et al.	35	92–97

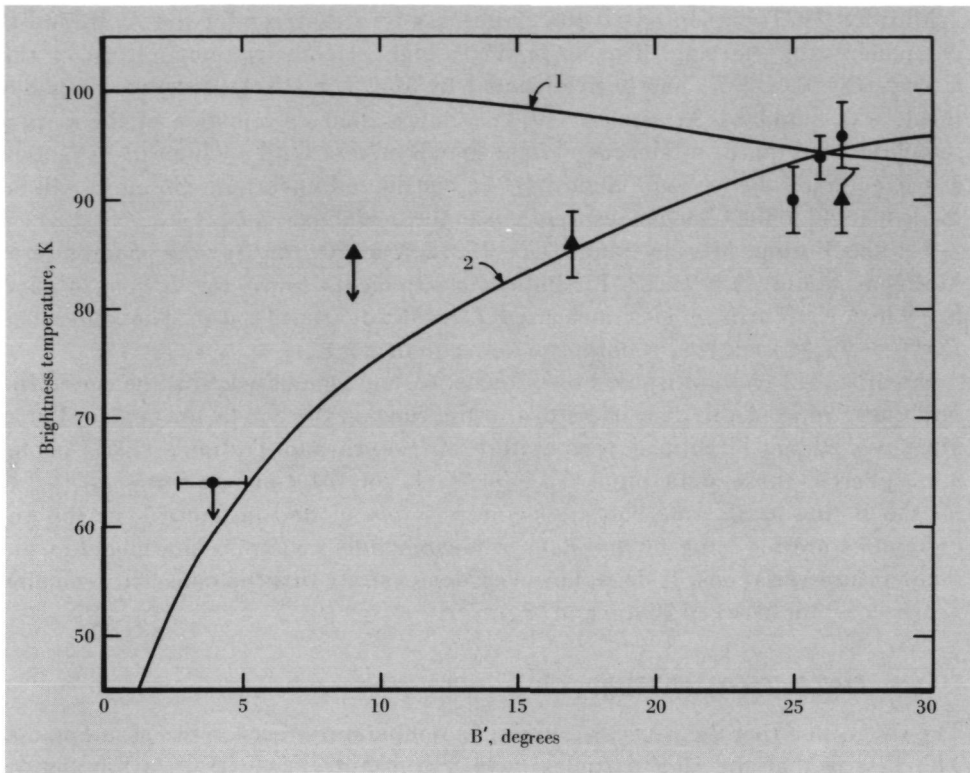


FIGURE 1.—Adjusted B ring brightness temperature measurements vs Saturnocentric declination of the Sun B' . Triangles refer to $\sim 10 \mu\text{m}$ and circles to $20 \mu\text{m}$ values listed in table II. The solid curves refer to brightness temperature models discussed in the text.

$T_B < 60 \text{ K}$ (Low, 1966). Saturn was 9.7 A.U. from the Sun, and the Saturnocentric declination of the Sun B' was $\sim 5^\circ$. The adjusted B ring temperature is $T_B < 64 \text{ K}$. The horizontal error bar in figure 1 indicates that the exact value of B' is not available.

Allen and Murdock's 1969 measurements of $T_B = 83 \pm 3 \text{ K}$ (Allen and Murdock, 1971) were made when $B' \sim -17^\circ$ and when $R = 9.2$. The adjusted B ring temperature at $12 \mu\text{m}$ is $T_B = 86 \pm 3 \text{ K}$.

In 1971 Murphy et al. (1972) measured the relative fluxes of Mars, Saturn, and Saturn's rings with high spatial resolution at $11 \mu\text{m}$ and $20 \mu\text{m}$. They quoted brightness temperatures of the rings relative to the disk which, in turn, was calibrated by observations of Mars earlier in the night. We reevaluated the data and used Murphy's (1973) model-dependent brightness temperature of Saturn as the calibration. We found $T_B = 89 \pm 3 \text{ K}$ for the B ring at $20 \mu\text{m}$ while the $11 \mu\text{m}$ data were found to have been incorrectly analyzed. No meaningful $11 \mu\text{m}$ temperature may be derived, but it is clear that the rings are $\geq 5 \text{ K}$ cooler at $11 \mu\text{m}$ than they are at $20 \mu\text{m}$. For these measurements $B' \sim -25^\circ$ and $R = 9.1 \text{ A.U.}$ The adjusted B ring brightness temperature is $90 \pm 3 \text{ K}$.

Murphy (1973) presented 20 μm brightness temperatures for the A, B, and C components of the ring. The surprisingly high brightness temperature of the C ring, $T_B = 89 \pm 3$ K, has been disputed by Morrison (1974), although a similar result was found by Armstrong (1971). Confirmation or rejection of the C ring temperature requires a telescope larger than 3 m, and until such an investigation has been made the measurement must be considered uncertain. No effort will be made to include the C ring measurements in the modeling.

For the B ring, Murphy found $T_B = 94 \pm 2$ K at 20 μm . Several months later Morrison found $T_B = 96 \pm 3$ K. Both measurements were for $B' \sim -26^\circ$ and $R = 9.0$ A.U. Morrison also measured $T_B = 90 \pm 3$ K at 11 μm . The difference $T_B(20) - T_B(11) = 6$ K is accurate to better than ± 2 K.

In early 1973 Nolt, Murphy, Ford, Radostitz, and Donnelly (1973) measured the brightness ratio of the disk of Saturn to the rings in the 30–43 μm region. Using Murphy's 20 μm brightness temperature for Saturn and Trafton's (1967) model atmospheres, these data imply $T_B \sim 90\text{--}95$ K for the ring ansa or $\sim 92\text{--}97$ K for the B ring at 35 μm . This temperature is not plotted in figure 1, as the uncertainties are too large for the data to meaningfully constrain any model of the temperature variations. It does, however, demonstrate that the emissivity remains nearly constant between 20 μm and 35 μm .

DATA EVALUATION

The suggestion that the rings are varying in temperature rests on two assumptions. The first is that the ring particles have a constant emissivity as a function of wavelength. The second assumption is that the upper limits of Low (1965, 1966) are valid.

The first assumption is clearly incorrect. Morrison's (1974) measurements show that the relative emissivities are $\epsilon_{11}/\epsilon_{20} \sim 0.4$, a surprisingly low value. The ratio $\epsilon_{20}/\epsilon_{35} \sim 1$, when combined with the ratio of 11 μm and 20 μm , suggests that the particle radii might be on the order of 25 μm . Yet other data clearly suggest $r \sim 1$ cm is more nearly typical (Pollack, Summers, and Baldwin, 1974; Morrison, 1974). It is also unlikely that the sharp drop in emissivity can be caused by restrahlen features in the particle spectra (Murphy, Logan, Salisbury, and Hunt, 1974). Any fully successful model must explain the emissivity ratios.

When the 11 μm and 20 μm data are examined separately, we see that evidence for variation in the B ring temperature at 11 μm is almost nonexistent, while the 20 μm variation is evidenced primarily by the upper limit set by Low (1966). Thus we see the importance of this one measurement, and the need for further information on the observation is emphasized. (The temperature is obtained from Aumann, Gillispie, and Low (1969), which references an abstract in 1966 that provides no details.)

It is important to note, however, that the classical ring model favored by Pollack et al. (1974) and others is inconsistent with the data, even if Low's 1965 point is invalid. The brightness temperature of a multilayered model must necessarily increase as $|B'|$ decreases. The observed brightness temperature will be

$$T_B = T_{particle} (1 - e^{-\tau \sec B'})^{1/4} \quad (1)$$

We have calculated the variation of T_B with B' following equation (1), using $\tau=0.7$ (Kemp and Murphy, 1973), and plotted it as curve 1 in figure 1. The data were arbitrarily passed through the point ($T_B=94.5$ K, $B'=26^\circ$) by setting the particle temperature equal to 100 K. We have not allowed for the drop in the particle temperature as we penetrate more deeply into the ring; the effect of a decreasing temperature with depth would be to straighten out the curve, resulting in little or no temperature variation with B' . At the same time, a temperature gradient in the ring would cause the apparent 11 μm brightness temperature to be *greater* than the 20 μm brightness temperature. This latter effect is caused by the more rapid decrease in flux with temperature at 11 μm than at 20 μm , with consequent greater weight being given to the hotter particles at 11 μm . The temperature difference is estimated to be between 1 K and 3 K in the opposite sense from the observed temperature difference. That is, if $T_B(20)=96^\circ$, then $T_B(11)$ should be 98 K, not 90 K as was observed. The difference in flux from 98 K to 90 K at 11 μm is more than a factor of 3.

A much more satisfactory fit to the data is obtained with a model that is somewhat unrealistic physically. In curve 2 of figure 1 we show the variation in brightness temperature of a monolayer assemblage of flat particles with the large surfaces lying in the ring plane. Each ring particle then receives less sunlight per unit area as the angle B' decreases and the temperature of a particle decreases according to

$$T_{particle} = T_{max} (\sin B')^{1/4} \quad (2)$$

and

$$T_B = T_{particle} (1 - e^{\tau'})^{1/4} \quad (3)$$

where τ' is the optical thickness, which does not vary with B' for a monolayer of particles that are thin compared to their width and breadth.

Curve 2 is seen to be in good agreement with all of the data including Low's (1965, 1966) upper limits and allowing for $\epsilon_{11}/\epsilon_{20} \sim 0.4$ (or alternatively $T_{20} - T_{11} = 6 \pm 2$ K) through some as yet unknown mechanism.

A closely packed monolayer of spherical particles would behave similarly to curve 2 but with a slower decline in T with decreasing B' . The very low value, $T_B < 64$ K, can still be explained since the Saturnocentric declination of the Earth B was greater than B' at the time of Low's observation. Thus he may have been measuring much of the shadowed portions of the ring particles, which may be very cold for sufficiently large particles.

CONCLUSIONS

The infrared brightness temperature variations place important constraints on models of Saturn's rings. After adjusting for the decreased Sun-Saturn distance and adjusting all measurements to B ring values only, we see that the temperature variations are not as large as was originally thought. If Low's upper limit of $T < 60^\circ$

(Low, 1966) is valid, then the particles must lie in a monolayer and have flattened surfaces lying in the ring plane, or possibly be spheres nearly in contact with one another. The "classical" multilayered agglomerate of small particles cannot produce the observed variations. If Low's (1966) upper limit is incorrect, the classical model is still inadequate but may be ultimately reconcilable with the data. The difference between the 11 μm and 20 μm brightness temperatures must be explained by any satisfactory model.

DISCUSSION¹

William Irvine I attempted to do a very quick computation of the effect of thermal emission from particles in the upper layer of a multilayer model on the particles lower down. If you treat the ring particle as having a grey opacity, then you can do a simple transfer problem and you find that the source function goes down essentially linearly with optical depth, which means that the temperature decreases very slightly, going as the fourth root as you go down through the layers. Also, if you neglect the Low (1966) measurement, there is very little tilt effect.

Robert Murphy That's right, you can almost argue that there is no tilt effect if you are willing to discount Low's original measurement.

Irvine Far be it for me to do that.

Murphy But it is a possibility.

Hugh Kieffer In my presentation I neglected to mention that we had a problem in dealing with the apparent brightness temperature and the bolometric albedo for the rings. We introduced a concept that has been helpful in this, and that is the concept of a radiative anisotropy index, q (i.e., the extent to which the thermal radiation is peaked in the solar direction or Earth direction (we have ignored the phase angle)). The problem is that if you use an albedo on the order of 0.6, you end with anisotropy on the order of 2, and that means that the radiation in the solar direction is on the order of twice the mean radiation in all directions. I don't really know what this means just yet in terms of the allowable particle size, rotation rates, and thermal inertias, but it is going to be constraining to some extent certainly. It tends to make it very difficult to have small, highly conductive particles, because they simply cannot generate such high anisotropy in their thermal emission.

Irvine Let me see if I understand that. You require that kind of anisotropy in order to match the brightness temperatures.

Kieffer Yes, in order to make the infrared brightness temperature measurements compatible with the insolation, which is known. The thermal measurements are higher than one would predict for the amount of energy we expect to be absorbed by the rings.

¹ Editors' note: At the workshop Murphy presented a multilayer model with a steep temperature drop inward from the illuminated face due to shadowing. Following discussion, a portion of which is included above, this model was deleted from the revised text on physical grounds.

You have to throw in the shadowing factors and the relative geometry as well. They do not differ greatly, depending on the detailed placement models you use. We looked at a hexagonal monolayer, where the spacing is hexagonally symmetric. The monolayer ended with the particles being spaced about 2 radii apart. And we also looked at the case of small particles uniformly distributed—in other words, perfectly uniform spacing of the particles in the ring and opacities on the order of 1. The amount of shadowing and the solid angle of the remainder of the ring as seen by any given particle are not strong functions of the detailed geometry that is used.

I think there is some evidence for an incompatibility between the reported visual albedo of the ring particles and their bolometric albedo based on the thermal radiation measurements.

James Pollack What value of bolometric albedo do you require in order to have an anisotropic value of 1?

Kieffer A bolometric albedo of about 0.3.

Pollack So a very low value of bolometric albedo is required.

Kieffer I could have cited the following numbers: for a bolometric albedo of 0.47, you get a q of 1.4, sort of a reasonable number; for a q of 8/3 the bolometric albedo is 0.76. So we are bounding the range we expect the bolometric albedo to lie in, but reasonable bolometric albedos do have appreciable anisotropy associated with them.

Pollack What is the limit we now think there is on bolometric albedo?

Kieffer A reasonable range for bolometric albedo is 0.45 to 0.9.

Murphy It is hard to see how you can match the IR measurements with a bolometric albedo of 0.9.

Pollack The polarization observations² are tremendously interesting; have you got any results for either ring A or ring C?

Murphy No, ring A misses the planet entirely at this time, and ring C is substantially smaller. We did not attempt a ring C measurement. You could determine the optical thickness for ring C by direct means without using polarization techniques.

Irvine In the polarization measurements you have to assume that Saturn's disk has a north-south symmetry.

Murphy Yes, that has been studied by Hall and Riley (1969) over a period of years, and they find that it does have north-south symmetry when they can see both halves of the planet. It was Hall, by the way, who suggested this measurement. He has done the same thing at a much shorter wavelength. His preliminary results are in agreement with ours.

Brad Smith That is surprising, because the planet has a very strong north-south dissymmetry in the ultraviolet.

² See Kemp and Murphy, 1973, for complete discussion of these observations and the deductions about ring optical thickness.

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George A. Morris, Jr.

DISTRIBUTION AND SIZE OF ELEMENTS OF SATURN'S RINGS AS INFERRED FROM 12-cm RADAR OBSERVATIONS

I would like to talk about the work Richard Goldstein and I did on Saturn last December and January at the Goldstone station, using a 64-m antenna. This is a particularly good time for radar observations. The rings are inclined at an angle of about 26° with respect to the line of sight, which increases the projected area of the rings and also reduces the amount of doppler spread. The result of this work was a positive radar return corresponding to about a 60-percent return from an isotropic scatterer with the projected area of the rings of Saturn, allowing for the Cassini division. This can be compared with a measured value of about 6 percent for Mercury, 12 percent for Venus, and the 8 percent we have measured for Mars.

The technique was to use radar signals at a wavelength of 12.6 cm. We had a beam width of about 0.1° with the 64-m antenna. So our beam width was still relatively large compared to the target. The radar signature would have the maximum frequency occurring, of course, for the particles in the inner ring, and the low frequency return from the particles in the outer ring. We expected a certain signature from the received signal and so we used a bandwidth from edge to edge at 12.6 cm of 586 kHz.

The round trip light time at the time of this observation was about 2 hr and 15 min. We broke up our observations into blocks, transmitting for 32 s and then turning the transmitter off for 32 s to form a calibration. This went on for the round trip light time. We took the received signal for 32 s and subtracted the spectra obtained during the 16 s on each side of the time when the signal should have been there as a calibration. So we wasted a relatively large amount of time in calibrating, but then this turns out to be a well-calibrated experiment. We were only able to get two of these round trips each night, and we observed on six nights.

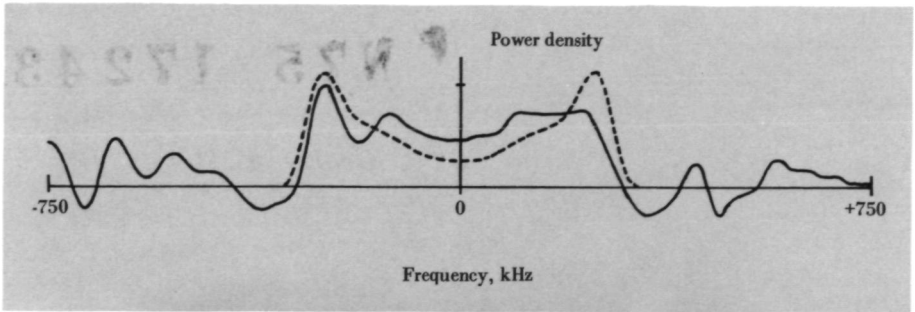


FIGURE 1.—Radar spectogram of rings of Saturn, with received power density plotted against doppler frequency shift. The theoretical curve (dashed) is for a two-ring system, with the particles closely packed in the inner ring and the outer ring 50-percent filled. For this model, the radar cross section of the particles would be 80 percent.

On each of the individual spectra you were able to see a signal; when you showed it around to people, almost everyone thought he was seeing a signal. Figure 1 shows the result of all of the six nights combined. We were operating with a total bandwidth of 1.5 MHz. The expected bandwidth was 586 kHz. The dashed curve shows a theoretical spectrum. The technique was to assume that the B ring is completely filled and that the A ring is half filled. The shadowing effect of Saturn is added to that to bring it down. Part of the B ring is completely shadowed, so we are down for that portion by 3 db. There is relatively good agreement between the theoretical curve and the data, within the limits of the noise that is present.

William Irvine The shadowing is the shadowing by the disk of the planet?

Morris Yes. The biggest disagreement between the two curves in figure 1 is the area toward the center, where we are receiving more power than we would have expected from this model.

Gordon Pettengill What is the standard deviation of the measurement? It doesn't look all that much smaller than the difference between the two curves. Can you really make that last statement without any statistics?

Morris No, not statistically. My statement is based on looking at where you see a difference between the two curves, so it may not be significant.

Pettengill Maybe 50 percent?

Morris It is just about that.

James Pollack That is random.

Morris We are not saying very much about it. It is an observation of the agreement or lack thereof that you see here.

Brad Smith Well, a fair amount has been said about it at meetings, however, particularly MJS meetings.

Von Eshleman In fact, is this different from what was shown elsewhere, especially in the center region of figure 1?

Morris No.

Eshleman It looks to me as though there is a smaller difference between the two curves.

Smith I agree. This does not seem to be what Dick Goldstein showed us.

Dennis Matson Is it not the case that there are more integrations taken toward the center due to the number of channels you have available?

Morris No. We started out with a 1-MHz bandwidth and we thought we had positive results there. After one night we decided to go to 1.5 MHz so we could get a better baseline. Some theoretical work that we did earlier on what the spectrum might look like indicated that the ears of the spectrum would even be higher. What we expected to find was a relatively narrow line near the edge—a limb brightening, if you will—and as a result of the first night's observations we were pretty sure we had something. We didn't observe the narrow bright feature, so we decided we didn't need resolution. What we would like to have is a better baseline for our measurement. That is the only difference in those data.

The temperature, if you wanted to label the observed curve, is about 0.6 of a millidegree at the peak. We estimated the return at 62 percent of what we would have received from an isotropic reflector with the same geometric cross section.

I think the means of interpreting the data is something that will be open to a lot of discussion. Certainly starlight is seen through the A ring, and we have heard here discussion of an optical depth of 1 for a B ring. Thus, we can't allow the rings to be completely filled. I think that is pretty clear. Dr. Murphy did discuss a disk model (see contribution by Murphy), and that would be pretty nice. If the rings were only a quarter filled, then we need something that has a reflectivity of 250 percent. How are we going to arrive at that kind of a number?

We examined several alternatives and I think we examined them in a simple form and didn't go into an extensive analysis. One appealing thing would be the multiple scattering model, but, when you need a backscatter gain of 250 percent, it becomes unattractive.¹

Pettengill Just what do you mean by "multiple scattering"? I think it is sometimes used to mean different things by different people.

Morris I was talking about it in the sense that Dr. Pollack uses.

Pettengill In what sense is he talking about it?

Pollack I think what he means is simply that you have to consider more than just the first scattering event.

Pettengill You mean from adjacent particles or from internal reflection?

Morris I think from internal reflection.

Pettengill Is that what you mean, Jim (Pollack)?

Pollack There are several possibilities. You can have radiation externally reflected and you can also have radiation transmitted; in many cases the transmitted radiation would probably be what you are interested in since there is more energy there.

Pettengill Maybe we should defer this to the end, but since you used the term, I keep finding myself floundering on the precise meaning, because different people have different meanings.

¹ Editors' note: For an alternate view of the attractiveness of multiple scattering as an explanation for the 12.6-cm radar return, see contribution by Pollack.

Pollack I think you are right. Let me clarify one thing. When Bill Irvine said “multiple scattering,” he generally meant it in the sense of higher orders of single scattering; in other words, everything but single scattering. When I used the term “multiple scattering,” I mean everything. I include the first scattering event as well.

George (Morris), your 250-percent figure makes me very uneasy for the following reasons. First, there is a secant z factor that is not negligible. The $\tau = 1$ that people have been speaking about applies for normal incidence. The second reason is that the particle size is much larger than a visual wavelength so that, in effect, does not include a diffraction component. At your wavelength there may well be a significant diffraction component that would have the property of doubling the optical depth. I think for both those reasons you need to think more about the need for a reflectivity of 250 percent.

Morris I think there may be many theoretical interpretations of the data, and it is clear that we need more data.

Pettengill Or more interpretation.

Morris There are, of course, some wild explanations. You could have mirrors out there that are directed perpendicular to the line of sight; you could have corner reflectors. There are lots of possibilities like that. You could have spheres with just the right properties so that you could obtain a large gain. But neglecting those, we elected to say that the most likely thing is that there are particles there which are relatively large compared to the wavelength, something on the order of a meter. At an eight of a meter wavelength, chunks on the order of a meter that are rough potentially could give you a gain of $8/3$ in the backscatter direction.

What kind of reflectivities could you get then with this type of a backscattering function? With silicates it would be possible to get reflectivities up to 80 percent, granites up to 110 percent, and metallic chunks up to 260 percent. It depends on what kind of a filling factor you would like to take.

Eshleman Excuse me, those numbers include the $8/3$ gain factor

Morris Yes. In the theoretical spectrum shown in figure 1 as the dashed line, we assume that the B ring is completely filled and the A ring one-half filled. Therefore, you need a reflectivity of 80 percent which would just allow silicates to do the job. If the filling were much less than that, you would need more exotic materials in the ring.

Irvine Allowing for multiple scattering can only help in that sense?

Morris Yes.

Hugh Kieffer Let’s examine that point for just a minute. Unless you start invoking a fairly peculiar phase function, how do you get beyond the $8/3$ factor?

Pollack That is not the point. The point is that he is multiplying $8/3$ times a reflectivity that’s appropriate for the Fresnel external reflection component. He is throwing away all the rest of the radiation, which may well get scattered in other directions. It is not a question of changing gain; it is making use of the additional scattered radiation.

Irvine If the layer is optically thick, then you just have to adjust the albedo of

the particles to some value less than 1 and you will get the observed reflectivity.

Eshleman If there is no loss.

Irvine You can still have some loss because you only need to get a 60-percent reflectivity.

Morris Unfortunately I do not know the significance of the central portion of the observed return.

Smith The difference from the theoretical curve seems to be within the noise, as you have drawn it in figure 1.

Morris Yes. What about the integrated power across the entire region where there is a difference between the two curves?

Smith It doesn't look that different.

Eshleman If you drew a curve subtracting the dashed curve from the solid curve in the middle, would anybody think there is any signal there?

Irvine This is carrying the discussion beyond what has been said here today, isn't it?

Morris Well, it has been discussed before, though, in other forums.

Eshleman It was discussed this morning, too, in the question of particles outside the A ring.

Irvine That is what I wanted to bring out, because not all of us have been in on those earlier discussions except by thirdhand accounts.

Morris If that difference is significant, what are the possible explanations? One is that there are particles outside the visible rings, i.e., either in the plane of the rings or out of it. Another possibility is that you are getting a radar return from Saturn itself. Saturn would likely behave as a diffuse scatterer. If you fit the data with that assumption and ask what would be the contribution from Saturn, it turns out to be 13 percent. I don't think we have any examples of planetary targets that scatter with that sort of a scattering law.

Pettengill What do you mean by the 13-percent number?

Morris I mean 13 percent of the isotropic cross section of Saturn.

Pettengill Isn't that what Venus gives, almost exactly?

Morris Yes, but the return from Venus has a different spectral shape.

Pollack Venus has a solid surface.

Smith What is the radar return from Jupiter?

Morris Jupiter doesn't show a return as far as we know.

Eshleman The return from Jupiter is less than 0.1 percent.

Frank Palluconi Is that correct for Jupiter? Has anyone looked with a broad bandwidth, or has the search been only for the zero doppler component?

Morris The narrowband component has been looked for several times. We are looking right now for the broadband component. We have made three observations. I reduced the first two very roughly, and it doesn't look encouraging.

Pettengill You mean from Jupiter?

Morris Yes, from Jupiter.

Pettengill There have been a number of attempts to observe Jupiter. The one I remember at Arecibo, which may no longer be as extensive as yours, showed that if you assume there was a highly specular type of reflection the limit was

0.0008 as a fraction of the geometric disk. If you assume that it was spread for some reason over the entire disk, the number is then 0.1 percent.

Morris Did you have the full limb-to-limb bandwidth?

Pettengill In the latter stages, yes. As I recall, JPL did report a positive detection from Jupiter at one time.

Morris That's right; for 1965 we reported a specular return with 1 kHz of bandwidth. It was a 5σ event and it was not at zero doppler; it was offset by 1 kHz or so.

Pettengill I remember something more like 8σ .

Morris If you assume that the predicts were correct, it has to have a very large slope to give that much of a doppler error. I think the interpretation at the time was that it was probably something going on in the atmosphere, so it may well be that there are atmospheric activities which would allow a radar return from a small part of the atmosphere.

Pollack I am worried about one thing: if you are going to take the difference seriously near the center of figure 1, you have to speak about something at radar wavelengths outside of the A and B rings, whose reflectivity or optical depth would not be substantially less than that of the A and B rings themselves. In that circumstance, I don't see why it would not have been photographed a long time ago.

Morris Well, I am not sure that is true. We have no way now of putting a fix on what sort of volume that material could occupy. It could be a relatively large volume with low spatial density. Since we don't have any ranging capability with this type of system, we can't tell. It is just that things which have low radial velocities will contribute in the center region.

Eshleman And they don't need to be in the ring plane.

Morris Right.

Pollack Even so, what you see when you take a picture is a projection against the plane of the sky. I would think that even if you spread it out quite a bit—say between 2 and 6 Saturn radii—you would still come up with something that probably could be photographed. What you actually see on the photograph is a two-dimensional projection.

Smith Isn't it true that a lot of the observed return near the center would represent particles quite close to the outer edge of the A ring?

Morris Our first guess at what the total return would look like was that the B ring would contribute a lot more and that we would get very peaky spectra. That is what we were really looking for.

Smith I am just getting back to what Jim (Pollack) was saying. If that were the case, we would expect to see material out there photographically and it wouldn't have to be spread out all over the sky. It would be quite closely confined to the outer edge of ring A; in other words, not zero frequency.

Morris You are talking about ± 150 kHz in radial velocity, so the particles could have relatively high velocities there.

Kieffer Nonetheless, a direct interpretation of your diagram, if you look at the excess above the dashed line, is that the unexplained component is on the order of 20 percent of the expected component. That means that if we just assume that all the radar and visual properties are the same for the unexplained as for the ex-

plained components, we would expect the visual observation of the unexplained component to have an integrated brightness of something like 20 percent of the known ring brightness, and I think that is far above the current visual limits. Can you comment on that ratio, Brad (Smith)?

Smith Yes, 20 percent is far too high.

Pettengill Would this be true even if the particle size distributions were not equivalent for the region outside of the A ring? Could you, by making those particles larger on the average, put more radar reflecting material there and not have it show up optically?

Pollack I don't think so. The dashed curve in figure 1 assumes a certain ratio of radar reflectivity of ring B to ring A. In particular, you assume that ring B is about twice as reflective as ring A. I rather suspect that if you made that ratio closer to unity you would probably generate and get a dashed curve that would give better overall agreement.

Morris Well, in a least-squares sense, the B ring equal to 1 and the A ring equal to 0.5 gave us the best fit.

Pollack My eye says if I pick the ratio closer to 1, I will tend to bring the peak down a little bit and bring the center up a little bit, and that will fit the data better.

Kieffer We are still playing a game in which the noise in the observed return could invalidate any argument.

Pettengill George (Morris), you said that the only way you could find to increase the apparent radar cross section over the geometric cross section, based on the optical observations, was to introduce the $8/3$ factor, which presumably results from Lambert-type scattering.

I wondered why that was necessary. If I look in the *Radar Cross Section Handbook* (Ruck, 1970) which I happen to have with me, I find that K_a for the particles varies from 4 or 5 up to an upper limit, which is determined only by the absorption inherent in the material. There are three examples given with dielectric constants; in one case 3, which sounds to me, based on lunar experience, not unreasonable at all for some mixture of ice and gravel, 4, and down to about 2.6. Then the straightforward Mie analysis gives you gains, i.e., ratios between the radar cross section and the geometric cross section, which vary from about 8 up to as much as 24. I am quoting the mean values.

As long as you have internal reflection—just as in the case of a rainbow effect—you can have an enormous backscattering that is not resonant. That is the point you discarded when you said it had to be resonant.

Kieffer Gordon (Pettengill), this is for spheres, is that right?

Pettengill This is for spheres, but my guess is that there would not be a major change for objects that were not spherical.

Morris I guess the wild oscillation . . .

Pettengill The oscillations in backscatter gain are not wild. That is the point I am trying to make.

Morris If you assume a distribution of particle sizes, that would smooth it out.

Irvine The glory is a phenomenon for spherical particles. It is an interference phenomenon caused by waves running around the surface of the sphere.

Pettengill Well, glory is only one part of the backscatter gain. I am not expert enough to give you the fraction it contributes, but, from the way it changes with radius, it can't be too sensitive to interference effects.

I wonder if this possibility was fully considered in trying to find an explanation for the radar return from Saturn's rings.

Morris I think that is a possibility. We did not refer to that particular information.

Pettengill It seems to me I could have a relatively unfilled ring with dielectric materials and sizes as small as a centimeter.

Morris The reason we rejected the case of, say, a sphere 0.2 of a wavelength in diameter is that it is unlikely that you just stumble onto the resonance frequency.

Pettengill This is not that sharply resonant.

Morris The curves that we looked at showed decided resonance effects, and we rejected them.

Pettengill If you are looking at conducting objects, you don't have internal reflection, and the resonance condition is well known. What I am trying to bring forward here is that there may be a mechanism to explain the ring reflection coefficient without a filled ring of large particles.

Eshleman I would be a little concerned about that explanation. If you took an elliptical surface it would still be true, but I think the particle surface has to be a very smoothly varying function.

Pettengill You are worried about the fact that the ring particles may well be rough?

Eshleman If they are rough, it is more likely an incoherent wave that emerges on the front side.

Pettengill My guess is you can still have values that are relatively high. Maybe Jim (Pollack) is going to speak a little bit more about this tomorrow.

Pollack I have tended to emphasize the way the multiple scattering affects the consideration.

Pettengill You are talking about what I call external multiple scattering?

Pollack That's right, and what you are pointing out is that if you consider single scattering alone, particles smaller than a meter can still produce high gain.

Pettengill Yes, I definitely think so, even if there were an appreciable contribution from external multiple scattering.

Have you treated the internal reflection contribution? Clearly this will add to the ability of a given dielectric material to effectively remove energy from the beam.

Pollack That's right. I am planning to do that in more sophisticated calculations than I have done so far. The work I have done so far, in fact, has only been for the isotropic-scattering case.

Lonne Lane From a practical point of view, given the questions that come up, our current knowledge, and the fact that the measurements from the radar apply to the largest particle size domain we have covered, where do we go from this point? In other words, will you continue with the S-band measurements and try to go to the X band?

Morris We are planning to go back with essentially the same system. We will

probably pick up a little bit, because next time, rather than using a switched radiometer, we will go to a frequency-hopping technique. Honestly, we didn't expect to get a return, so we used a simple system.

Pettengill Dick Goldstein and I have a joint proposal to be used with Arecibo both bistatically with the 210-ft Goldstone antenna and monostatically with the Arecibo telescope.

Lane Is that at S band?

Pettengill At S band in November 1974, if the system is ready. That should pick up between 10 and 14 db, depending on whether it is monostatic or bistatic.

Morris We also hope to get on X band, maybe in 1974 or 1975.

Pollack I think the really interesting direction is to go to the longer wavelengths. That is interesting because we know for sure that if you go to X band or something like that wavelength you are going to essentially get back the same type of signal you are seeing at S band. You have essentially the same type of cross section, and the interesting distinction between your (Morris) interpretation, which says they are really big (bigger than a meter), and my interpretation that they are only on the order of 2 cm, is that they really should be very bad reflectors at a meter wavelength if what I say is true. That is really the critical test to do.

Morris Have you observed at meter wavelengths? (To Pettengill.)

Pettengill It's absolutely impossible to attempt this type of experiment at 70 cm using Arecibo alone because of the rather long echo time.

Morris You would get about 30 min of return?

Pettengill About 25 or 30. There are reasons why the system is very inefficient under the particular conditions that apply to Saturn observations.

Pollack How about if you do it bistatically?

Pettengill Then you have to find another antenna that is not too much smaller. Even if you are not hampered by the round trip time, the system, which has particular problems for Arecibo, has less gain at the longer wavelengths and noise is up. This amounts to about a 15- or 20-db penalty, and, if you put 15 db on the S-band return shown in figure 1, where are you?

Irvine I think the X-band observations would be valuable because there are passive radio observations at that wavelength also.

Pollack It appears that 70 cm would provide a critical test.

Irvine All I am saying is that it is just one of the critical tests.

Pollack What are the possibilities of trying to do something at longer wavelengths by looking at a radio source that goes behind the rings of Saturn? You have a fairly large fraction of the sky covered by the rings, and celestial sources generally increase in strength as you go to the longer wavelengths.

Glenn Berge The statistics are pretty poor. It might be worthwhile to conduct a survey of sources along the Saturn track a couple of years in advance.

Pollack It is a different ball game than looking for occultations by the Galilean satellites because you have vastly more projected area.

Walter Jaffe Using the Westerbork telescope, in a week or so there might be several sources that would be occultated by Saturn. You would have to do a background search to pick out the individual sources and know exactly what

you were going to look at beforehand. If you have a very sensitive high-resolution telescope, you can find many more sources.

Pollack I think the Arecibo instrument would be a perfect survey telescope to use for this purpose.

Pettengill I agree, particularly if a relatively short baseline interferometer is available to reduce the confusion.

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Michael A. Janssen

SHORT WAVELENGTH RADIO OBSERVATIONS OF SATURN'S RINGS

So far we have not discussed the connection between the radar results for the rings and the rather interesting constraints supplied by passive radio measurements of Saturn. I will try to broach this subject here. This and the next presentation will be concerned with passive radio observations of Saturn's rings. The useful observations cover the wavelength range from about 1 mm out to 21 cm. I am going to talk in particular about the shorter wavelength results—from about 1 mm to about 2 cm—and Dr. Berge (see following contribution) will subsequently discuss the longer wavelength measurements.

This division of wavelength measurements is primarily due to the different instrumental techniques that are used for the two wavelength ranges. The most sensitive way of looking for radio emission from Saturn's rings uses the interferometric technique, whereby we may actually attempt to observe the brightness of the rings themselves as opposed to some combination of disk plus ring emission. Single antennas at radio wavelengths simply don't have the resolution to separate the ring contribution from the disk contribution. This has to be done with the only means that are available, e.g., waiting for the ring inclination to vary or trying to guess what the disk contribution alone will be. I will concentrate primarily on these points in order to define the constraints—or better, lack of constraints—which currently exist in this important wavelength range.

Now, with the possible exception of a couple of measurements near the 1-mm wavelength, the results of all the radio observations are consistent with the simple hypothesis that the rings don't exist at all. It is important to keep that clear in the following discussion. The classical and obvious interpretation of this fact has been that the rings become optically thin at radio wavelengths. This would occur if ring particle sizes were less than radio wavelengths. The radar result of course upsets this interpretation and requires a rethinking of the interpretation of the passive radio observations. Since the radar measurements are quite new,

my contribution to the rethinking is still pretty much in a preliminary stage—I make no pretense of a definitive analysis here.

The radar result appears to pose an interesting problem. It obviously indicates that there is a strong interaction with radio waves. The passive results give the opposite impression, and it is not immediately clear whether or not these are compatible results. The simplest case I can think of is shown as example 1. I would emphasize that this is a very simple-minded analysis, which I would like to use as a springboard to a bit more detailed discussion.

Example 1

A = area of isotropically scattering surface (Saturn disk area = 1)

R = reflectivity

T = physical temperature

From 13-cm radar:

$$AR = 1.2 \times 0.62 = 0.75$$

Contribution to Saturn disk temperature:

$$\Delta T_D = A(1 - R)T$$

Eliminate A :

$$R = \frac{1}{1 + (1/0.75)(\Delta T_D/T)}$$

Best radio limit:

$$\frac{\Delta T_D}{T} = < 0.1 \Rightarrow R > 0.88$$

Simple dielectric case:

$$R = \left(\frac{n-1}{n+1} \right)^2 \Rightarrow \eta > 33$$

$\epsilon > 1000$

Possibilities:

Iron meteorites?

Enhanced radar backscattering?

Small, low-loss particles?

I assume that there is something in the neighborhood of Saturn that can be described in essence by an area and a reflectivity. Let us assume isotropic scattering for this thing, and let us assume some physical temperature. Rephrasing the 13-cm radar results, we get the product of the area times the reflectivity to give the ring radar cross section area that Dr. Morris (see preceding contribution)

just gave us. Now, we can solve for the thermal emission from this reflector with the same model if we make some assumptions about the connection between reflectivity and emissivity. The contribution to the disk temperature of Saturn should be the area times the emissivity times the temperature. If we now eliminate the area between these two equations, we can solve for the reflectivity in terms of the contribution that we would expect for the disk temperature. (Note that I normalized the areas to the area of Saturn's disk.)

The best radio limit is that the contribution from the rings relative to that of the disk is on the order of 0.1. This gives us a reflectivity of about 0.88. Now, if we assume that this is due to a dielectric surface (e.g., if we want a physical model, we can think of large dielectric objects), we can take the Fresnel equation and solve for the refractive index. We find that the refractive index has to be greater than 33, or, in other words, the dielectric constant has to be something greater than 1000. Obviously this is a simple model. It is interesting that it fails to work by such a large factor.

Gordon Pettengill This assumes that some of the radio waves are getting absorbed. But suppose that the waves rattle around inside, as I suggested (see discussion in preceding contribution by Morris); there would be relatively little loss, and there would be no contribution to the temperature.

Janssen Yes. As a matter of fact, there are several ways around this, which I hope to lead into. This could be due to polished copper balls or something of the nature you suggest. The two sets of results could be made consistent with iron meteorites, for example, if the conductivities can be made high enough. Also, earlier we were discussing the possibility of enhanced radar back-scattering; one can imagine extreme cases. A third possibility which I find interesting is a model that Dr. Pollack has suggested and Dr. Pettengill has anticipated in his remark. That is, we simply have low loss particles, large enough to reflect 12-cm radar according to the Fresnel law but with little or no effective absorption and hence reduced emission.

Pettengill The Fresnel law merely says that one minus what you see as a reflection in the radar case had to come from inside or go inside reciprocity holds and that just isn't true in your example.

Janssen I would point out that for the case of most dielectrics of meter size, the internal rays are absorbed as, for example, with silicates.

Pettengill They are dry and cold. In the case of the Moon, they talk about hundreds of wavelengths, don't they?

James Pollack No, in the case of the Moon, it is more like 5 or 10 times the wavelength of observation.

Pettengill Of course, the Moon isn't as cold as this material is.

Pollack No, it is not. And that fact will probably lower the imaginary part of the index.

Pettengill But if you are talking about 10 cm to meter particles, surely at 6- or 10-mm wavelength you wouldn't encounter that limiting case, would you?

Janssen Well, meter size particles are 100 wavelengths already. The dielectric

constant, the loss tangent I have looked up for common materials, gives very significant attenuation over such distances.

Pettengill At these temperatures?

Janssen Yes.

Irvine That is probably for homogeneous particles. If you have some kind of a conglomerate with a lot of internal reflecting planes, such as a snowball at optical wavelengths, then it seems to me that you could significantly increase the reflectivity.

Janssen I would like to emphasize that the purpose of this example is to show that the simplest case you can use to explain these two sets of radio data runs into serious problems. In detail, it is obviously a very complicated problem. From this example, I think it is clear that in the general case we would expect to see some thermal emission. In the following, I want to concentrate on a somewhat more elaborate model, which I will use to reexamine the present millimeter wavelength limits on the ring brightness temperature. The passive radio data are not taken in such a way that we simply measure a ring contribution or a ring brightness temperature separately from Saturn's disk temperature. We need a model to extract this information, and I want to elaborate on a very simple model that can be made consistent with the high radar reflectivity.

There are two ways of separating out the ring contribution from the disk flux. First, we may look for variation in the total flux from Saturn over a long period of time in which the inclination of the rings varies significantly. The rings, when fully open, present a cross section area comparable to the disk area of Saturn.

Irvine Are there long-term programs to look for such an effect?

Janssen As far as I know, only Eugene Epstein of Aerospace has carried out a consistent program to observe Saturn. I will show you his data in a minute.

A second way of singling out the ring emission for a single observation is to estimate the disk contribution on an a priori basis and assign the difference between the expected disk contribution and the actual observed flux to the rings. This is obviously a very shaky technique, but it nevertheless supplies some kind of restraint. We will deal with this point first.

Table I lists the disk temperature measurements of Saturn that have been made over the last several years. These are depicted as a function of wavelength in figure 1. I have excluded some observations for which the errors were larger than 15 or 20 percent. Shortward of 2 cm, this effectively summarizes the data that presently exist. I would point out that there is a great question of calibration in these measurements since the millimeter flux scale is very uncertain. The error bars are those given by the experimenters, and no attempt has been made in this figure to find a common flux scale. Those cases where the experimenters make no attempt to give an absolute uncertainty are represented by open circles.

According to these data, the disk temperature of Saturn, taken to include the ring contribution, appears to be about 140°. Model atmosphere calculations of Saturn's disk temperature predict about this value for Saturn alone. The thermal

emission from Saturn is believed to be due to the presence of ammonia in small amounts in the atmosphere. Ammonia is a very good microwave absorber and has a strong inversion band at about 13 mm. At the pressures involved, this microwave absorption is broadened into the far wings and gives absorption over a broad range of wavelengths. Ammonia saturates in regions of the atmosphere cooler than about 160°. Detailed calculations for both Saturn and Jupiter, allowing for the many uncertainties about the atmospheres of these planets, do not allow for a disk temperature much colder than about 140°. To get a disk temperature of 100°, for example, we would have to have some mechanism for getting microwave

TABLE I.—Measurements of Saturn's disk temperature, 1 mm–2 cm.

λ , cm	B, degrees	T_D b. K ¹	T_D b/ T_D Ω ²	Reference
0.12	+10	140 ± 15	0.90 ± .14	Low & Davidson (1965)
.12	-26.4	169 ± (10) ³	1.15 ± .10	Harvey & Werner (1973)
.14	-26.4	194 ± (8)	1.29 ± .06	Rather (1973)
.21	-21	164 ± 12	1.00 ± .10	Ulich et al. (1973)
.31	-21	148 ± 11	.82 ± .10	Ulich et al. (1973)
.33	-1.5	125 ± (4)	.817 ± .026	Epstein et al. (1970)
.33	-6.9	124 ± (2)	.810 ± .013	Epstein et al. (1970)
.33	-12.7	123 ± (2)	.804 ± .013	Epstein et al. (1970)
.33	-17.9	129 ± (2)	.843 ± .013	Epstein et al. (1970)
.33	(⁴)	125 ± 13	.82 ± .02	Epstein et al. (1970)
.35	-15	132 ± (6)	.94 ± .04	Pauliny-Toth & Kellermann (1970)
.8	+9	132 ± (9)	.92 ± .06	Kutuza et al. (1965)
.82	-17	132 ± (4)	.92 ± .03	Kuzmin & Losovsky (1971)
.84	-18	151.1 ± 7	.96 ± .03 ⁵	Wrixon & Welch (1970)
.9	-24.8	134 ± (5.4)	.921 ± .037	Gary (1973)
.9	-26.6	147 ± (4)	1.010 ± .028	Gary (1973)
.9	(⁴)	142.4 ± (3.2)	.98 ± .02	Gary (1973)
.95	-15	127 ± (3)	.91 ± .02	Pauliny-Toth & Kellermann (1970)
.96	(⁶)	126 ± 6	.80 ± .06	Hobbs & Knapp (1971)
.98	-18	138 ± 6	1.06 ± .04 ⁵	Wrixon & Welch (1970)
1.18	-17	130.8 ± 6	.94 ± .04 ⁵	Wrixon & Welch (1970)
1.27	-18	127.2 ± 6	.935 ± .03 ⁵	Wrixon & Welch (1970)
1.46	-17	133.2 ± 8	.92 ± .07 ⁵	Wrixon & Welch (1970)
1.53	+10	141 ± (15)	.94 ± .10	Welch et al. (1966)
1.95	0	145 ± 4	.884 ± .05	Pauliny-Toth & Kellermann (1970)
2.07	-26.5	162 ± 3.8	.938 ± .04	Gary (1973)

¹ Saturn disk temperature errors without parentheses indicate authors' estimate of absolute error. Errors in parentheses are relative errors with no absolute error given.

² Errors in the ratio measurement are calculated where possible on the basis of relative rather than absolute errors.

³ Measurement relative to Venus assuming $T_D \Omega = 285$ K. Ratio to Jupiter assumes $T_D \Omega = 150$ K.

⁴ Averaged over preceding data.

⁵ Ratio to Jupiter based on data in Wrixon et al. (1971).

⁶ No data given.

absorption at a level of the atmosphere that cold. There doesn't seem to be a way to do it. In a gross attempt to remove some of the uncertainties about model atmospheres and the flux scale, I have ratioed the Saturn measurements to Jupiter where possible. These are shown in figure 2. We would expect Jupiter and Saturn to have approximately the same disk temperature, and the experimental uncertainties are reduced since we don't have to contend with calibration uncertainties. Interestingly enough, temperatures appear to lie somewhat to the cool side of Jupiter; all but about 3 points are below unity in figure 2. There are two interesting points significantly greater than unity, however. Both are recent measurements near 1 mm, one by John Rather (1973) at Kitt Peak with the 36-ft telescope and the other by Harvey and Werner (1973) of Cal Tech using the 200-in. telescope. Both were made with the rings wide open, and they do seem to show an excess. Observe that Low and Davidson's (1965) old 1-mm point, made at about the time the rings were seen edge on, is below the line.

It is not worthwhile to look too closely into the data represented in this fashion, since the measurements were made at different ring inclinations and there are spectral effects quite likely contained in those data. What I intend to do now is

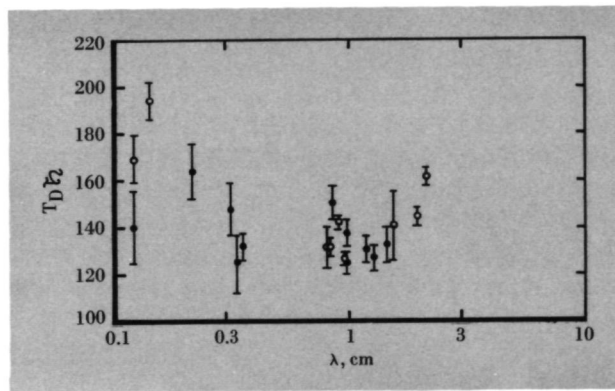


FIGURE 1.—*Microwave spectrum of Saturn, 1 to 20 mm. See text for distinction between open and filled circles.*

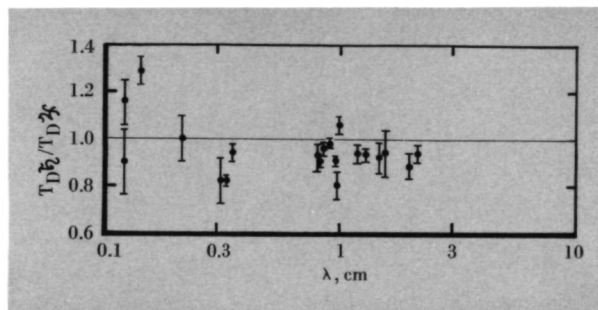


FIGURE 2.—*Observed ratio of the brightness temperature of Saturn to that of Jupiter for the wavelength range 1 to 20 mm.*

reinterpret these data in terms of a model suggested by the radar results. The classical interpretation has been that the rings are highly transparent at radio wavelengths, in which case the obvious approach would be to set an upper limit to their optical thickness. The radar results strongly imply that the rings are in fact optically thick, however, and to explain these small thermal contributions we postulate instead a low surface emissivity for the rings. Such a model may subsequently be interpreted in terms of small or moderate-size low loss dielectric particles; however, I will not attempt such an interpretation here.

Example 2 outlines a simple model in which I assume for simplicity that the rings are infinitely thick in the radio range. I will treat the ring surface as being characterized by an emissivity E such that the brightness temperature is proportional to the emissivity times the physical ring temperature. This is obviously much too simple a model for a detailed inquiry into its physics. My primary intent is to interpret the existing microwave observations, and I think the model, although it contains many implicit and undoubtedly erroneous assumptions, is compatible with the present uncertainty of the data.

Example 2

Rings:

Solid angle Ω_r
(A and B rings)
Temperature T_r
Surface emissivity E

Disk:

Ω_s Solid angle
 $\Omega_{s\phi}$ Unobscured solid angle
 T_s Brightness temperature

Optically Thick Rings:

$$T_D = \frac{\text{Disk} + \text{Rings} + \text{Disk reflected from rings}}{\Omega_{s\phi}}$$

$$T_D = \{\Omega_s T_s + E\Omega_r T_r + f(1 - E)\Omega_r T_s\} / \Omega_{s\phi}$$

The net disk temperature I have written as the sum of three terms: the product of the solid angle of the unobscured disk times the temperature of the Saturn disk (which I assume to be uniform); the product of the emissivity times the area of the rings times the ring temperature; and, since the reflectivity of the rings is high, the thermal emission from the disk reflected from the ring. This last term amounts to about 2 or 3 percent, which I would note is in principle measurable by interferometric techniques. I have normalized the disk temperature according to convention by dividing by the solid angle of the disk of Saturn.

Figure 3 shows the results of some computations based on this model. I have assumed that the temperature of Saturn's disk—the radio brightness temperature—is 150° , and that the physical ring temperature is 100° . These are simply round numbers and are not to be taken too seriously. I did the calculation implied

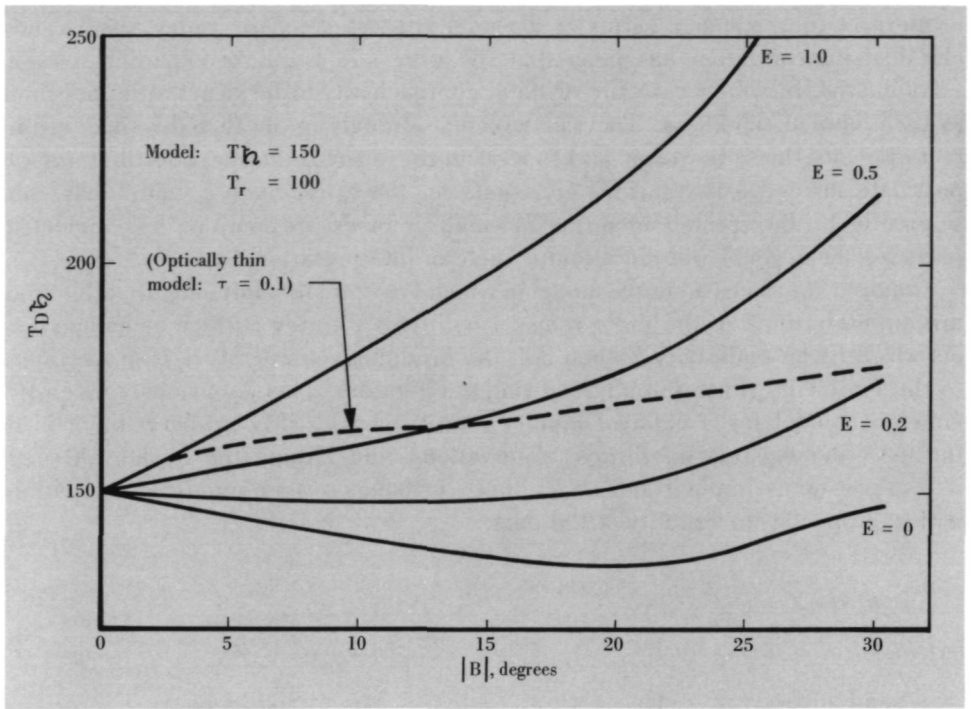


FIGURE 3.—Variation of apparent Saturn disk temperature with ring inclination for several values of emissivity E .

in example 2 to obtain the variation in disk temperature with ring inclination for several choices of the emissivity.

Now, as the rings open up, we see more ring area. If the rings had a large emissivity, we would expect to see a large contribution, as much as doubling the apparent disk temperature. If the rings are simply scatterers, however, as the rings open up their effect is only to obscure a portion of the disk. We won't see any thermal emission from the disk, and hence the disk temperature will appear to decrease. Beyond about 20° the outer ring begins to go off the disk and the disk temperature begins to go up in either case. It is interesting to note that, except for small but finite contributions from the rings, we don't get much of a variation until we get well out into very high inclinations.

This model can be criticized from several points of view. We are assuming a constant physical ring temperature, and of course we might allow that to vary as suggested previously. Another point is that if we treat more correctly the multiple scattering problem implied here, the emissivity is not really expected to be constant as we vary the angle at which we look at the rings; the emissivity will decrease somewhat, depending on the albedos of the individual particles in the medium. Third, we are taking the extreme limit of a large optical depth. For the case of an intermediate optical depth, as the rings open up the effect is in the opposite direction, and the curves will tend to have less slope to them as we approach the case

of optically thin rings illustrated by the dotted line. Notice that if we have an optically thin model, as soon as the rings open up slightly, we have an almost constant contribution and see no large effect with the ring inclination. Of course, we always have a positive contribution to the disk temperature in that case.

I am going to ignore these points and continue on with the basic case I discussed to see what interpretation we can make of the data. Figures 4 through 7 illustrate an attempt to fit the data I showed you previously in terms of this model. Here I have divided the data into four batches according to wavelength and plotted them as a function of the ring inclination. We will continue to work with the Saturn/Jupiter ratio measurements to remove as many uncertainties as possible. In fitting the model curves to the data, there are really two parameters we have free. In addition to the emissivity, there is a choice as to what the unobscured disk temperature of Saturn should be. We would expect it to be somewhere around unity.

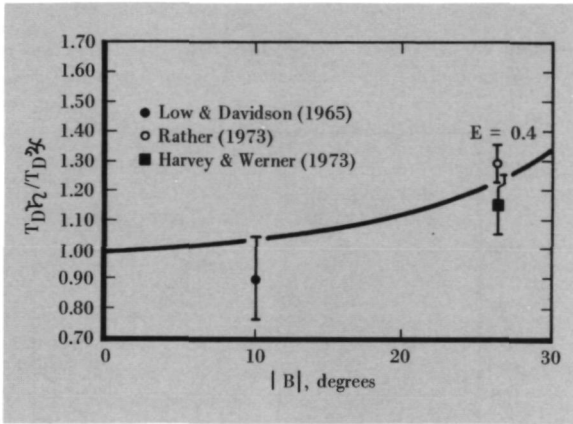


FIGURE 4.—Brightness temperature ratio as a function of ring inclination for observations near 1 mm.

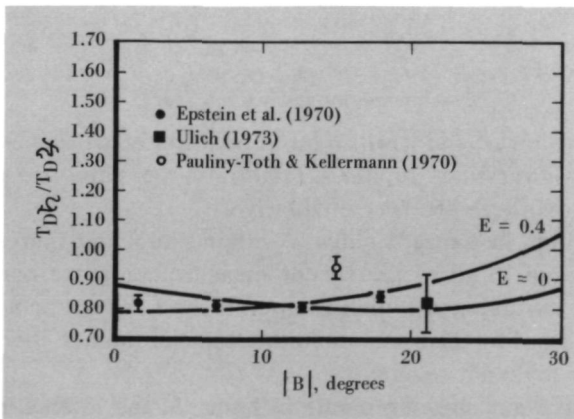


FIGURE 5.—Brightness temperature ratio as a function of ring inclination for observations near 3 mm.

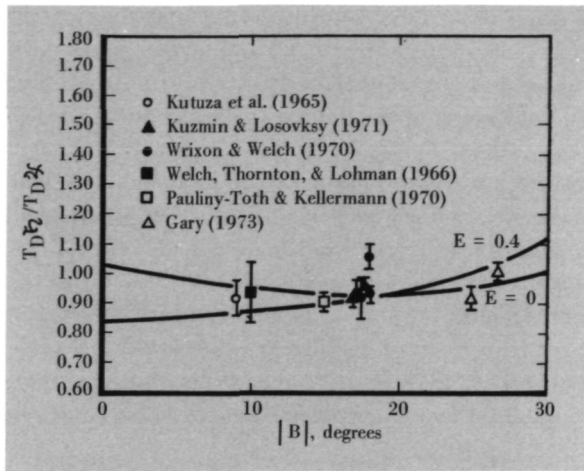


FIGURE 6.—Brightness temperature ratio as a function of ring inclination for observations between 8 and 15.3 mm.

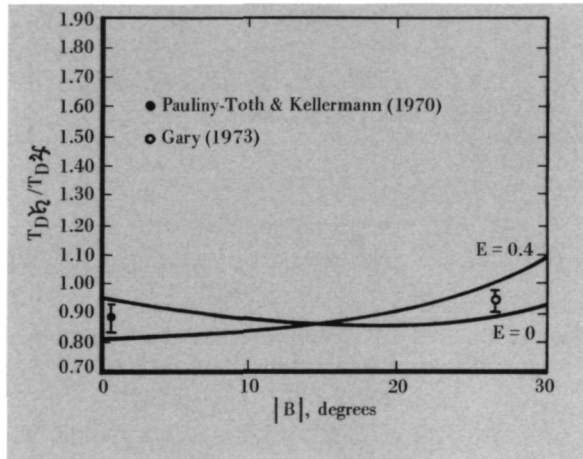


FIGURE 7.—Brightness temperature ratio as a function of ring inclination for observations near 2 cm.

It can be higher or lower, and it will want to be lower as we can see in the figures. As it gets much lower than Jupiter's brightness temperature, it becomes less likely. But the limits there are very qualitative.

The measurements in figure 4 show a rather significant increase as the rings become fully opened. Both of the recent measurements are consistent with an emissivity of 0.4, assuming the disk temperature of Saturn should be 150° . If it is less, as is suggested by the 3 mm and other measurements, the emissivity could actually be rather higher.

Referring to the 3-mm measurements in figure 5, the interesting points are by Epstein et al. (1970). He has made measurements from the period of about 1966 through the present, although he has only evaluated the data up to about 1969.

The ratio measurements he has made with Jupiter are very precise. His error bars are much smaller than any other errors in these figures. Nevertheless, with this small range they are still consistent with a large range in emissivity. There is no apparent effect with inclination, yet the model still fits the data for emissivities as high as 0.4. The best fit is about 0.25 for the emissivity.

The data from about 8 to 15 mm shown in figure 6 are much less conclusive. There are a good deal more data here, but there are no observations near the edge-on case. I have shown two recent points by Gary (1973) which may indicate an upturn as the rings open up, although one can still fit a large variety of curves through these points. And, again, for the 2-cm data, there is a statistical argument one can make with the data points as they are, and anyone can see that they cover about the same range of cases.

Epstein made measurements this last year, and it will be interesting to see those data reduced. I think that is probably the major improvement that can be made taking the present approach.

Note that the zero emissivity case for the simple model I have taken doesn't give the best fit to the data. Essentially the data, except for the 1-mm case, are consistent with a straight line. For the model I have taken, where we actually have a decrease and then an increase with the ring inclination, the fact that the data are flat gives us an indication that the rings are contributing to the disk temperature of Saturn. On the other hand, that is also saying that they are consistent with the absence of any rings at all; this is consistent with the optically thin case but not with the radar result. The best fit in all these cases, except perhaps near 1 mm, is in the neighborhood of $E=0.2$. Emissivity as high as 0.4 is less likely on the basis of the data, but it is certainly not excluded.

The limits at longer wavelengths are much tighter than this. The only indication that we have that the rings do exist at radio wavelengths are the high 1-mm points, and the inference may be that the rings gradually or suddenly disappear through the millimeter range with no really very strong constraints. For example, it is not clear that they have decreased in emissivity between 1 mm and 3 mm on the basis of present data. It will be shown in the next presentation, however, that beyond 2 cm we definitely have $E < 0.4$.

In summary, the most obvious interpretation of these data is that the rings disappear beyond 1 mm. However, it is shown here that we obtain a reasonable interpretation if we assume that the optical depth can be large while the effective emissivity of the rings is low. Generally, the limits on the thermal emission are not very strong in the millimeter-to-short-centimeter range.

Two things can be done in the near future to improve this state of affairs. First, the 3-mm points can be examined in their entirety. Also, an emissivity of 0.4 at 8 mm would suggest that there is something more than a 20-percent brightness contribution from outside the disk of Saturn. This should be easily detectable if it is present. We at JPL are presently developing an interferometer to work at this short wavelength which will be capable of detecting a much smaller ring contribution.

Finally, there remains a question as to whether such a model is really applicable

at all. This model does make definite predictions about what the microwave appearance of Saturn should be. There is at least one possible test here; the optically thick, low-emissivity rings would give the appearance of a cold band across the disk of the planet, and this could be searched for with an interferometer. In any case, there would be a minimum brightness temperature for the rings. If they are totally reflecting, they must reflect some thermal emission from the disk. That limit seems to be around the 2- or 3-percent level, which is marginably detectable at present by interferometric techniques.

DISCUSSION

Dennis Matson What is the relationship between the emissivity E you use and the effective radiometric emissivity for your model?

Michael Janssen The emissivity times the physical temperature of the rings, assuming that they are optically thick, gives me the spectral brightness temperature.

Matson At optical wavelengths, the rings are absorbing a certain amount of energy that must be disposed of.

James Pollack The emissivity could be very different at infrared wavelengths. Most of the radiation from Saturn's rings is emitted at infrared wavelengths. He (Janssen) is not giving an integral over all wavelength space. He is simply saying at a particular radio wavelength the emissivity could be very small. In his model, E is an unknown that he will try to deduce from the existing observations.

Gordon Pettengill Does 0.4 really seem so low when one takes into account all the other facts that we heard today? It doesn't sound so low to me.

Janssen Yes, it is interesting. I did do some multiple scattering calculations, assuming a thick isotropically scattering medium of small particles. I obtained some calculations from John Martonchik here at JPL for the case of varying optical depths at an inclination of about 25°. The emissivity in this case could be related directly to the particle single scattering albedo, assuming isotropic scattering. The single particle albedo for the optically thick case with an emissivity of 0.40 is 0.93.

Pettengill Suppose they are not optically thick at radio wavelengths. I don't think we have any evidence which convincingly demonstrates that they are. Optical wavelengths are one thing, and radio wavelengths are quite another. An experiment that detected a radio source through the rings would certainly be a nice way of settling it.

Janssen If we take a τ of 0.5 and an emissivity of 0.4, a single scattering albedo of 0.75 is required. The single scattering albedo doesn't go down tremendously fast with decreasing optical depth.

Pettengill The A and C rings likely possess a lower optical depth than ring B. The effective optical depth at radio wavelengths for the rings as a whole may well be less than 1.

Janssen However, if we go to very low values of optical depth, which are suggested by the longer wavelength interferometric data, an emissivity of 0.1 implies a much higher single scattering albedo. In the extreme case, you require something in excess of 1.

Pettengill It may well be that the radio optical depths are relatively small.

Pollack I think you have to be a little bit careful because the radar observation places severe constraints. If you make the optical depth much less than 1, you will never get the observed radar reflectivity.

Pettengill That is with your model. (See contribution by Pollack for a description of this model.)

Pollack If you want to get a large radar return at one wavelength, then for all wavelengths shorter than that the optical depth must have some reasonable value.

Pettengill I am suggesting that if these objects have a certain physical temperature, their ability to radiate effectively depends on the optical depth. That optical depth may be down around 0.1, in which case you won't see very much ring emission.

William Irvine You are using optical depth for absorption or emission.

Pettengill That's right.

Irvine Jim (Pollack) is using it as the total extinction optical depth, including that provided by the scattering.

Pettengill I am saying the objects are largely decoupled from their environment because they possess small absorptivity.

Pollack That is my model. I certainly won't disagree with you.

Pettengill Maybe your model with multiple scattering will do it, but I am not personally convinced that it is absolutely necessary to invoke the external multiple scattering effect.

Janssen The ring particles must be good scatterers in order to produce the radar return. The loss for ice at 1 cm at about 100 K is, as I remember, about 10^{-3} to 10^{-4} per cm. That gives you a path length for an eight-fold attenuation of many meters. So ice particles can be fairly large. I think the numbers you used in your calculation, Jim (Pollack), are a little more pessimistic, which says ice is more absorbing.

Pollack I was hoping very much that that would be the case. I used something that would be representative at room temperature, and I would be very delighted to hear your values at 100 K.

Pettengill I don't see why a high radar cross section requires a high emissivity.

Robert Murphy There is always the possibility that Saturn itself has changed temperature as a function of time.

Janssen That's true; I should have mentioned that when you ratio the Saturn measurements to those of Jupiter you do not avoid the problem of temporal changes in the temperature of either planet.

Murphy Yes; I wanted to point out that there are spatial variations on Jupiter that are significant. They are associated apparently with some cloud features. Saturn is just too small to really be sure.

Janssen Those aren't, however, necessarily related to the microwave brightness temperature.

Murphy Because the temperature is tied to the ammonia saturation level?

Janssen Yes. The problems associated with taking ratios could be avoided by making a set of measurements interferometrically, looking directly for emission from the rings. Then you don't have these ambiguities.

Irvine Is Epstein's the only long-term measurement set?

Janssen That's right. It is the only consistent effort that has been made to observe the rings over the full range of ring inclination. Otherwise all we can do is take the data and try to ratio it to take out systematic errors, as I have shown.

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Glenn L. Berge

MICROWAVE INTERFEROMETRY OF SATURN: APPLICATION TO THE BRIGHTNESS TEMPERATURE OF THE RINGS

I plan to speak about radio interferometric measurements of Saturn and Saturn's rings. This will limit my remarks to observations at longer wavelengths ($\lambda \geq 3$ cm), because until recently there were no interferometers operating at shorter wavelengths that were capable of observing Saturn. However, Mike Janssen has informed me of one exception: there are some existing measurements made with the Hat Creek interferometer at 1.2-cm wavelength, but it is not yet clear whether they are of sufficient quality to be of scientific value.

Table I lists the existing interferometer measurements of which I am aware, with the exception just noted, in chronological order. Since there are relatively few measurements, they can be discussed individually.

However, we should first consider the model visibility functions shown in figure 1. These may appear confusing at first, but they will help in understanding what follows. The curves represent the interferometer amplitude response V as a function of baseline length β . The latter is a normalized quantity obtained by multiplying the length (in wavelengths) by the radius (in radians) of the circular disk being observed. If the disk is elliptical, then $\beta = [(aA)^2 + (bB)^2]^{1/2}$, where a and b are the major and minor axes, respectively, and A and B are the corresponding baseline components.

The solid curve is the response to a uniform disk with a flux density of unity, and it passes through zero amplitude just beyond $\beta = 0.6$. For a very small baseline there is no resolution, which simply means that the disk subtends a negligible part of an interferometer fringe on the sky. When the response begins to drop, one knows that the size of the disk is no longer negligible compared to the fringe spacing. When the response finally reaches zero, that means the power is equally distributed between the positive and negative portions of a fringe.

The other two curves in figure 1 apply specifically to Saturn if there is also power being received from the rings. For each of the curves, it is assumed that

TABLE I.—Existing interferometer measurements of Saturn.

λ , cm	Dates	B	Instrument	Reference
10.4	September 29–October 11, 1965	+ 5°	Owens Valley interferometer	Berge & Read (1968)
9.0	September 19–26, 1966	– 1°		
21	October 29–November 10, 1970	– 22°	Owens Valley interferometer	Berge & Muhleman (1973)
21	November 1970	– 22°	Westerbork array	Jaffe (unpublished)
3.7	August 1971	– 25°	Owens Valley interferometer	Muhleman & Berge (unpublished)
21	August 12–15, 1971	– 25°	NRAO interferometer	Briggs (1973)
3.7, 11	April 1972	– 26°	NRAO interferometer	Briggs (unpublished)
6	December 1972	– 26°	Westerbork array	Jaffe (unpublished)

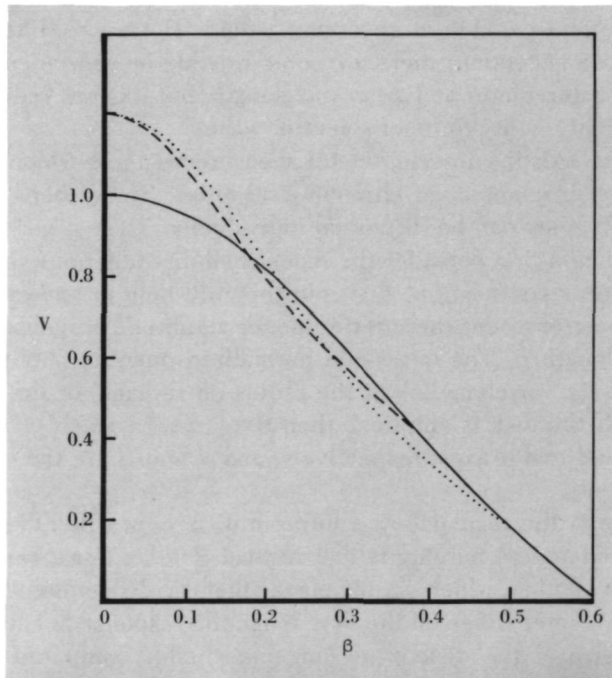


FIGURE 1.—Sample visibility functions. Solid curve: uniform elliptical disk. Dotted curve: uniform circular disk with uniformly bright A and B rings. The ring contribution is 20 percent as large as the disk contribution, and the baseline is parallel to Saturn's equator. Dashed curve: same as dotted case but with uniformly bright rings extending to 3 Saturn radii.

the baseline is parallel to Saturn's equator and that the additional flux density provided by the rings is 20 percent of that provided by the disk. The dotted curve assumes that there is uniform radio brightness for the area of the visible A and B rings. It appears that the best way to distinguish between the dotted and solid curves is to compare observations at $\beta \approx 0$ with observations at $\beta \approx 0.2$ or 0.3 . The dashed curve makes the same assumptions as the dotted curve, except that the uniformly bright rings extend to 3.0 Saturn radii instead of only 2.3 radii. In this case everything happens on a shorter scale in β .

I won't say very much about the first two entries in table I except to quote the final result. At the time the observations were made, the tilt of the rings was very small so that the angle subtended by them was small, and the upper limit to their brightness temperature was therefore quite large. The rings were not detected, and the upper limit just mentioned was 40 K at 10.4 cm.

Figure 2 shows averages of all of our 21-cm Saturn data (the third item in table I). The amplitude scale has not been normalized to unity; it represents the flux density that would be measured if the Earth-Saturn distance were 8 A.U. The three filled circles are the most accurate data points. The others are less accurate and were taken almost a year earlier. Because the latter are of poorer quality and because there may have been changes in that 1-year period, I'll consider only the filled circles.

The lower curve is simply a fit to the point with smallest β , assuming a uniform disk the size of Saturn. This curve matches the other filled circles very well. This, then, is the scheme for looking for ring emission. If part of the total emission were from a region the size of the visible rings, we would have expected less flux

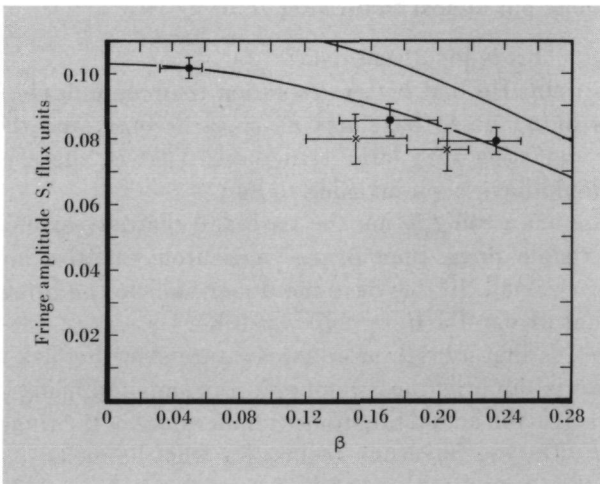


FIGURE 2.—Averaged 21-cm Saturn data. Crosses are from February 1970 and filled circles are from October and November 1970. The interferometer baseline was east-west. The lower curve is described in the text; the upper curve may be disregarded.

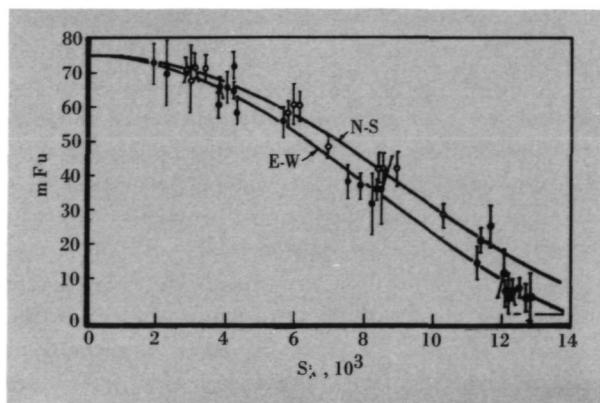


FIGURE 3.—Observed 21-cm visibility function for Saturn. Filled circles are for a baseline approximately parallel to Saturn's equator and open circles are for a baseline approximately parallel to Saturn's rotation axis. The curves are the expected response for the two cases assuming a uniform elliptical disk.

density at the larger values of β . However, a uniform disk the size of Saturn with no contribution from the rings fits these data perfectly well. The uncertainties could allow a small ring contribution to be present, so we determined an upper limit to this contribution. The upper limit to the brightness temperature of the rings at 21 cm (for $B = -22^\circ$) was 10 K.

Figure 3 shows the corresponding observations by Briggs (1973). He has plotted every point without taking averages. Neither axis has been normalized. You can see that he has gone out almost to the first zero.

William Irvine This is his 21-cm data?

Berge That's right. He had better resolution than we did. On the other hand, his observations didn't go to baselines as short as ours, and therefore he was less sensitive in detecting very large structures. That is, there could be a very large ring that would have been invisible to him.

Assuming that such a thing is not the case and that any extended emission is confined to the visible rings, then Briggs' measurements also indicate that this contribution is very small. In this case the upper limit to the brightness temperature of the rings at 21 cm (for $B = -25^\circ$) was 6 K.

Von Eshleman Is that a brightness temperature over the disk of Saturn?

Berge No, that is the brightness temperature computed using the angular size of the rings. Thus, it is the actual brightness temperature of the rings.

James Pollack Do you have any feeling for what happens to these results if you don't allow the portion of the disk of Saturn that is behind the rings to come through the rings?

Berge I'll get to that in a minute.

The main point I am making here is that we have some very low upper limits to the brightness temperature of the rings, if we can assume that the model we

have used for the rings is correct. If, on the other hand, the emission should extend to a much larger radius than the visible rings, we would be less sensitive to seeing it. Briggs, in particular, would be less sensitive because he doesn't go to such short baselines. An interesting effect of the model fitting is that if the assumed size of the rings is allowed to increase, then even though the upper limit on their flux density contribution rises significantly, the upper limit for the brightness temperature may stay about the same.

Walter Jaffe is with us today, and I have talked very briefly with him about his results. By looking qualitatively at the results, he sees no obvious evidence of rings. However, he has not interpreted the data quantitatively yet in terms of upper limits. If you have any questions, I think perhaps he might be willing to answer them later.

Pollack Jaffe's observations are at what wavelength, Glenn?

Berge He has measurements at 6 cm and at 21 cm.

At this point I am going to go beyond the scope of my title a little bit. I am going to leave the subject of the brightness temperatures and consider the question that Jim Pollack raised a moment ago. Namely, what happens if there is extinction at radio wavelengths by the rings so that, at a time like the present when part of the disk is substantially obscured by the rings, there is a dark strip across the disk of Saturn? Obviously, the best way to observe this sort of effect is to have a baseline oriented in the direction of Saturn's polar axis. Then it should be quite a simple matter to see the modification of the visibility function. In fact, I believe Briggs has some data of this very type obtained in 1972. I don't know what his results are or if he has any yet.²

However, even in the case of the east-west measurements, there is also a modification of the visibility function. For example, in the summer of 1971 when Briggs observed at 21 cm and we observed at 3.7 cm, the B ring and perhaps a little of the A ring were obscuring the higher latitudes in one hemisphere of Saturn. Since the part that is obscured has a smaller east-west size than the disk, the overall size should increase. I find that for the situation existing in the summer of 1971, the east-west size as measured by the position of the first zero in the visibility function should increase by about 5 percent due to this effect, assuming that the extinction is total. It's roughly linear in the sense that if the extinction is only 50 percent, then I would expect the zero crossing to move only by 2.5 percent instead of 5 percent.

Irvine Why does the overall size appear to increase?

Pollack I think the point is that if you consider strips parallel to the equator, the mean length of those strips will be bigger when you delete the poles than when you include the poles.

Berge The polar regions are smaller in their east-west extent. I think you can see that if I could extinguish both polar regions, then what remains is broader

² Editors' note: Briggs, in a paper recently submitted to the *Astrophysical Journal* describing his 3.7- and 11-cm measurements made in 1972, does find considerable extinction by the rings.

than the disk as a whole, and taking out just one of them does the same thing qualitatively.

Gordon Pettengill Wouldn't it have a greater effect north-south?

Berge Yes, and that is why I mentioned before that one should really use polar baselines.

It is interesting to note that in Briggs' results at 21 cm, the size that he fits for the disk is in fact a bit larger than the visible disk. When he fits his data, he finds that the curve comes down and crosses zero a little bit earlier than expected, which indicates a size slightly larger than the visible size. This occurs whether he simply fits to the disk or whether he fits to the disk plus the ring. In either case he gets a result that is about 2 percent too large for the size of the disk.

Moreover, that is based on the radius given in the *American Ephemeris and Nautical Almanac*, which is perhaps a bit too large. I notice that in a review paper by Cook, Franklin, and Palluconi (1973), they suggest a radius that is 1 percent less. That would mean that Briggs' result for the size is 3 percent too large.

Pettengill You say this occurs whether or not he fits with the rings included?

Berge That's right.

Pettengill How does he include the rings? What weight does he give to the extinction and emission? It certainly wouldn't be true if he assigned the rings the same value as the disk of Saturn.

Berge I see what you mean, I think. What he is fitting in one case is simply the size of the disk and its temperature. In the other case he is fitting the size of the disk, its temperature, and the temperature of the rings. He is then adding an extra parameter.

Pettengill He must be assuming something because surely if he assigned unit emissivity for the rings it would make a gross difference.

Berge No, he doesn't have to assign an emissivity, because he is simply solving for the observed brightness of the rings. He does assume that where they cover the disk, the temperature is the disk temperature plus the ring temperature, and this requires zero extinction.

Hugh Kieffer Is it not possible to maintain a constant enough calibration to see a difference in the total flux as the planet is obscured by the rings? In other words, how much does the total flux from the entire system at these wavelengths decrease?

Berge Well, this is the sort of thing that Mike Janssen (see contribution by Janssen) was talking about for shorter wavelengths.

Kieffer It hasn't been done at longer wavelengths?

Berge To date, there hasn't been sufficient accuracy or sufficient repeatability to do that sort of thing, especially at 21 cm. At this wavelength the confusion from background sources is comparable to the total flux density from Saturn for both of the instruments. Clever techniques have been used to remove it, but it is still a big problem in measuring the total flux density, at least at such a long wavelength.

Perhaps I am spending too much time on this point because the 2-percent

discrepancy that Briggs finds is only a one σ result, although if you make it 3 percent, then it is 3 ± 2 percent.

The interesting thing is that at 3.7 cm at the same time we get almost the same result. That is, compared to a radius 1 percent smaller than the American Ephemeris value, it appears that the east-west radio size is 3 percent too large. Based on the scatter of our data, the error is slightly less than 1 percent, but systematic errors may increase this significantly.

One thing I should have mentioned earlier when I was talking about temperatures is that for the 3.7-cm measurements just discussed, we have also set some limits for the temperature of the rings. We find that the ring contribution compared to the disk contribution is about 3 ± 3 percent. But we also find that if we raise the size of the rings to an outer radius of 2.65 Saturn radii, a fit to the data gives a ring contribution that is almost significant. The formal result is 8 ± 3 percent for the ratio of ring contribution to disk contribution. Even so, this amounts to only about 5 K for the brightness temperature for the rings.

Brad Smith You say the data would fit if you increased the radius of the rings?

Berge That's right.

Smith You would include ring A?

Berge Yes, if we include the B ring and the A ring, and then something else that goes out to 2.65 Saturn radii in such a way that the whole system is uniformly bright, then we get the numbers that I quoted.

Smith Where does ring A fall? This is beyond the edge of ring A, then?

Pollack The A ring terminates at 2.23 Saturn radii.

Berge Yes, we are treating the ring system, B, A, and D', let's call it, as uniformly bright.

Smith Optically, ring A falls off in brightness considerably from the Cassini division to its outer edge.

Berge That's right. So our model is probably wrong in detail.

George Morris Did that diameter optimize the temperature of the rings?

Berge Yes, that radius provides the maximum ring temperature.

Pollack And what is that temperature in degrees Kelvin?

Berge 5 K.

Pollack Eight percent is 5 K?

Berge Yes, using the angular extent of this extended ring system.

Pollack Does this analysis allow for obscuration of the disk by the rings?

Berge No, this analysis does not allow for the obscuration. Another point is that the size we determine for the disk from our data is also apparently well separated from whatever we do to the rings. We seem to need a larger size for the disk no matter what we do to the rings. No one should place too much weight on the result I just discussed because systematic effects may be giving us a fictitious result. My purpose in mentioning the result was to point out that, for our data, it appears that a small ring contribution is consistent with a ring of larger dimensions than the visual rings.

I was planning to say something about the possibility and value of observing an occultation of a radio source by Saturn's rings. However, we have already covered that subject pretty well, and I think some people are interested in it. It seems to me to be a valuable thing to do if one can survey beforehand to look for appropriate sources to be occulted.

Mike Janssen mentioned the subject of scattering of disk emission by the rings, which one would expect to be a few percent. We are certainly at the level of a few percent now, and, with any increase in accuracy, one should expect to see, if not emission, at least radiation scattered by the rings from the disk.

I don't think that I need to go into any physical interpretation of these results, because that was done quite thoroughly in the previous presentation.

It seems clear that the radar and radio results are by no means inconsistent, but between them they certainly offer some strong constraints on ring models.

DISCUSSION

William Irvine Glenn, have any of the 3.7-cm results been published?

Glenn Berge No. The 3.7-cm observations that we have are still in the stage of being interpreted. I don't know how soon Briggs' new measurements might be published.

Hugh Kieffer How far are we in terms of experimental observations from the point where, by looking for the reflected thermal energy from Saturn, you could make a statement as to whether the reflection coefficient of the planet-rings-Earth was less than the required radar reflection coefficient? How far is the experimental technique from being able to say something about the reflection phase function which the rings might have?

Gordon Pettengill How would you distinguish reflection from the direct emission?

Kieffer I am assuming we can separate them. You do expect to see something from the rings, even if it's only radiation from the disk reflected off the rings.

Berge My colleague, Duane Muhleman, has made some scattering calculations, but of course they require some assumptions. For example, if you assume that the ring is composed of isotropic scatters and there is no shadowing, then the scattering one expects to see is 5 percent of what you get for the disk itself, and we are certainly at that level now.

Kieffer It seems to me we are at the point here of having an additional technique or additional way of interpreting the data which may lead to the light.

Pettengill You have to take a guess at what the effective scattering is in the bi-static geometry of the disk-rings-Earth.

Berge Yes, that's the tough part.

Sam Gulkis Have you done the Saturn limb-darkening calculation at the long wavelengths? I am wondering whether or not the effective disk size is really smaller and the effects you are seeing are even larger than the 3 percent.

Berge That is a good point, Sam. I should have mentioned that one can explain these size results by assuming limb brightening, but that isn't likely. There is limb darkening, probably, and limb darkening makes the size discrepancy even

larger. One could indeed have total extinction for the rings and still be consistent with the data if you put in adequate limb darkening.

Irvine You could have preferential darkening toward the poles.

Berge That would make it look larger east-west and smaller north-south to the interferometer.

Irvine But isn't that the kind of thing that you get with ring extinction?

Berge That's right. Polar cooling is qualitatively the same as the obscuration effect. That is another possibility that I didn't mention.

Irvine Do you have any idea what the magnitude of that might be from the models you have used?

Michael Janssen It should be small. In the major planets, ammonia absorption, as we understand it, is quite an effective thermostat. While you might physically have a great change in the temperature as you go toward the poles, the ammonia will just shift around such that it will tend to cancel out the effect. There is no reason to expect polar cooling on Saturn or Jupiter.

James Pollack The point is that optical depth unity usually is found in a very narrow range of possible temperatures.

Gulkis That isn't necessarily true at the longer wavelengths where you're off the saturation curve. Once you break through the ammonia cloud, you are again tied very closely to the lapse rate and NH_3 abundance in the atmosphere.

Dave Morrison Glenn, did you give the transmission of the ring if we accept the uniform disk and the 3-percent discrepancy in radius?

Berge If there is no polar cooling and if there is no limb darkening, then the transmission is 2/5, using these very tentative data.

Pollack That's like an optical depth of unity, as good as the observations are.

Robert Murphy The models you used contain ammonia. But there is no direct evidence of ammonia on Saturn. If it is not there, I gather, we are in worse trouble, because then you should get a higher temperature from deeper in the atmosphere. Is that correct?

Berge Yes.

Murphy We should bear in mind that there is no evidence for ammonia.

Berge Perhaps the radio data is the best evidence at the moment.

Murphy If we could understand the rings under those assumptions, yes.

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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 $2 \mu\text{m}$ absorption feature and on the very strong absorption feature at $1.5 \mu\text{m}$. 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 $2 \mu\text{m}$ band and the $1.5 \mu\text{m}$ band can consistently be fit with a mean particle radius on the order of about $25 \mu\text{m}$ or a mean diameter on the order of $50 \mu\text{m}$. This number corresponds very nicely with the $100 \mu\text{m}$ 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

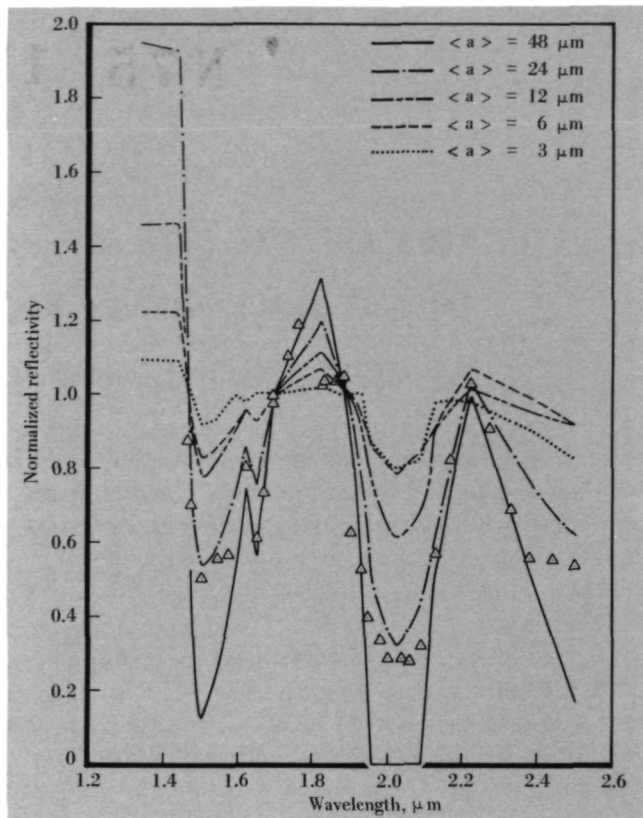


FIGURE 1.—Spectral reflectivity of Saturn's rings. Triangles indicate observations by Kuiper et al. (1970). Theoretical curves for ice are labelled by the value of mean particle radius. Both theoretical and observed values have been normalized to unity at 1.7 μm and the calculations have used a value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

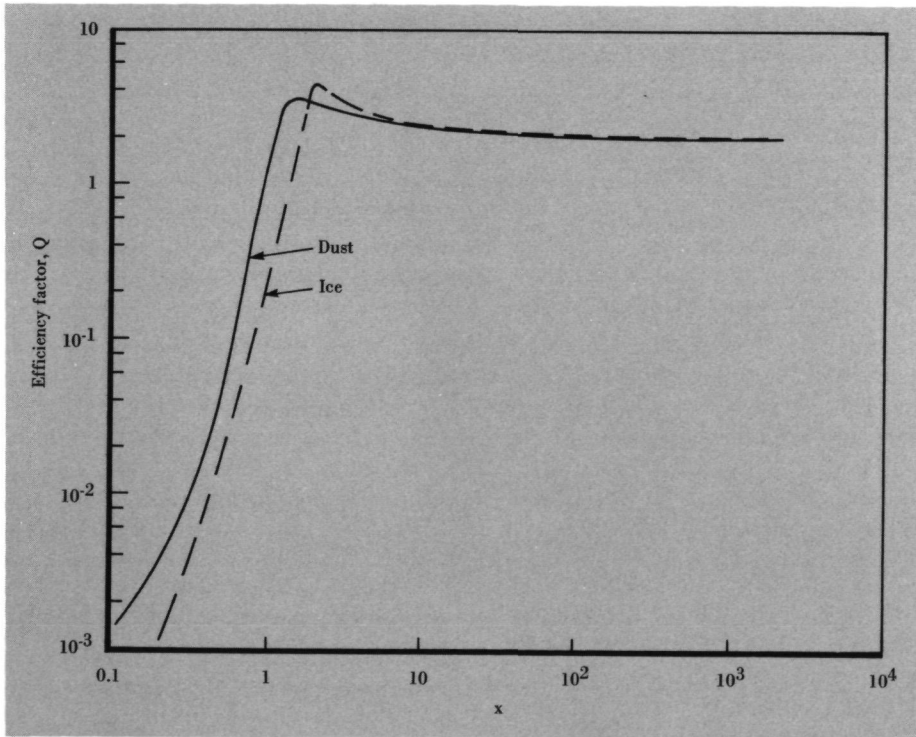


FIGURE 2.—Extinction efficiency Q as a function of x for pure ice and silicate particles at radio wavelengths. x is the ratio of particle circumference to wavelength.

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

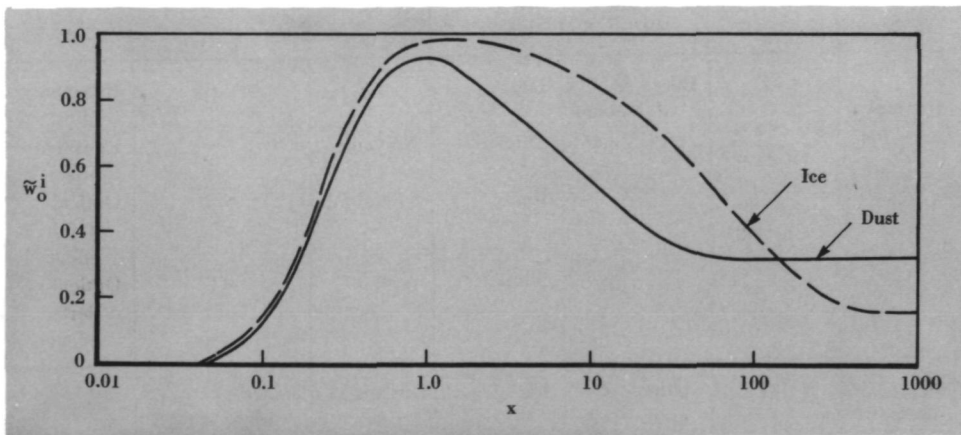


FIGURE 3.—Isotropic single scattering albedo \bar{w}_0^i as a function of x at radio wavelengths for pure ice and silicate particles.

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 Silicate	> 1 > 1	≥ 2 ≥ 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 think 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 occultation. 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.

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Jacques Blamont

THE "ATMOSPHERE" OF THE RINGS OF SATURN

The question I would like to address is the outgassing of Saturn's rings and the possibility of detecting it.

After discovery of the hydrogen Lyman-alpha emission around comet Tago Sako Kosaka by OAO II, observations we made on comets Bennett and Encke from OGO V in 1971 have shown that it is possible to deduce the source intensity of the hydrogen atoms from the Lyman-alpha emission intensity.

The total source intensity was of the order of 10^{29} atoms of hydrogen per second for comet Bennett and something like 10^{27} atoms of hydrogen per second for Encke.

Comets may well be ice-covered bodies, and this is also probably true for Saturn's rings.

I would like to make one suggestion. Dr. Franklin mentioned yesterday that it would be interesting to obtain the scattering diagram of an ice particle in space. Maybe one could obtain such a diagram from a comet nucleus, at a distance of the order of two A.U. from the Sun when the comet, being far away from the Sun, does not change from day to day. You just have to wait for a comet and there are 15 comets a year to try.

From our comet observations, we decided to look for the possibility of the same outgassing of H and OH from Saturn's rings. This work was carried out by my student, Michel Dennefeld.

First of all, the source of gas around Saturn's rings is computed. The products of ice will be essentially H_2O , H, and OH. There are different physical mechanisms for producing these species, and the corresponding production rates are given in table I. In order to decide if the source is significant, these rates can be compared with the rate for comets, which is of the order of $10^{28} s^{-1}$; that is a significant source and can be detected easily, but a source of $10^{20} s^{-1}$ cannot be detected.

Ice sublimation at a temperature of 80 K is a source of the order of $10^{20} s^{-1}$.

However, I heard yesterday that the temperature may be as high as 95 K. Then we have a source intensity like $5 \times 10^{26} \text{ s}^{-1}$, which is in the range of detectability.

If the temperature is 80 K, the most intense source is meteoroid bombardment. We have used the Cook and Franklin (1970) value for meteoroidal bombardment. In this mechanism, the meteoroid hits a particle, and local heating vaporizes H_2O .

Solar wind bombardment does not have a large effect.

Interstellar gas (hydrogen, helium), which hits Saturn's rings, has a small effect.

The most intense source of gas in the whole Saturnian system is the Titan atmosphere, which produces $4 \times 10^{28} \text{ s}^{-1}$ of atomic hydrogen.

For OH, bombardment by solar wind creates 10^{24} radicals per second.

Once the source strength has been computed, the subsequent history of the gases has to be determined.

Table II gives the lifetimes of those molecules and atoms in the vicinity of Saturn. All the lifetimes are long compared to the orbital period of atoms, molecules, or radicals around Saturn. For instance, the photodissociation time of H_2O , approximately 10^7 s , is 10^3 times the orbital period; therefore, something like

TABLE I.—Sources and production rates.

Species	Source	Rate, s^{-1}
H_2O	Ice sublimation $T=80 \text{ K}$	1.4×10^{20}
	$T=100 \text{ K}$	5×10^{26}
	Meteoroid bombardment	4×10^{26}
	Solar wind bombardment	5×10^{25}
	Interstellar helium bombardment	5×10^{24}
H	Solar wind bombardment	10^{23}
	Titan	4×10^{28}
OH	Solar wind bombardment	10^{24}

TABLE II.—Lifetime at Saturn.

Atom or molecule	Lifetime, s	Process
H_2	3×10^9	Photodissociation $\rightarrow \text{H} + \text{H}$
H	15×10^8	
H_2O	9×10^6	Photodissociation
OH	6.5×10^7	Photodissociation
Orbital period	4×10^4	

1000 encounters between one molecule of H_2O and the ring will take place before the production of an atom of hydrogen.

The sticking coefficient of H_2O over the ring is unknown since the very nature of the ring is unknown. It has to be of the order of 10^{-2} if we want to have a serious production of hydrogen. However, if we obtain a large source by having a higher temperature of the rings (about 100 K), we may, even with a larger sticking coefficient, obtain quite a number of hydrogen atoms.

After the computation of the lifetime, the distribution of molecules and atoms among the different orbits has been determined.

The number densities of different species are given in table III.

The orbits depend greatly on the formation processes, because the velocity is a function of the energy of the atoms or molecules.

For instance, an atom of hydrogen, when created by bombardment, will have a very low velocity on the order of 1 km s^{-1} ; if it has been created by photodissociation of H_2O , it will have a velocity around 18 km s^{-1} . Therefore, hydrogen created by bombardment will stay in the plane of the ring; if it has been created by photodissociation of water, it will fill a sphere around Saturn.

After all processes have been taken into account, a number density of 10 atoms of hydrogen per cm^3 is formed from outgassing of Saturn's rings.

Table IV gives the luminosity expressed in Rayleighs (1 Rayleigh corresponds to 10^6 photons per m^2 emitted in 4π steradians). Around the rings of Saturn, an emission strength of 10 Rayleighs of Lyman-alpha and 10 Rayleighs of OH resonance line has to be expected. The intensity of 500 Rayleighs of Lyman-alpha emitted by the Titan torus has been computed in the case of an optically thin

TABLE III. — *Number densities of species.*

Species	Number density, cm^{-3}
H_2O	10–100
OH	1–10
O	1–10
H (Saturn)	1–10
H (Titan)	10–10 ³

TABLE IV. — *Luminance.*

Species (and source)	Luminosity, Rayleighs
H (Titan)	500–1000
H (Saturn's rings)	1–10
OH (Saturn's rings)	10

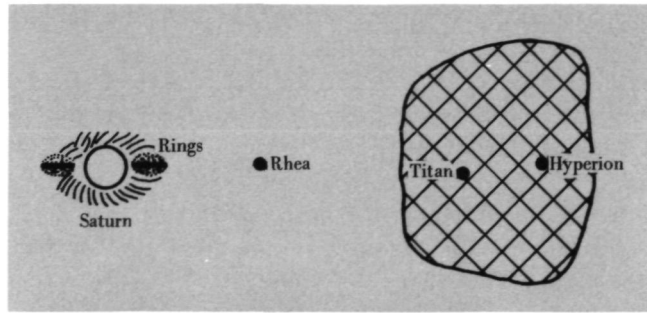


FIGURE 1.—Schematic view of a cross section through the Saturn system as seen in Lyman-alpha.

medium. Actually, the center of the torus is optically thick, and an exact computation for an optical thickness of 4 would provide an emission intensity of 100 Rayleighs.

This is a very easily detected phenomenon from MJS.

Figure 1 gives you some feeling for what it would look like. This would be the system seen in Lyman-alpha. It has an odd shape because of the properties of the orbits of the atoms of hydrogen around Titan, but it should be the most spectacular phenomenon in the Saturn system in the ultraviolet. Around the rings you have a very small emission of Lyman-alpha due to the atoms that have been created by meteoroid bombardment, and all around the system you will have a weak but detectable glow due to the atoms created by photodissociation.

DISCUSSION

James Pollack How did you compute the evaporation rate of the water vapor from the ring particles?

Jacques Blamont It is a cocktail of basic thermodynamics and measurements that have been published.

Charles Lillie What is the extent of that cloud around Titan?

Blamont It's of the order of 1 Mkm in diameter. As seen from the Earth, it would have a diameter of 5 min of arc. The existence of the torus has been proposed by Neil Brice (1973) and was discussed extensively last week at the Titan atmosphere workshop held at the Ames Research Center.

William Irvine You are saying that Titan is the source for that?

Blamont Yes.

Irvine You have to assume things about the atmosphere of Titan to make that prediction.

Blamont You have to assume an exospheric temperature of 100 K and a flux of hydrogen, which I think almost everyone agreed on at the Titan atmosphere workshop.

Brad Smith This would be a toroidal ring that would surround Saturn?

Pollack Yes, essentially because Titan loses hydrogen fairly rapidly. We have evidence that there is a significant amount of hydrogen in its atmosphere, which would suggest that a steady state has been established.

Lillie We had some observations of Saturn in Lyman-alpha with the satellite OAO II. It is possible to set upper limits on the surface brightness around the system. The photometer had a 10-min field of view, and it observed a surface brightness, assuming it is filled, of about 59 Rayleighs, plus or minus about 30. It is dominated by the noise of the system.

Blamont You mean around Titan or around Saturn?

Lillie This observation was centered on Saturn.

Blamont That is quite comparable to what I am talking about; I am thinking of 100 Rayleighs for Titan.

Lillie We are almost just about right. You are almost permitted.

Irvine Do you know if Titan was in the field of view?

Lillie Yes, I think it would have been. I don't know the distance of Titan from the primary at the time. I don't know the radius of Titan's orbit.

Fred Franklin It is 20 Saturn radii.

Lillie All that has to be in the instrument field of view is the toroid; you don't have to see Titan physically. As long as you hit the toroid, it's in the bag.

Blamont Yes, but the toroid is an extended source, with varying optical thickness in Lyman-alpha.

Lillie I calculated that if we could get about a 70-Rayleigh toroidal cloud, we would come out about right, which is permitted by the observation.

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Charles F. Lillie

THE MARINER JUPITER/SATURN PHOTOPOLARIMETER EXPERIMENT

I will discuss the observations of Saturn's rings we plan to make with our photopolarimetry experiment. I will consider only Saturn's rings, although, of course, we also have quite interesting scientific objectives that deal with the planets and their satellites.

The MJS photopolarimeter is basically a simple, general-purpose photometer which also has a capability for making polarization measurements. Figure 1 shows the instrument's location on the scan platform, boresighted with the television, ultraviolet spectrometer, and infrared spectrometer experiments.

Figure 2 illustrates the conceptual design of the instrument. It consists of a 6-in. Cassegrainian telescope, photomultiplier tube, aperture plate, analyzer wheel, and filter wheel. It is a simple instrument designed to make measurements of intensity and polarization at 7 wavelengths in the 2200 Å to 7300 Å spectral region.

The aperture plate provides four alternative fields of view, 4°, 1°, 1/4°, and 1/16° in diameter. The analyzer and filter wheels are shown in figure 3. Each wheel has eight positions. The filter wheel contains seven filters and a calibration source. The analyzer wheel contains a dark slide, an open position, and two sets of polarizing filters with different orientations so that we can make measurements of the polarization of light reflected from the rings of Saturn, for example. One set of polarizers is used for faint object measurements; the other set is combined with neutral density filters and is used for planetary observations.

James Pollack How many polarizer orientations will you have?

Lillie There are three orientations for each set of polarizers: 0°, 60°, and 120°. Our standard sequence is to make an open measurement and then three measurements through these polarizing filters.

We have tried to select filter band passes which isolate the various spectral features in the atmospheres of the outer planets. For example, at 2200 Å there is

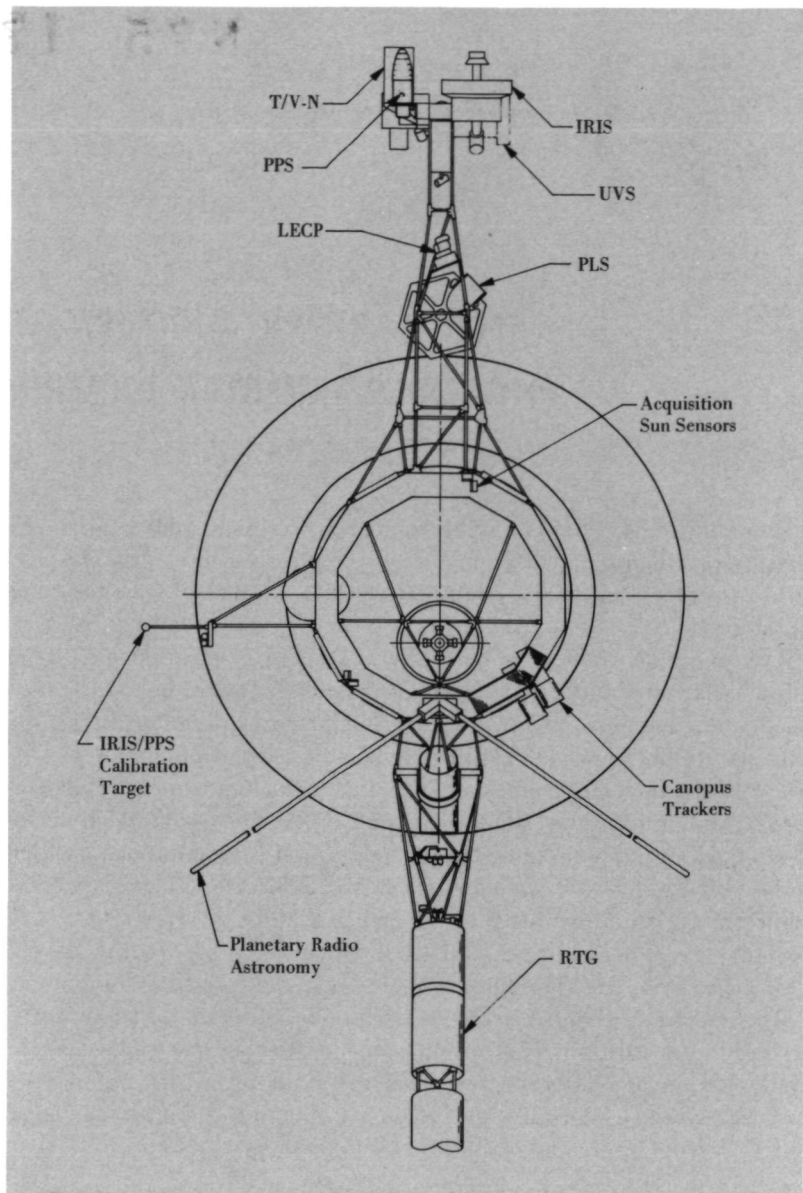


FIGURE 1.—Spacecraft configuration showing location of photopolarimeter (PPS) on scan platform.

an ammonia absorption edge, so the principal source of opacity in this band pass will be ammonia. The $7270\text{-}\text{\AA}$ filter is centered on a methane absorption band, so it is selective for methane.

In light of what Jacques Blamont said (see preceding contribution), our $3150\text{-}\text{\AA}$ filter is selective for OH emission at 3090 , so we will have a sensitivity for OH if it is present near the rings of Saturn.

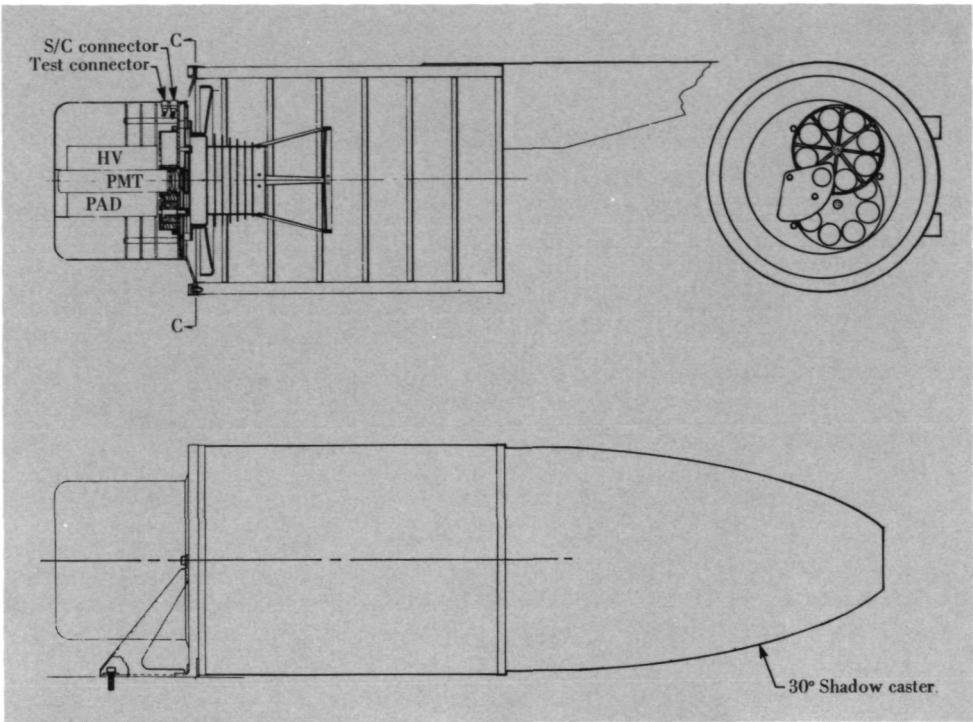


FIGURE 2.—Conceptual design of photopolarimeter.

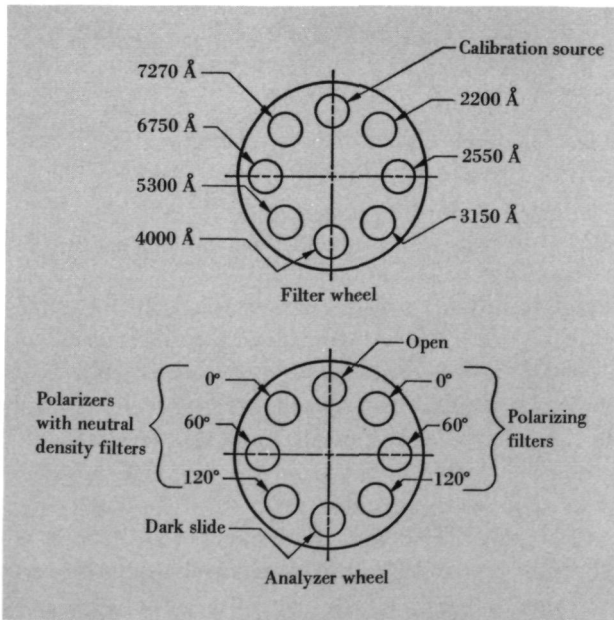


FIGURE 3.—Configuration of filter and analyzer wheels.

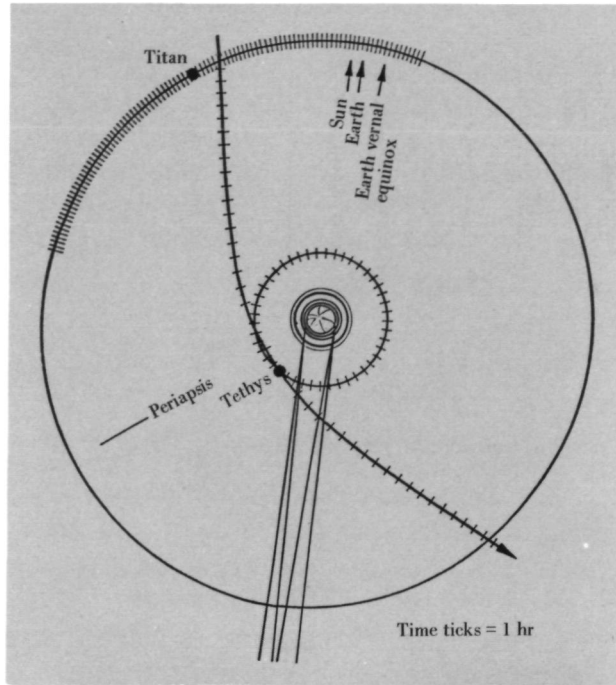


FIGURE 4.—JSX from north pole of Saturn trajectory.

Figure 4 shows the path of a sample trajectory (JSX) during encounter with Saturn. The unique thing about this experiment is not the instrument itself but the fact that it is on a spacecraft being sent to the vicinity of Saturn. This offers us the opportunity to make the same sort of measurements that we do from the ground, only in situ, with different geometries than those we can obtain from Earth and of course at much higher spatial resolution. The advantage of going to Saturn is demonstrated by the fact that at closest approach the spacecraft will be about 3600 times closer to Saturn than the Earth ever is. Thus, a feature that subtends an angle of 1 arc sec as seen from Earth will subtend an angle of 1° when seen from the spacecraft near Saturn.

We have the possibility of making observations with much higher spatial resolution, and the big, bright scattering disk that interferes with ground-based observations will be a long way from our field of view as we fly by the planet.

Figure 5(a) shows Saturn as seen from the JSX spacecraft at closest encounter minus 8 hr. Note the orientation of the rings and the view we will have. A 5° -wide-angle TV frame has been superimposed on the planet to give you an idea of the size of the planet as seen from the spacecraft. Our 4° field of view, of course, would fit just inside this wide-angle TV frame, and our $\frac{1}{4}^\circ$ field of view would fit in about 20 times. At this time we would be able to make photometric maps of the surface brightness of the rings that would have a resolution better than the best ground-based observations. From about 10 hr before encounter to 10 hr after encounter, the resolution with our instrument with a $\frac{1}{4}^\circ$ field of view is equivalent to a resolu-

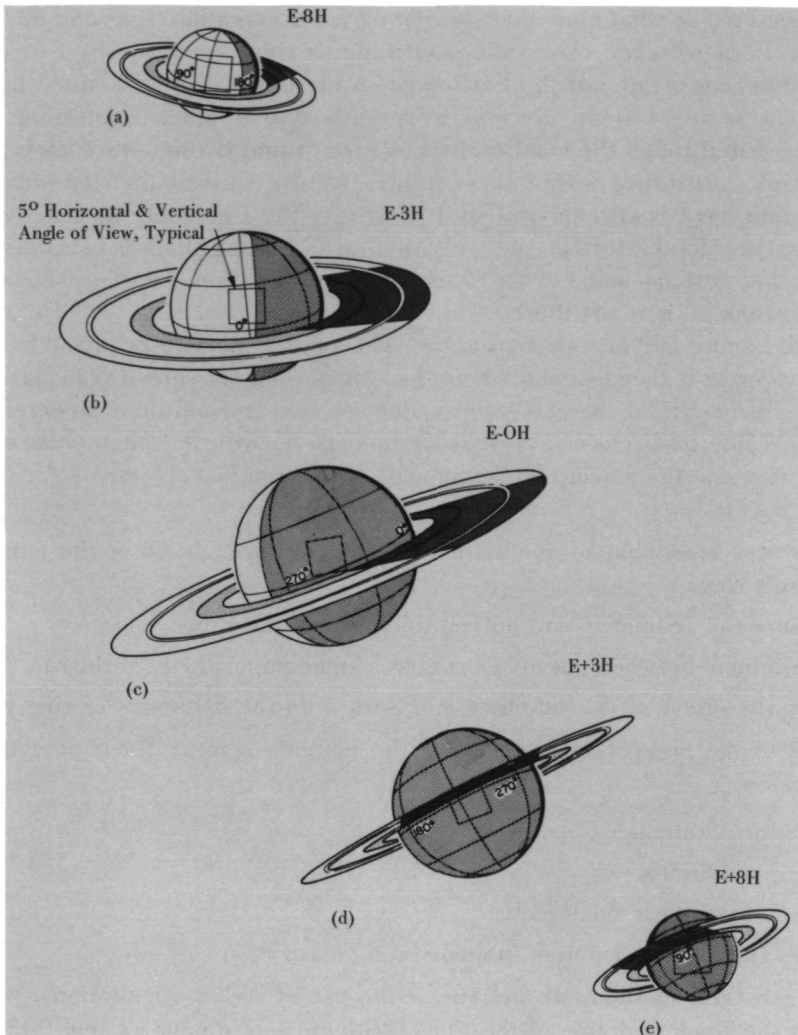


FIGURE 5.—View of Saturn as seen from JSX trajectory.

tion of better than half an arc second with a ground-based telescope.

Figure 5(b) shows the planet at encounter minus 3 hr. You can see the changing orientation of the rings and the difference in apparent size of the planet. Figure 5(c) shows the viewing geometry at closest approach and maximum apparent diameter of Saturn. In figure 5(d) you can see that we have crossed the ring plane (at about $5 R_S$) and are now viewing the rings from the underside. We plan to observe the rings periodically during the encounter phase and to measure their surface brightness from various angles. We are particularly interested in observations during ring plane crossing when we see the rings on edge and then immediately following when the Sun is on one side of the plane and the spacecraft is on the other.

We want to see what kind of scattering we get through the rings and the Cassini division. Barnard made observations of Saturn's rings when the Sun was on one side of the ring plane and the Earth was on the other, and he noticed light condensations at the Cassini division, apparently due to single scattering of light from the Sun through the Cassini division. The A and B rings were dark because of multiple scattering within these rings, but the Cassini division was bright. These data have been analyzed by I. Ferrin at the University of Colorado, who found optical depths for Cassini's division on the order of $\tau = 0.14 \pm 0.09$, which implies that perhaps some of the "missing mass" mentioned earlier (see contribution by Franklin) is really there.

Finally, figure 5(e) is a view from the spacecraft at encounter plus 8 hr. Saturn gets smaller as it recedes, and we are looking at the dark side of the rings. This is the basic geometry of the encounter sequence as seen from the spacecraft.

I would like to discuss next what we hope to do with the photopolarimeter as we go through the encounter sequence. Our scientific objectives for Saturn's rings are as follows:

- Determine size, shape, albedo, distribution, and orientation of the particles in Saturn's rings

- Measure the geometric and optical thickness of the rings

- Discriminate between possible structures and compositions for the ring particles

- Study the effect of the satellites and Saturn on the dynamics of ring particles

Some of the measurements that will be made to achieve these objectives are listed below:

- Stellar occultations

- Solar occultations

- Surface brightness photometry

- Phase angle dependence of intensity and polarization

One powerful method, we believe, is the use of stellar occultations, primarily through the shadowed area of the rings. With our instrument we feel that we can make a measurement every 64th of a second on stars as faint as $V = 4^m$. We will be able to determine the optical depth of each of the rings as a function of wavelength, and we will be able to say something about particles with radii smaller than $10 \mu\text{m}$.

Hugh Kieffer What is the noise limit or digitization limit for a fourth-magnitude star — just a rough estimate?

Lillie We get a signal-to-noise ratio of 10 when we look at an eighth-magnitude star.

Kieffer The limit is noise rather than digitization?

Lillie That's right, we are using a pulse counting system for the detection of photometric events.

Jacques Blamont Isn't the limit set not by the instrument noise but by the scattered light from the rings?

Lillie Yes. We must worry about the baffling of the instrument so that we reject light from the disk of Saturn, even though it is quite a bit off axis, and from the bright portion of the rings. The stellar occultation measurements will very definitely have to be made through the dark side of the rings.

James Pollack How accurately can you measure polarization?

Lillie Our objective is a precision of 0.5 percent. We believe we have demonstrated that we can achieve this accuracy.

We also hope to make observations of solar occultations by looking at a target on the spacecraft to measure variations in the solar flux reaching the spacecraft when it is in the shadow of the rings.

As we approach the planet, we plan to map the surface brightness of the rings in seven band passes and to determine polarization as a function of wavelength. We do not expect to be able to make complete maps of the rings at all phase angles, but we do hope to select areas in the rings for which we follow the intensity variations and the variations in polarization as a function of phase angle as we sweep by the planet.

In addition to getting the wavelength dependence of the optical depth, we also hope to be able to say something about the size of any large particles by observing their Fresnel diffraction patterns. At a distance of about 50 000 km, we feel that we will be able to see the Fresnel diffraction pattern from any particles larger than about 5 m.

Brad Smith Do you have the time resolution necessary for that measurement?

Lillie A 64th of a second should be sufficient. The instrument has the capability of making more rapid observations. The way the instrument is designed, the integration time is specified by the flight data system, and the format we have specified at present calls for a 64th of a second, but that is a variable and can be adjusted.

Smith That is a tough measurement. The size of the Fresnel zone would be small and the relative velocity large.

Pollack The Fresnel zone you are speaking of is from individual ring particles.

Lillie Individual ring particles occulting a star, for example.

Pollack I don't quite understand, how do you separate that from all the other ring particles?

Lillie If there are particles on the order of tens of meters in diameter, they should individually occult the star. If there is a superimposition of the Fresnel diffraction patterns, you have a modeling problem, but presumably you can get the rate at which these events occur, make a model that assumes a characteristic size for the particles, and fit that to the fluctuations in the signal from the star.

Robert Murphy That is going to be on the order of 60 Hz if you go to the extreme of large particles.

Lillie In situ measurements have the following advantages:

Phase angle coverage

Dark sky background

High spatial resolution

Extended wavelength coverage

I would like to elaborate a little bit about the dark sky background with respect to some of the remarks made yesterday about the surface brightness of the D' ring, which may or may not be there. We are going to reduce the surface brightness of the night sky quite a bit just by going out there. As seen from Earth, the surface brightness of the night sky is principally due to air glow and the zodiacal light, neither of which will be a factor when we get to Saturn. Putting some numbers on these parameters, the air glow contributes about $150 S_{10}(V)$, where $1 S_{10}(V)$ is the surface brightness equivalent to one 10th-magnitude visual star per square degree. The zodiacal light contributes about $200 S_{10}(V)$, and integrated starlight contributes about $30 S_{10}(V)$ if the galactic latitude is greater than 40° , which should be the case for some flyby geometries.

Thus, from the ground, we see a sky brightness of about $380 S_{10}(V)$ or about 22.5 magnitudes per square second of arc. Without the contributions of air glow and zodiacal light, the sky brightness at Saturn is only about 8 percent of that on Earth, a decrease of 2.8 magnitudes.

Thus, the sky background is going to be about 25.3 magnitudes per square second of arc, the darkest sky we are apt to get until we get out of our galaxy. Kuiper (1973) has measured a surface brightness of 15 magnitudes per square second of arc for the D' ring, which seems to be at or very near the limit of ground-based capability, primarily due to scattered light from Saturn. If this really represents a signal-to-noise ratio of 1:1, then, with our instrument using the large field of view, we have the same signal-to-noise ratio when we are looking at a surface brightness of about 30 magnitudes per square second of arc. It seems the D' ring as we fly by Saturn is going to seem exceedingly bright as compared to the weakest signal that we can detect with this instrument.

It is of interest to note that, with our 4° field of view and an integration time of about 1 min, the threshold of detectability at a signal-to-noise ratio of 10:1 is about 1.5 10th-magnitude stars per square degree, which is the predicted level of the cosmic sky background due to external galaxies in the field of view of our instrument. Thus, in the vicinity of Saturn, another objective of our experiment is to measure the contribution of the extragalactic light to the sky background, or at least to set upper limits on it.

These basically are the observations we plan to make with the photopolarimeter. With polarization data, we are going to be able to say something about the properties of particles in the rings. We should be able to determine particle shape as being spherical, irregular, or of a regular crystalline form. In the case of spherical particles, we should be able to find accurate values of the mean particle refractive index. For nonspherical particles, it will be possible to distinguish between particles that are larger than, on the order of, or smaller than the wavelength of the scattered light. It will also be possible to confirm or reject particular particle shape and refractive index possibilities such as ice crystals.

In the case of large particles several meters in diameter, it should be possible

to say something about surface texture and the composition of any coatings. If their surface resembles that of Phobos and Deimos, we should be able to determine that and to derive an albedo from the polarization data. There is a definite relationship between the phase function of polarization and the geometric albedo of the asteroids so that from the polarization-phase-function diagram you can find the albedo of particles directly.

In summary, by going to the vicinity of Saturn, the photopolarimeter experiment will be able to repeat the classical observations and extend them to shorter wavelengths with larger phase angle coverage at higher spatial resolution than has been possible heretofore from the ground.

DISCUSSION

James Pollack You say that you thought you could get refractive index information even if the particles were irregular? How is that possible? For a spherical particle, you have Mie theory to compare your answers against. What do you have to compare your answers against for nonspherical particles?

Charles Lillie I meant to say it would be possible to confirm or reject certain particular particle shape and refractive index possibilities. In other words, if you assume a particular particle like a solid ammonia crystal or an ice crystal, which has an index of refraction associated with it, you can reject certain combinations.

Walter Jaffe Is there a program for this instrument of general astronomical observations before the encounter?

Lillie We do have a target-of-opportunity program associated with the MJS mission plan. During cruise, certain selected objects will be observed for calibration purposes.

Brad Smith We have a request for a target-of-opportunity program.

Lillie The program is still under negotiation. I assume that it is going to be accepted. If it is, we will be making observations of planets other than Jupiter and Saturn, and there will be some observations of stars for calibration purposes.

Jaffe I was also thinking of extragalactic, very low-surface-brightness objects.

Lillie We are interested in making observations like that. Whether they will be in the timeline for the spacecraft is not clear at this time.

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Bradford A. Smith

THE MARINER JUPITER/SATURN VISUAL IMAGING SYSTEM

In the interest of saving time, I will try to be brief. Many of the objectives that Chuck Lillie mentioned (see preceding contribution) are also objectives of interest to the imaging science team. The principal difference between imaging and photopolarimetry is that we have very much higher spatial resolution while the photopolarimeter has higher photometric accuracy.

Let me describe the optics that are presently included in our baseline system. These are a so-called "narrow-angle" camera, which has a focal length of 0.5 m, and a "wide-angle" camera, with a focal length of 100 cm. The television image format contains approximately 800×800 picture elements (pixels) in its 1-cm² scanning area.

This means that for the baseline optical systems, one line pair corresponds to a resolution of about 10 seconds of arc for the narrow-angle camera and five times that for the wide-angle camera. Our choice would be for longer focal lengths, but we have been given an inherited hardware design. Our narrow-angle camera is, in fact, the narrow-angle camera that was flown on Mariner 9. We presently have a request¹ into the MJS project office and the NASA program office to change this baseline design by increasing the focal length of our narrow-angle camera to 1.5 m and the wide-angle camera to 200 cm. This would get us down in the range of 3 seconds of arc or so per TV line pair.

The spectral response of the narrow-angle camera ranges from about 315 nm, I believe, to about 600 nm. Essentially, the red cutoff is established by that fact that we are running out of vidicon response. The wide-angle optics are still open to question, and we are attempting to come up with an optical design that will allow us to go further out into the ultraviolet, perhaps as far down as 200 nm, if the vidicon is still holding up at that wavelength. The vidicon response in the ultraviolet is not known at this time. We have eight filter positions for each of the two cameras, and these filters have not yet been selected.

New Mexico State University.

¹ Approval was subsequently received for the longer focal-length optics.

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Chuck Lillie showed you some figures that give the geometry of the trajectory past the rings of Saturn. The observations we could make might be, for example, the radial distribution and phase function of material in the rings as we fly past the ring system and perhaps the vertical distribution (that is, the geometric thickness of the rings) as we go through the ring plane. Let's assume that we are able to make a close pass to the rings, and I think this almost certainly means crossing the ring plane through the orbit of Mimas unless the question of the D' ring is settled between now and then. We could, with the 1.5-m system, get just about close enough to the rings so we could see particles on the order of, say, 0.3 km to 1 km, and this of course has considerable impact on the monolayer model or perhaps Jim's source particles² floating around on the edge of ring A.

We can also look for Janus and determine whether or not it exists. Parenthetically, I might add that we made a search for Janus at New Mexico State University last winter with a limiting magnitude of about 14.6 or 14.7, visual. This is about 0.7 of a magnitude fainter than Dollfus had estimated for Janus. This search was, incidentally, unsuccessful.

This doesn't necessarily mean that Janus does not exist. Dollfus could easily have been off that far in estimating the brightness of the satellite. However, there will be other passages through the ring plane. They occur about 1980 and, I think, before the MJS spacecraft reaches Saturn.

In 1966 there were not many observatories that were prepared for, or even thought of looking for, additional satellites inside the orbit of Mimas. I think Dollfus was the only one who planned his observations in that way. The material in the D' ring still seems to be subject to question, but with intensifier devices and better observing techniques in 1980 we should be able to answer some of these questions before MJS arrival at Saturn.

In the past we have had little success in trying to do photometry with vidicons. I think there is no reason to be terribly optimistic for MJS, except for two additional factors that have not been present in previous television experiments on Mariner spacecraft. The first is that we expect to have some sort of inflight calibration, and the second is the photopolarimeter which will also help us calibrate the vidicon.

We expect, then, to get both the spatial distribution of material in the rings and brightness measurements. This would allow us to meet many of the objectives that Lillie mentioned while discussing the photopolarimeter.

² See contribution by Pollack for a discussion of these "parent" or "source" particles.

DISCUSSION

James Pollack What is the minimum thickness of the rings you could measure if you go through the ring plane at the orbit of Mimas?

Brad Smith About 1 km at a distance of 40 000 km from the outer edge of ring A, if we have the 1.5-m system. That would be roughly 1.5 TV lines.

Hugh Kieffer For an extended object, you ought to be able to get down to a resolution of perhaps even less than a TV line.

Smith Yes, and we can do extensive processing, of course, with multiple frames.

THE MARINER JUPITER/SATURN ULTRAVIOLET EXPERIMENT

The ultraviolet spectrometer on the MJS spacecraft is designed to detect and measure atmospheric properties. We record the spectrum from 400 Å to 1600 Å. We use two modes of operation—an airglow mode and an occultation mode. In the airglow mode, we measure the radiation from the planetary atmosphere, which will be predominantly solar radiation, resonance scattered by the atomic or molecular constituents such as hydrogen Lyman- α at 1216, or the helium 584 Å line. In the occultation mode, sunlight is reflected into the spectrometer, and the solar spectrum is recorded. As the atmosphere moves between the spacecraft and the Sun, the absorption characteristics of the atmosphere are measured over the full wavelength region. The nature of the absorption through the spectrum can be used to identify the absorber as well as to measure the abundance in the line of sight to the Sun. We can interpret the thermal structure down to and through the turbopause, a range of about 1000 km. The atmospheric constituents common to the outer planets are H, He, H₂, NH₃, and CH₄.

Apparently Saturn's rings are composed of particulate matter. If this is so, the solar spectrum in the occultation mode will be uniformly attenuated. If there is a gaseous component, we will be able to detect it in either mode. However, we do not have a prime experiment associated with the rings. The whole planet and ring system will be mapped in emission as we approach, and we will have good sensitivity because the length of time available for observing is a matter of days.

DISCUSSION

James Pollack What is the lower limit to the flux you can detect from hydrogen airglow?

Lyle Broadfoot As we approach the planet, I would say 10 Rayleighs would be easily detectable.

Kitt Peak National Observatory.

Pollack So you could detect the amount of hydrogen Jacques Blamont has spoken about (see contribution by Blamont).

Broadfoot Yes, I suspect that we could get even lower than 10 Rayleighs, considering the amount of time that would be available to observe.

Jacques Blamont The difficulty isn't with the instrument sensitivity, it is with the interplanetary Lyman- α background, which is about 250 Rayleighs.

Broadfoot That is correct. The background is substantial and would be troublesome, but observations of the planet would not contain the background component unless the planet were smaller than the field of view. The background would cause some confusion when we look at the rings unless we are careful to make the ring observations against the planetary background.

Thomas E. Burke

THE MARINER JUPITER/SATURN IRIS EXPERIMENT

The principal investigator of the infrared spectroscopy and radiometry investigation is Dr. R. A. Hanel of Goddard Space Flight Center. Unfortunately, he was not able to be here today.

Figure 1 is a schematic representation of the Jovian spectrum from the near ultraviolet to the thermal infrared regions. The spectrum for Saturn should not be markedly different except, of course, for the radiance levels. The incident solar energy is shown by the solid line at the right of the diagram. Some of the solar energy is absorbed (the dashed line), and the remainder is reflected or scattered. The energy that is absorbed is reemitted in the thermal infrared, together with any energy from internal sources.

The IRIS instrument (an acronym for infrared interferometer spectrometer) consists of a coaxial interferometer and radiometer. The bandpass of the interferometer extends from 200 cm^{-1} to 3300 cm^{-1} (50 to $3.3\text{ }\mu\text{m}$); the bandpass of the radiometer extends from 5000 cm^{-1} to $33\,000\text{ cm}^{-1}$ (2.0 to $0.3\text{ }\mu\text{m}$). These bandpasses are indicated by the solid horizontal lines near the top of figure 1. We are considering the possibility of extending the high-frequency cutoff of the interferometer out to 4000 cm^{-1} ($2.5\text{ }\mu\text{m}$).

The optical layout of the MJS IRIS is depicted in figure 2. An $f/8$ Cassegrain telescope provides large energy throughout and a narrow field of view (0.25°). The diameter of the primary mirror is 51 cm. A dichroic element is used to separate the energy for the radiometer from that for the interferometer. Near infrared and visible energy ($\geq 5000\text{ cm}^{-1}$) is passed by the dichroic element to a focusing mirror and to the radiometer detector. Near and thermal infrared radiation ($\leq 4000\text{ cm}^{-1}$) is reflected, collimated, and passed to the interferometer. The radiating surface and the various blankets and baffles are also depicted.

The optical layout of the Michelson interferometer that was used on the Nimbus 3 and 4 spacecraft is depicted in figure 3. The layout of the MJS interferometer

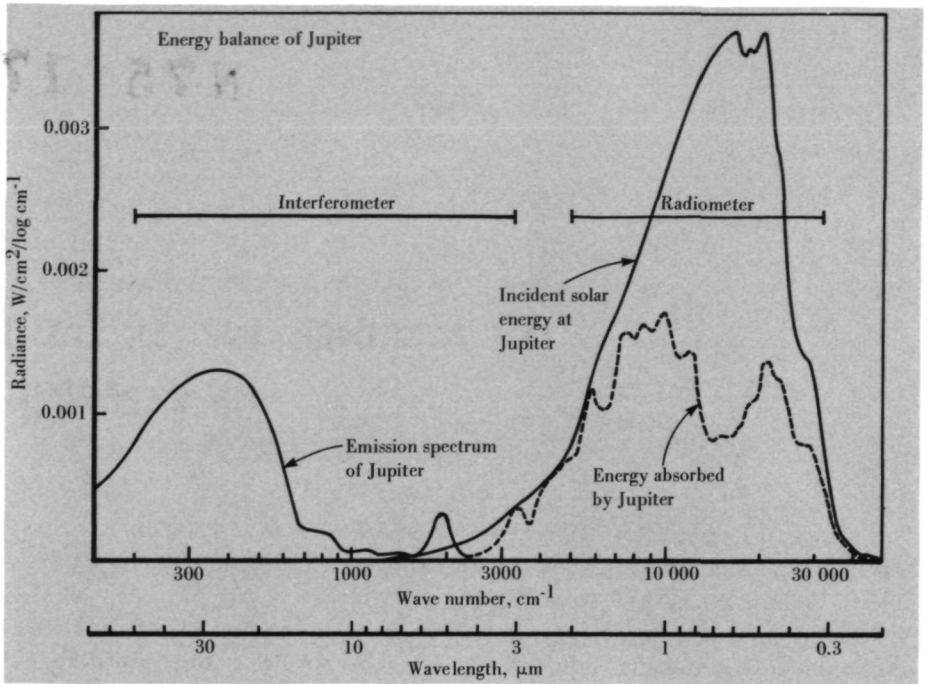


FIGURE 1.—Energy balance of Jupiter. The spectral ranges of the interferometer and radiometer are superposed on the thermal emission spectrum and upon the reflected solar spectrum.

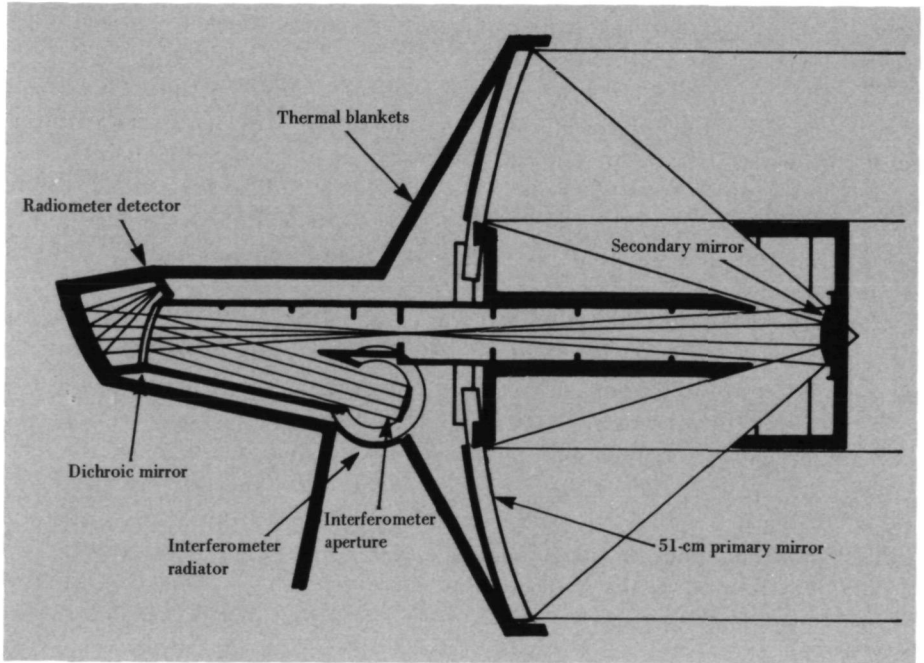


FIGURE 2.—Optical schematic diagram of the MJS IRIS.

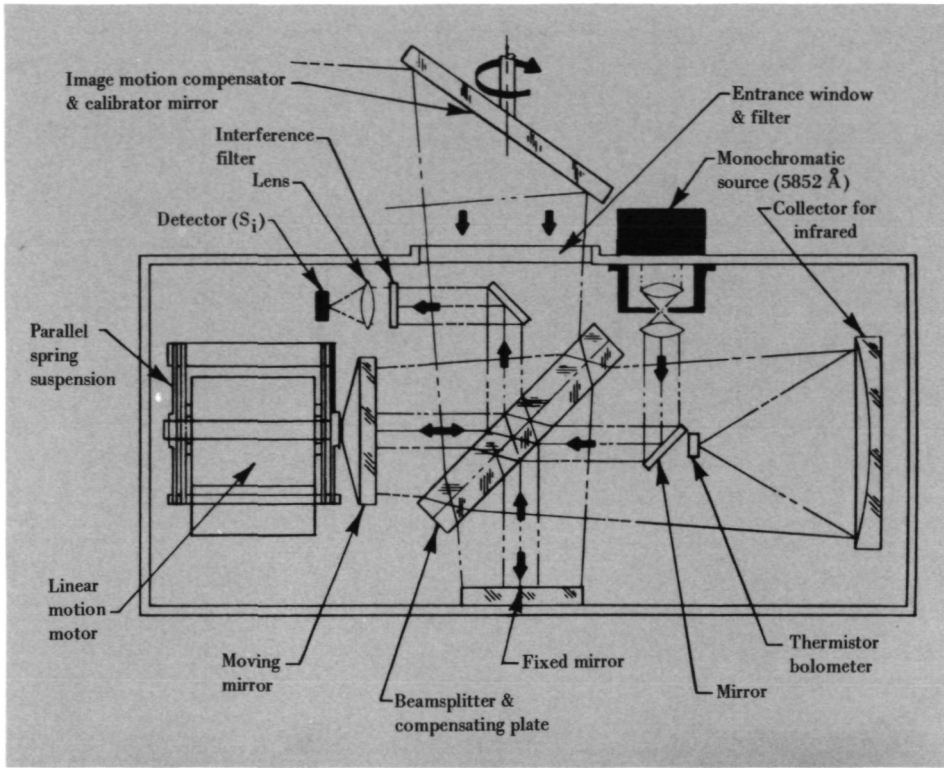


FIGURE 3.—Optical schematic diagram of the Michelson interferometer.

will not be substantially different: the image motion compensator and calibrator mirror will be replaced with the Cassegrain telescope; the entrance window/filter and the beamsplitter compensator will be deleted; and the thermistor bolometer will be replaced by a detector with lower noise equivalent power—either a thermopile or a pyroelectric detector. Note that the interferometer has a coaxial interferometer using the 585.25-nm neon line for wavelength calibration.

Figure 4 indicates the approximate magnitude of the signals expected at Jupiter and Saturn. The thermal emission curves are blackbody curves for various temperatures (130 K for Jupiter and 96 K for Saturn). The reflected solar radiation curves are based on an albedo of 0.5. The noise equivalent radiance (NER)—the level of radiance for which the signal-to-noise ratio is unity—is indicated on the figure. One can infer the signal-to-noise ratio as a function of frequency from the expected signals and the NER: near the peaks of the blackbody curves the signal-to-noise ratio is in excess of 1000, and near the crossover between emitted and reflected energy the signal-to-noise ratio is not significantly greater than unity. However, I would emphasize that the instrument has a spectral resolution of 4.5 cm^{-1} . The NER can be lowered by degrading the spectral resolution.

I would like to briefly review some of the objectives of the IRIS investigation.

Measurements of reflected solar radiation and thermally emitted radiation over a wide range of latitudes and phase angles will permit determination of the energy

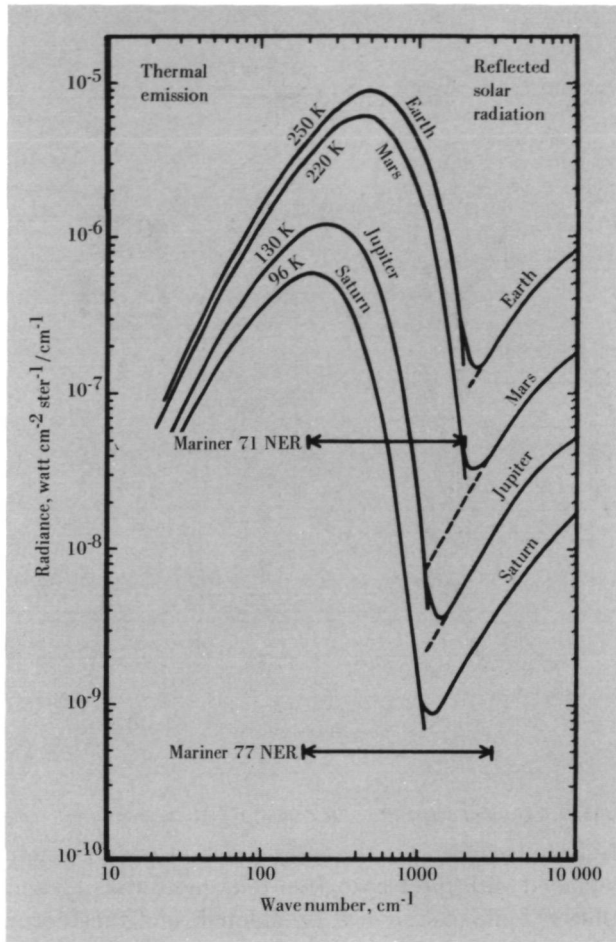


FIGURE 4.—Radiance vs frequency for Jupiter and Saturn. The noise-equivalent-radiance (NER) for the MJS-IRIS (Mariner 77) is shown.

balance of Jupiter and Saturn (i.e., the existence of internal heat sources and their magnitude).

The IRIS will provide information about the quantities (or upper limits) of methane, ammonia, hydrogen, and water vapor. The high spectral resolution and low NER of the IRIS will permit a search for a variety of minor constituents, including the so-called prebiotic molecules. The IRIS will provide information about the hydrogen-helium ratio as well as about isotopic ratios, H/C, C/N, and C/O.

The IRIS will permit determination of the three-dimensional temperature fields (latitude, phase angle, and height) of the planets, Titan, and perhaps other satellites. The three-dimensional temperature fields will be used in investigations of atmospheric dynamics.

The IRIS will provide information concerning the presence of water ice, silicates, and chondritic materials in solid bodies and in aerosols. Since these items are particularly germane to the ring investigations, I would like to show some of the relevant Mariner Mars 1971 results.

Figure 5 depicts the average of approximately 2000 spectra that were acquired during the Martian dust storm. One can readily see the wing of the $15\ \mu\text{m}$ CO_2 absorption band, several other CO_2 features, H_2O lines, and a very broad absorption feature between 800 and $1400\ \text{cm}^{-1}$. This latter feature is due to the silicates that were present in the atmosphere. The position of the transmission minimum, characteristic of the silicate content of the material, permits one to distinguish between the so-called acidic and basic silicates. A synthetic spectrum for a Martian atmosphere consisting of only CO_2 and H_2O is superposed on the same figure.

During the latter part of the mission, a spectrum was acquired over a cloud-free region of Arcadia; shortly thereafter, another spectrum was acquired over a cloud-covered region of the Tharsis ridge. These spectra are shown in figure 6, displaced for clarity. R. Curran of Goddard Space Flight Center has calculated the absorption characteristics of a water-ice cloud from the complex refractive index of water ice and an assumed particle size distribution. One can readily see the good agreement. The discontinuity in the calculated spectrum is caused by the use of two sources of refractive index data.

The previous remarks of C. Lillie and B. Smith are also applicable to the IRIS investigation. The IRIS has a circular field of view (0.25° full cone) that is the same as the narrow field of view of the photopolarimeter and about one-half the size of the proposed long focal length imaging system. At closest approach, the 0.25° field of view will permit a number of observations that are completely within the extent of the A and the B rings.

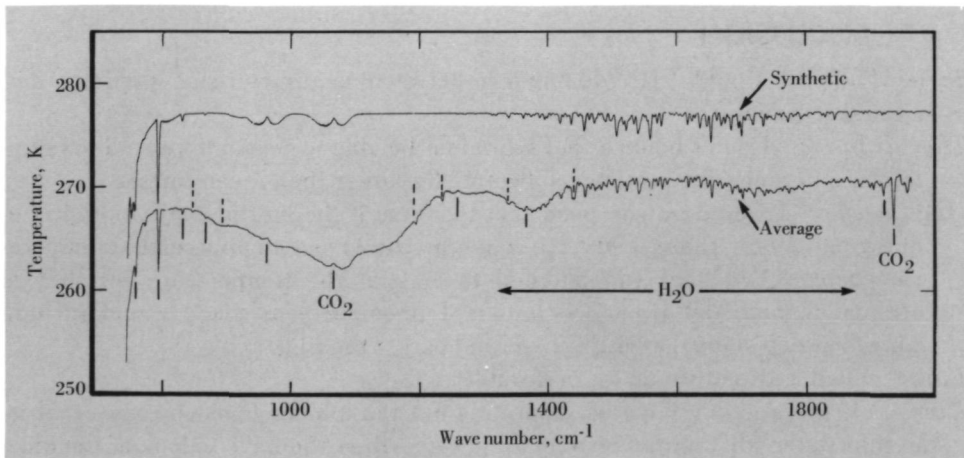


FIGURE 5.—Mariner 9 spectra showing broad silicate feature between $800\text{--}1200\ \text{cm}^{-1}$ and atmospheric CO_2 and H_2O features.

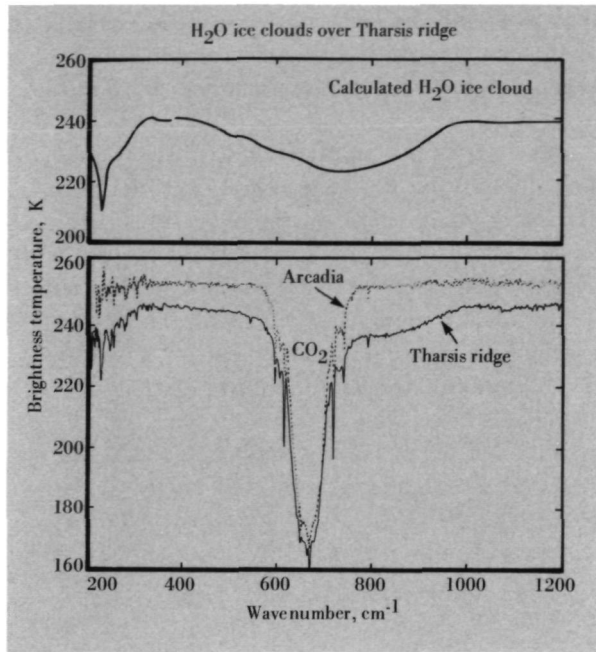


FIGURE 6.—Experimental and theoretical H_2O ice cloud spectra.

The IRIS will be used to measure the equilibrium temperature of ring particles prior to their shadowing by Saturn and to measure their minimum temperatures just prior to exiting Saturn's shadow. From these measurements we hope to determine the thermal inertia of the ring particles and, perhaps, to infer their size and density. The particle sizes for which we expect this sort of measurement to be applicable run from several millimeters to the order of a centimeter.

DISCUSSION

James Pollack Could IRIS distinguish between centimeter-size particles and meter-size particles?

Thomas Burke I don't believe that we would be able to see an appreciable temperature difference for particles substantially larger than a centimeter.

Hugh Kieffer I would maybe push that to 10 cm if the inertia were appropriate, but beyond 10 cm there is just too much inertia to see an appreciable temperature change. You said you planned to look at the temperature just before entrance to and exit from occultation. I presume your plans would include trying to sweep along the entire shadowed part of the ring?

Burke Such a sweep would be preferable.

Kieffer If there is any hope of separating out the inhomogeneous case—that is, the thin water frost on top or something else—then you have to look at the knee of the curve (see contribution by Kieffer) after the particles enter the shadow. Hopefully that would be part of the plan.

Pollack Have you given any thought to trying to measure the temperature when looking on the sunlit side, as contrasted with looking at the shadowed side?

Burke Yes, we have. This is an observation we hope to make.

I would like to mention one other ring experiment that we are considering.

We would like to use the radiometer to determine the optical thickness of the rings when the spacecraft passes behind the rings.

Kieffer Your instrument has very good spectral resolution. Has any thought been given as to what you might infer based on the departure from a single black-body curve for the ring particles?

Burke I don't think that has been considered at this point.

Von R. Eshleman

THE MARINER JUPITER/SATURN RING OCCULTATION EXPERIMENT

Radio occultation measurements of the rings of Saturn constitute one of several experiments to be conducted by the Mariner Jupiter/Saturn radio science experiment team. The members are J. D. Anderson, G. Fjeldbo, and G. S. Levy of JPL, and T. A. Croft, G. L. Tyler, and myself of Stanford University. A very substantial contribution to the material I am presenting today is due to A. E. Marouf, a graduate student at Stanford.

As a spacecraft passes behind the rings of Saturn as viewed from Earth, the downlink radio signals will be affected in frequency, spectrum, and amplitude by the material in the rings. Measurements of these signal characteristics can provide information on the amount, distribution, particle size, and other characteristics of this material.

Each spacecraft will transmit two coherent signals at 13-cm (*S* band) and 3.5-cm (*X* band) wavelengths. The spacecraft antenna has a diameter of 366 cm, and ground antennas are the 64-m paraboloids of the Deep Space Network. Transmitter powers are 10 W at *S* band and 13 W at *X* band.

Gorden Pettengill Are the spacecraft transmitters solid state?

Eshleman No, they are two-level, traveling-wave-tube amplifiers. The higher power levels are 28 W at *S* band and 22 W at *X* band, but these cannot be used simultaneously at encounter because of other demands on total spacecraft power.

The radio team is particularly concerned about obtaining a flyby trajectory that includes occultation across the whole extent of the ring structure. Figure 1 is an example of some of the studies of this problem by P. Penzo and his colleagues at JPL. It shows that, for a given arrival date (September 21, 1981) and radius of closest approach (4 Saturn radii), there is a pivot point relative to where the spacecraft appears to go behind the planet as viewed from Earth. The θ angle is the one defined by Penzo previously (see contribution by Penzo).

Department of State, Bureau of International Scientific and Technological Affairs; on leave from Stanford University.

Figure 2 shows a closeup view of a relatively favorable condition for ring occultation on the entry side of the planet. This trajectory also provides a clear radio occultation measurement of the atmosphere (and ionosphere) at entry, so that atmospheric and ring particle effects can be separately determined. Note that at exit the atmosphere would not be seen clear of possible ring effects.

Figure 3 shows the variation with time of the inclination of the plane of the rings as viewed from Earth. The period of time for spacecraft arrival at Saturn for the defined MJS project is between late 1980 and June 1981. The inclination is small during this period—between 4.5° and 7.5° . Note that at later times this angle gets larger.

Figures 4 and 5 illustrate the potential importance of ring inclination and ring-spacecraft distance to the radio occultation experiment. The angle ψ is the ring

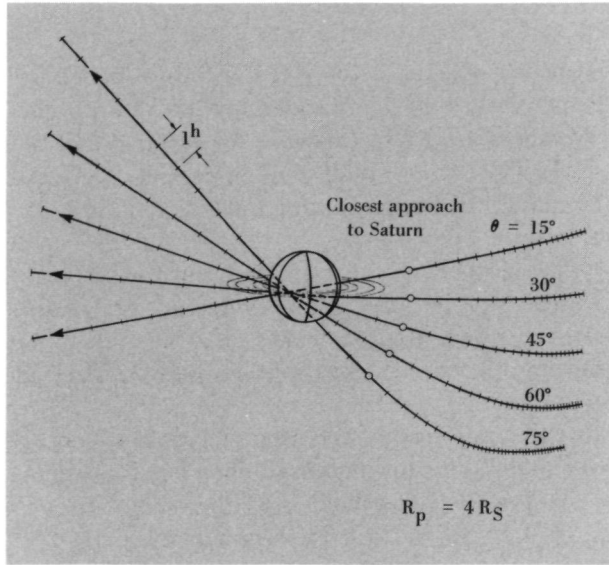


FIGURE 1.—Saturn trajectories viewed from Earth on September 21, 1981.

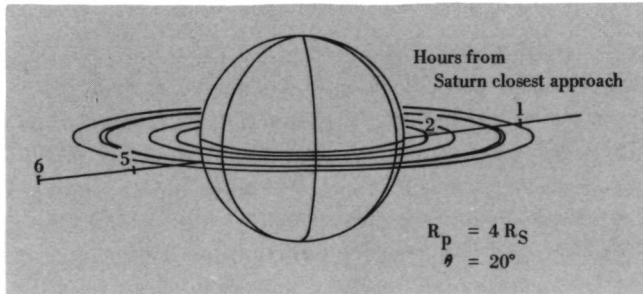


FIGURE 2.—Ring occultation trajectory of September 21, 1981 arrival date.

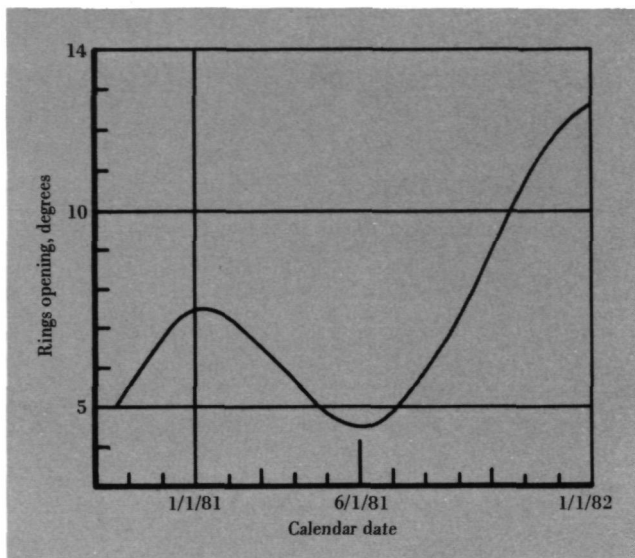


FIGURE 3.—Ring inclination as viewed from Earth for the period around MJS Saturn encounter.

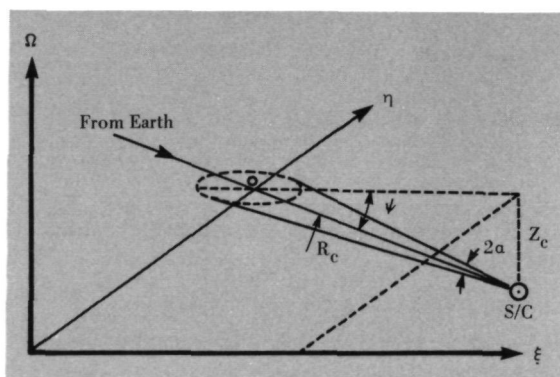


FIGURE 4.—Spacecraft antenna ring viewing geometry.

inclination, R_c is the distance from the spacecraft to the intersection point in the ring plane ($\xi-\eta$ plane) of a line from the spacecraft to Earth, and α is the half-width of the spacecraft antenna beam. For an inclination angle of 8° and several different (relatively small) values of R_c , figure 5 illustrates the area in the ring plane illuminated by the antenna for values of α of 0.5° , 1.5° , and 3° . This figure also shows the disk of Saturn and the relative light intensity of the rings as a function of radius.

Under certain conditions discussed below, the resolution of the radio occultation experiment would be limited by the illuminated area of figure 5. The X-band beam illumination of the ring plane would be an ellipse somewhat smaller than

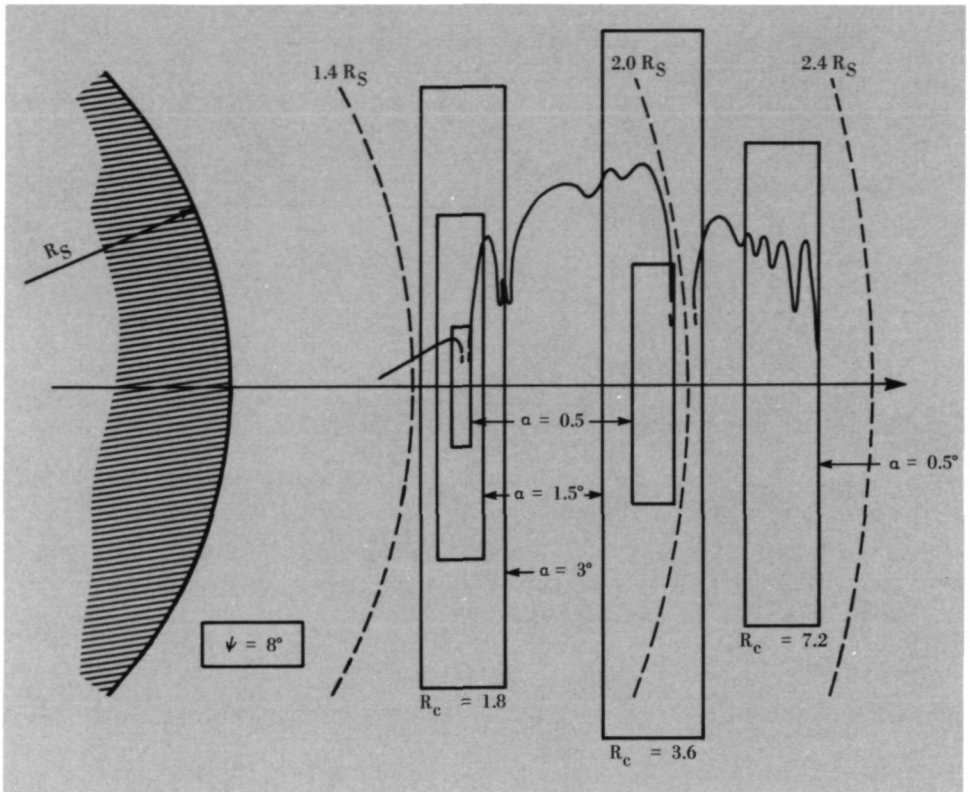


FIGURE 5.—Antenna patterns superimposed on ring system for specific spacecraft-ring-Earth geometry.

would fit in the $\alpha = 0.5^\circ$ rectangle of figure 5, while the S-band ellipse would be a bit smaller than the corresponding $\alpha = 1.5^\circ$ rectangle.

Pettengill The smaller rectangle is for the X band?

Eshleman The smallest one ($\alpha = 0.5$) is actually larger than the X-band pattern, since the half-power beamwidths correspond to $\alpha = 0.34^\circ$ at X band and $\alpha = 1.23^\circ$ at S band.

Note that if the antenna beamwidth limits resolution, it will be difficult to obtain reasonable discrimination of changes of ring characteristics with radius unless the ring inclination is relatively large and the flyby distance is small.

The next sequence, figures 6 through 9, shows a view looking perpendicularly down on the ring plane. These examples are for Earth being 30° above the plane, so that the spacecraft at occultation is 30° below the ring plane. In figure 6 the spacecraft is shown quite close to the planet, only $2R_S$, or one Saturn radius from the surface of the planet. It is moving perpendicular to the Earth-spacecraft line at 10 km/sec. Figures 7, 8, and 9 show the spacecraft on this path at successively later times. For these conditions, the contours shown are loci of constant doppler shift (in kilohertz at S band or 11/3 times this value at X band) for scattering

from ring particles at the indicated positions. That is, for a ray from the spacecraft to the ring particle and then to Earth, the doppler shift due to particle and spacecraft motion would be the value indicated at that position. (There are, of course, some areas where the ray from the spacecraft to a ring particle or from a particle to Earth would be shadowed by the planet.)

These doppler contours can be used to determine theoretical radio-frequency spectra for rings with various radial distributions and particles with different assumed scattering properties. The inverse problem is also being studied (i.e., we wish to know how to determine ring and particle properties from measured spectra).

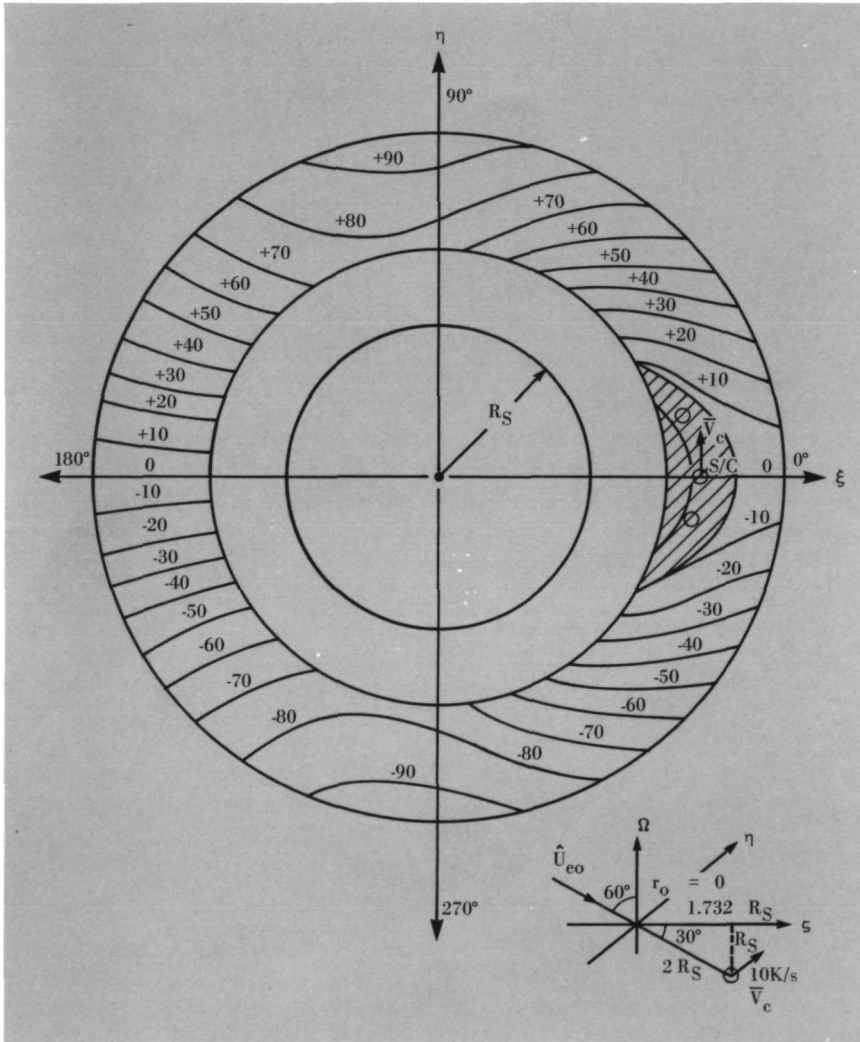


FIGURE 6.—Doppler contours for the visible ring system illuminated by the spacecraft with $r_o/R_S = 0.0$.

Pettengill Did you say these were drawn for an inclination of 30° ?

Eshleman Yes. Thus they are not exactly representative, since smaller angles are expected at encounter, although they do illustrate the important characteristics.

You might think of the full range of contours in these figures as representing doppler effects if the spacecraft antenna were omnidirectional. With the actual high-gain antenna, only those elliptical illumination regions discussed earlier would contribute to the spectrum. The spacecraft antenna could be pointed in directions other than directly toward Earth in order to study a wider range of

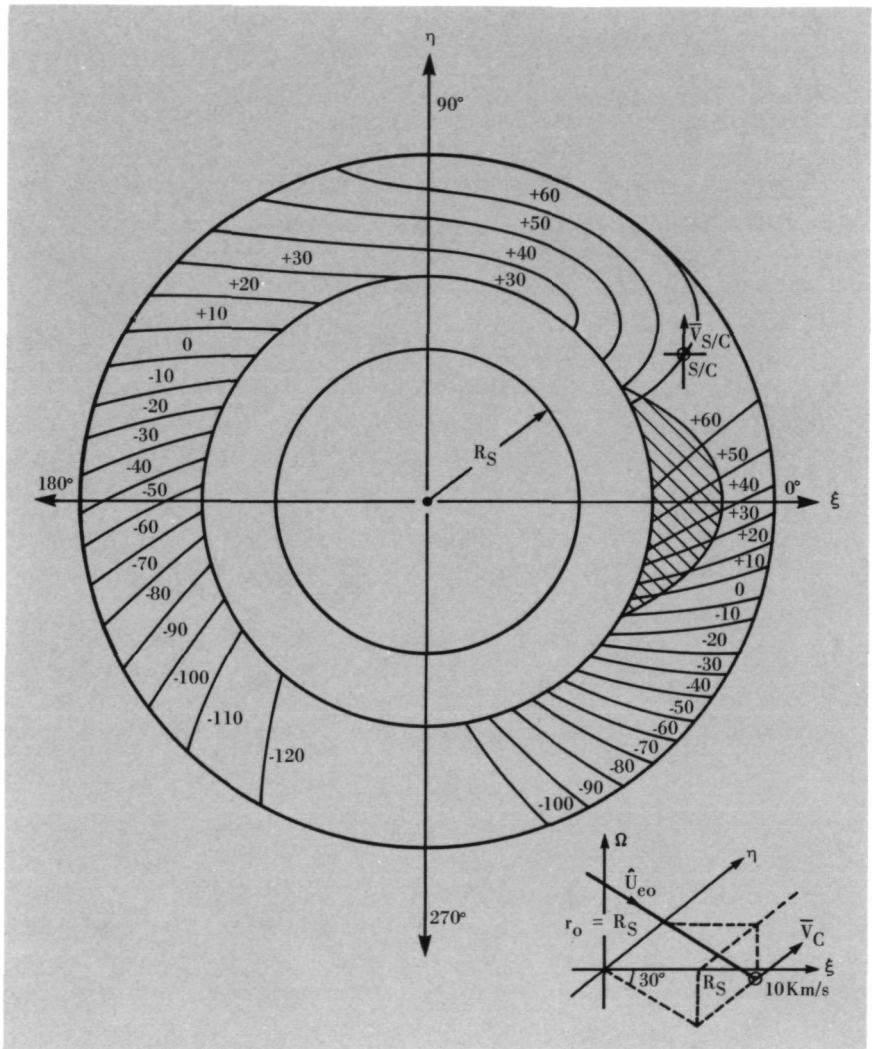


FIGURE 7.—Doppler contours for the visible ring system illuminated by the spacecraft with $r_o/R_s = 1.0$.

particle scattering angles. In principle, it would be possible to study scattering angles from direct backscatter to direct forward scatter.

James Pollack I'm sorry, I'm confused here. Are you speaking of an experiment you actually plan to do where you shine the antenna at the rings and look at the signal that gets bounced off the rings?

Eshleman We will be concerned primarily with the angles very near forward scattering, but we also hope to be able to orient and move the spacecraft so that the antenna will look in directions well away from the direct line to Earth, in order to gain more complete information on the scattering properties of the particles.

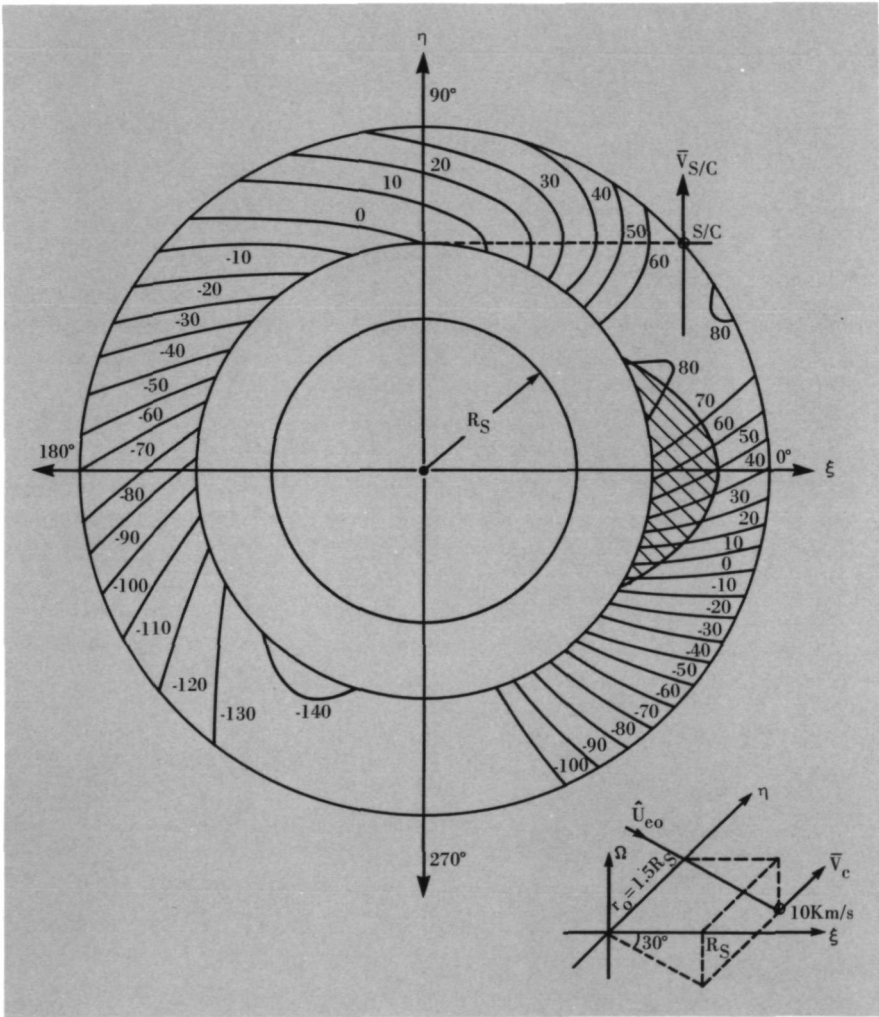


FIGURE 8.—Doppler contours for the visible ring system illuminated by the spacecraft with $r_0/R_S = 1.5$.

Now I would like to outline briefly several characteristics of radio wave scattering from particles. The size of the particles is of particular interest. Table I illustrates five different particle size scales that delineate six regions where the scattering characteristics would differ in this experiment. The inverse wave number at X band, k_X^{-1} , is about 0.5 cm. The inverse wave number at S band, k_S^{-1} , is about 2 cm. The radius of the spacecraft antenna, R , is about 180 cm. The radius of the principal Fresnel zone at X band, F_X , is about 2 km if the spacecraft distance from the ring intersection point described earlier is 4 Saturn radii, and the corresponding scale at S band, F_S , is about 4 km. These five scale sizes are boundaries to the six size regions specified as A, B, \dots, F in table I.

The following discussion would apply ideally to cases where the particles are

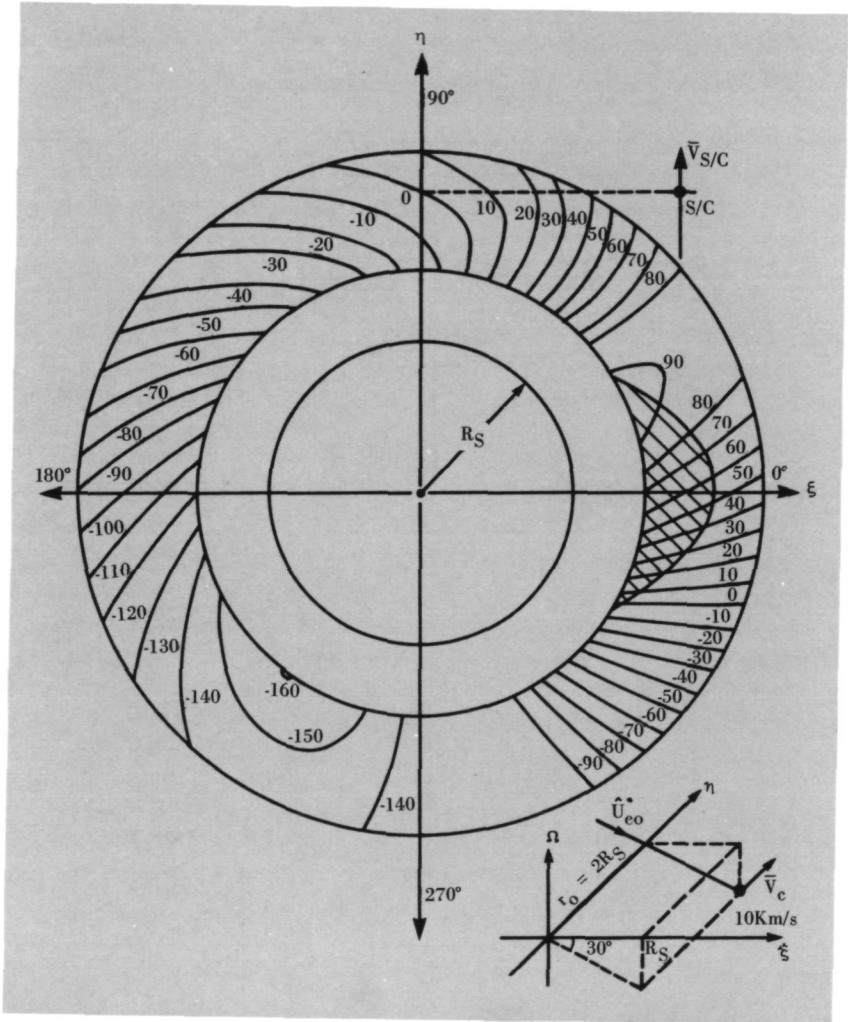


FIGURE 9.—Doppler contours for the visible ring system illuminated by the spacecraft with $r_o/R_s = 2.0$.

TABLE I.—Scales which determine regions of different particle scattering behavior.

Scale, cm			
k_x^{-1}	0.5	<i>A</i>	$a < k_x^{-1}$
k_s^{-1}	2	<i>B</i>	$k_x^{-1} < a < k_s^{-1}$
<i>R</i>	180	<i>C</i>	$k_s^{-1} < a < R$
F_x	$2 \cdot 10^5$	<i>D</i>	$R < a < F_x$
F_s	$4 \cdot 10^5$	<i>E</i>	$F_x < a < F_s$
		<i>F</i>	$F_s < a$

all of the same size, but if there is a narrow range of sizes it would still be applicable. Important information on the size distribution would also be available based on these same scales if there were a wide spectrum in size, but this condition is not analyzed here.

Consider first region *A*, where all particles are smaller than the smallest size (0.5 cm) in the table. (The Earth-based radar measurements of the rings indicate that this size regime is not to be expected.) Figure 10 outlines some features of this case. In the absence of ring particles, the received signal would be an impulse in the frequency domain, as illustrated by the dashed line. In general, particles could cause a frequency shift (f) of the impulse, a reduction of power in the coherent signal to P_c , and an incoherent signal of power P_i and spectral width Δf . For case *A* it is expected that the incoherent signal would be too weak for detection, so P_i and Δf would not be measurable. Because there are two coherent wavelengths, the wavelength dependence of the loss of coherent power could be measured. For lossless particles this change would be due to scattering and would be proportional to λ^{-4} , while for lossy particles it would vary as λ^{-1} .

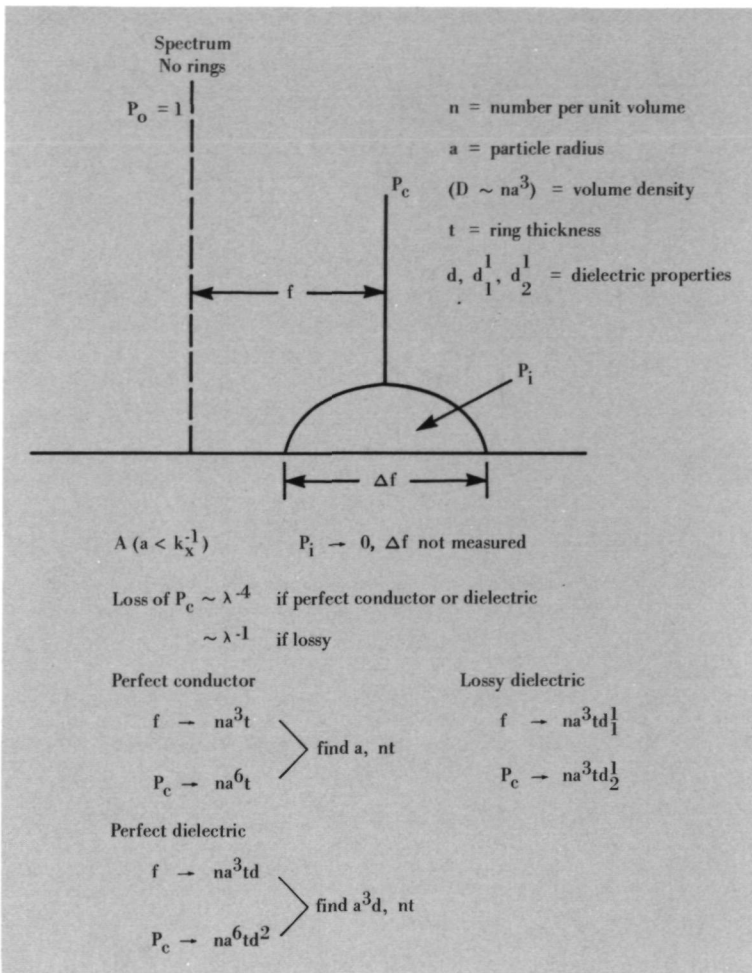


FIGURE 10.—Illustration of spectral information in case A.

Pollack Any real particles will show λ^{-1} dependence if they are small compared to the wavelength.

Eshleman It does take only a very small loss to do that, I agree.

Pollack A metal ball, if it is large compared to the wavelength, is a perfect reflector; if it is small compared to the wavelength, it is a perfect absorber.

Eshleman Well, if it were lossless, it would not be an absorber.

Pollack Yes, but it isn't.

Eshleman I am giving illustrations in terms of lossless and lossy particles, not in terms of real metals and real dielectrics but rather perfect conductors and perfect dielectrics as compared with lossy particles.

The loss in coherent power will represent different measures of ring and particle properties, as shown in figure 10, for lossless and lossy materials. Similarly,

the frequency shift of the coherent signal will represent other combinations of these properties.

Under certain conditions some of these properties are separable, but in general one might characterize the results as being important mainly when combined with the results of other experiments for a more complete description of ring and particle properties. Obtaining resolution in radius from signal characteristics in this case (A) is determined by Fresnel zone size (i.e., very fine resolution) and not by the antenna illumination area illustrated in figure 5.

Cases C and D are illustrated in figure 11. Case D is for particle sizes larger than the spacecraft antenna but smaller than the X-band Fresnel zone; that is, for particles having radii between 180 cm and 2 km. Consider, for example, the case where these large particles are dense. That is, they are either perfectly absorbing, or they are reflecting if multiple scattering is not important in total signal characteristics. For this case there would be no frequency shift of the coherent signal.

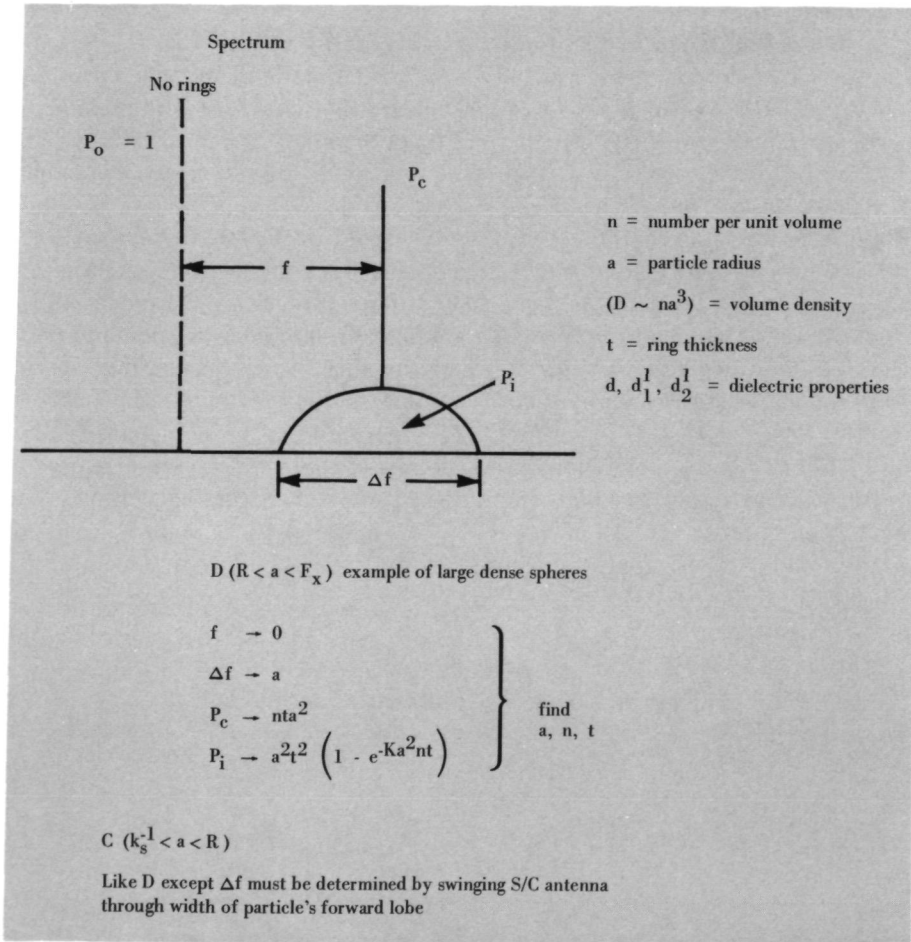


FIGURE 11.—Illustration of spectral information in cases C and D.

The spectral width of the incoherent signal would be a direct measure of particle size. Measurements of coherent and incoherent power would give other combinations of ring and particle parameters which would make possible the separate determination of particle size, ring thickness, and the number of particles per unit volume, all as a function of radius in the ring plane. Resolution for case *D* would not depend on the illuminated area in the ring plane but would be a smaller area determined by particle size.

Case *C* is for particles smaller than the spacecraft antenna but larger than the S-band inverse wave number, or between 2 and 180 cm. Here the results would be the same as in *D*, except that it would be necessary to swing the spacecraft antenna beam somewhat away from the direction to Earth in order to measure the spectral width of the incoherent signal, Δf . This is because the main forward lobe of scattering from these particles is wider than the antenna beam width, since the particles are smaller than the antenna. Resolution in the ring plane for this case is set by the illuminated area.

For case *B*, signal properties would be as in *A* at S band and like *C* at X band.

For case *F*, where the ring particles are larger than both Fresnel zones, signal strengths at both wavelengths would be reduced in a characteristic manner when a particle passed across the line of sight from the spacecraft to Earth, yielding a measure of particle size. For *E*, this reduction would be much more pronounced at X band than at S band.

There are other features that will be considered in future studies, such as the effects of an ionized gas that might accompany the ring particles. Also, there is work remaining to be done in theoretical studies of scattering from particles of various characteristics and in inversion studies to relate measurements to particle properties. However, at this time I would say that these are not the most vital considerations relative to gaining maximum information about the rings of Saturn from the radio occultation experiment. The most critical problems are related to such matters as instrument capabilities, details of the geometry and timing of the trajectories, and the ability and agreement to maneuver the spacecraft for radio beam swinging purposes. All of the above considerations will require much attention from radio team members from now until the experiment is conducted in 1980 or 1981.

Robert K. Soberman

THE SISYPHUS PARTICLE DETECTOR

I will describe the particle measurement subsystem we are planning for the MJS 77 missions.

Briefly, our scientific objectives with respect to Saturn's rings are as follows:

Measure the particulates outside of the visible rings. These include particulates orbiting in possible more distant (fainter) rings and particulates scattered out of visible rings.

Measure the meteoroid environment in the vicinity of Saturn.

From the above, develop an understanding of the dynamics of the rings with respect to their collisional interaction with the environment.

With the present project constraints, we will be passing outside the visible rings. The intent is to measure those particles that may be orbiting further out, such as the possible D' ring particles and those particles scattered out of the visible rings. Another objective is to measure the meteoroid environment in the vicinity of the planet and try to develop from this an understanding of the dynamic history of the rings with respect to their collisional interactions.

The parameters to be measured are listed below:

Spatial distribution

Orbital distribution

Size distribution (by optical detection)

Mass distribution (by impact detection, using the telescope primary mirrors as the impact target)

Density?

Albedo?

Composition?

I have put question marks on the last three parameters to be measured. Our success with these depends on how closely we can combine the size and mass distributions from the two different kinds of detectors (i.e., impact and optical). If one can determine the size distribution and the total amount of light scattered, one can determine the mean albedo of the particles. Composition has to be the most difficult parameter, but from albedo, density, and scattering properties, one can hope to determine whether the particles are ice or stone.

Figure 1 gives the geometry for the optical particle detector or Sisyphus. It depends basically on a variation of the parallax principle. We use multiple telescopes. Three is the minimum needed; we have a fourth for redundancy and data improvement. If one can measure the entry and exit time through the overlapping fields of view, then one can mathematically solve for the trajectory relative to the instrument. Briefly, it requires the solution of the five vector equations with the notation shown in figure 1. Because of spacecraft constraints, the telescopes are to be extremely close together, and consequently the parallax is small. This means that the velocity measurements, particularly in the direction of the optical axis, depend to a large extent on how slowly the particle is moving with respect to the instrument.

Figure 2 shows the planned instrument sensitivity for MJS 77 as compared to the instruments now flying on Pioneers 10 and 11. The instrument is particle-range-to-radius dependent. A number of improvements allow us to improve the signal-to-noise ratio substantially over the Pioneer version (our first flight attempt). The various sensitivity curves are for different electronic bandwidths. By narrowing the bandwidth, we raise the signal-to-noise ratio but degrade the accuracy of the time resolution. At 9 to 10 A.U. in the widest bandwidth, we would obtain about

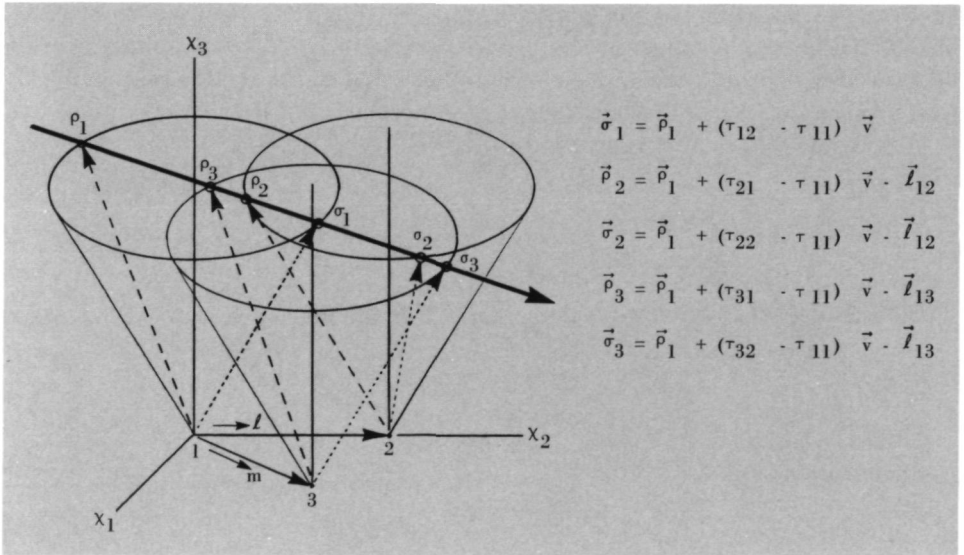


FIGURE 1.—Sisyphus geometry.

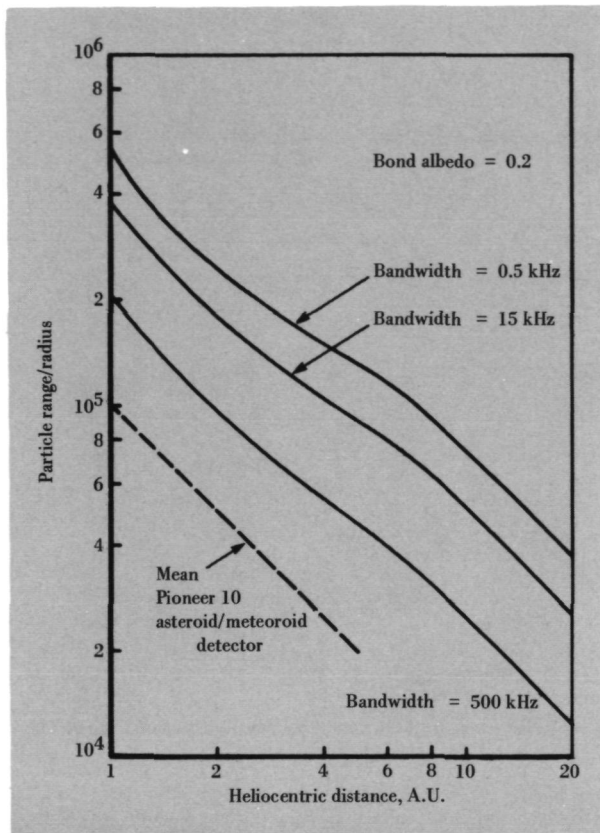


FIGURE 2.—*Sisyphus* sensitivity vs heliocentric distance for MJS 77 trajectory.

2 to 3×10^4 for the range-to-radius ratio if we assume a Bond albedo of 0.2. The sensitivity improves at the narrower bandwidths. I might also add that we plan to be looking at particles in two electronic bandwidths simultaneously (which gives us additional redundancy). The narrower or more sensitive bandwidths are particularly applicable to the large particle sizes we have heard discussed these last 2 days (i.e., on the order of meters in diameter).

I might point out that when you get to large boulders at large distances, with our parallax there is no ranging or orbit measuring capability. If one makes a reasonable assumption of the velocity of the particle passing through the fields of view, then one has a way of gauging the range from the total time in the field of view and hence the particle size.

Figure 3 is a cross section view of one of the telescopes. An acoustic detector is shown mounted on the primary mirror. This will be in coincidence with a light flash pulse detector. A particle that strikes the primary mirror would be detected by both the acoustic and the light flash detector. We have approached NASA Headquarters with the idea of adding an ionization detector. This would measure the ions that are generated at impact. The light flash detector is subject

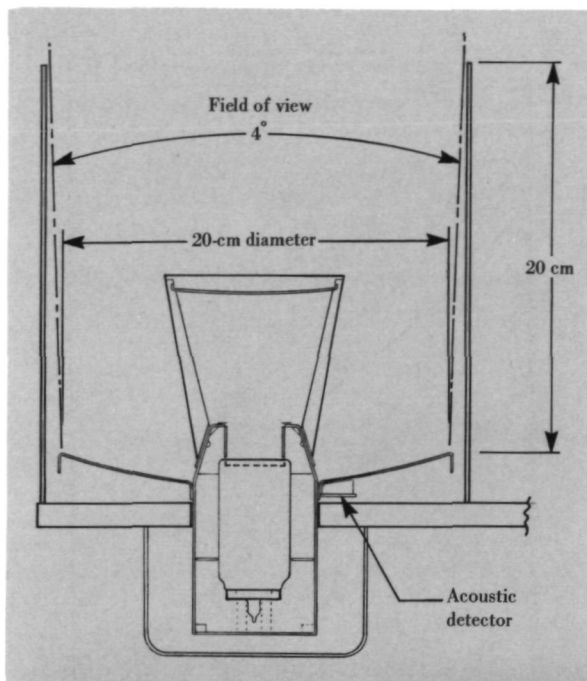


FIGURE 3.—*Sisyphus* telescope equipped for impact flash-acoustic detection.

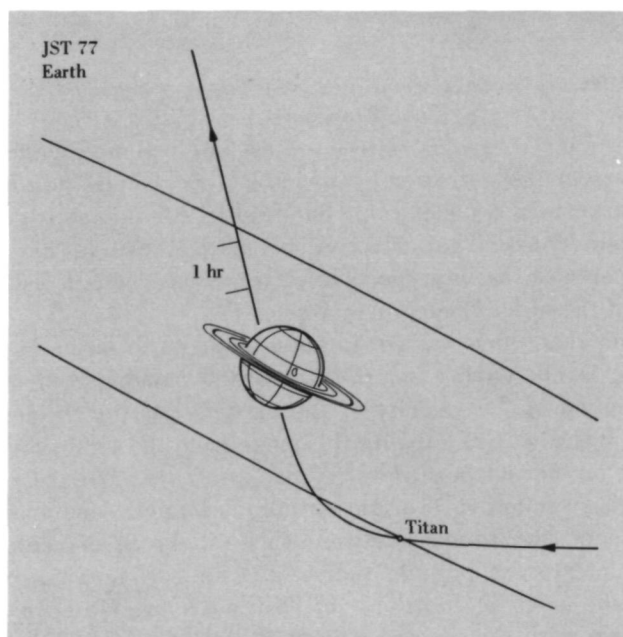


FIGURE 4.—*Saturn* flyby trajectory, view from Earth.

to noise. If we were to utilize only coincidence with the acoustic detector, we would have to raise the threshold of the light flash detector to particles no smaller than about 10^{-10} g. If we could utilize coincidence with an ionization detector, we could trust the combination down to 10^{-14} or 10^{-15} g particles.

We are still addressing a number of problem areas. The first, trajectory, was just discussed by Von Eshleman (see contribution by Eshleman). He would like to see occultation by the planet at a fairly close range. For the optical detector to work, we need to cross the ring plane while still outside the planetary shadow. This is in conflict with some of the requirements of the other experiments and is currently being examined by the MJS 77 project. We also have a problem with the acceptance angle for the particle impact detectors. These need to encompass particles that

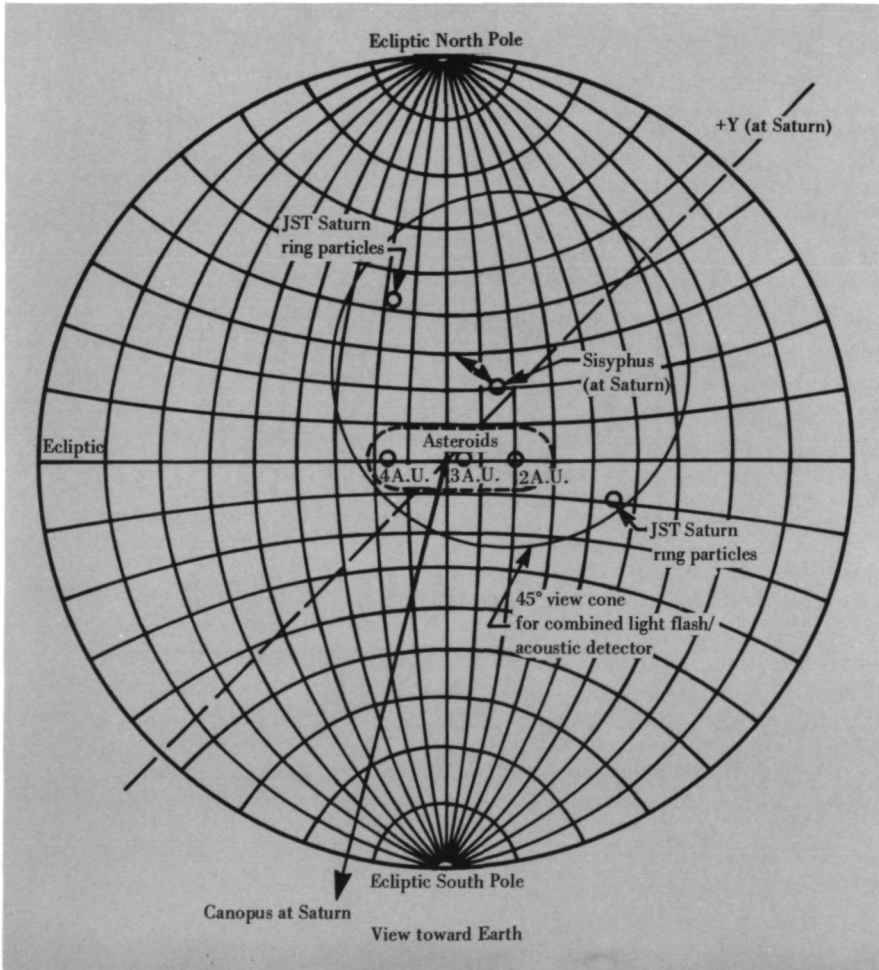


FIGURE 5.—Direction of particle encounter relative to the instrument and spacecraft for two MJS 77 Saturn encounter trajectories.

may have widely different encounter trajectories for the two spacecraft. Finally, we also wish to avoid illumination by the planets, since this would degrade our sensitivity.

Figure 4 shows a sample trajectory, and you can see the problem of trying to keep outside the shadow of the planet during ring plane passage.

Figure 5 addresses the particle encounter problem with the two guideline trajectories. It was possible to have a light shield around the telescopes and still allow a particle impact acceptance angle of about 90° into the primary mirrors for both the JST and JSI¹ (or currently a JSX-type) trajectory past Saturn.

Figure 6 is an example of the data that we have obtained from Pioneer 10. All that is required for this plot is to measure the time that the particles spend in the field of view of the telescopes. We believe that we have seen several of the Kirkwood gaps in the asteroidal debris and a sweeping effect due to the planet Mars.

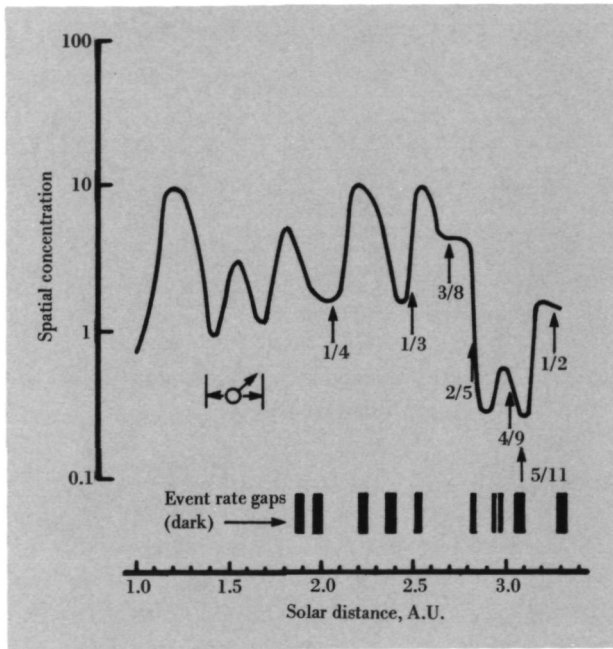


FIGURE 6.—Relative spatial concentration of particles as a function of solar distance.

DISCUSSION

James Pollack How big are these particles (in figure 6)?

Robert Soberman These particles are primarily less than a millimeter.

Von Eshleman How many counts do you have at a given distance? What is the error bar?

Soberman The error bars are substantial but nowhere wipe out the gaps. These are 2-week averages of particle events with a smooth curve drawn through average data.

As far as developing error bars, what we did was to assume the worst of two possible cases for our uncertainties—that is, the square root of the number of particles in each 2-week period times the mean time spent in the field of view, or the longest time spent by a single particle in the field of view. We use the larger of those as the uncertainty.

Brad Smith Bob, what do you do on Pioneer 10 since it is spin-stabilized? Aren't you getting stars flashing through all the time?

Soberman Yes. We have a star exclusion circuit. We measure in 3° segments around the rotation cycle, and, if we see an event in the same sector of the sky on the next or succeeding cycles, the instrument excludes it.

Smith Do you have a cone angle of about 90° ?

Soberman Yes, our optical axis is 45° to the spin axis.

¹ Designations for trajectories being considered for the MJS 77 missions.

GAPS IN OUR KNOWLEDGE OF SATURN'S RINGS

Gordon Pettengill We now have the rest of the time we want to spend here today to go over what we have heard in the last day and a half. I suggest deciding first of all what we would like to know, looking both at what we think we know as well as what we're sure we don't know, and remembering there is a grey region in between where we are wrong but think we aren't. Then, after we have identified those areas, let's try and see what techniques might be brought to bear to improve our knowledge.

My first question is from a naive point of view. Why is it that only Saturn has rings? Is there a simple one-word answer to that?

Fred Franklin I have an answer.

Pettengill Because it has Mimas?

Franklin Because it has Mimas, essentially, but I don't think I can say much more. I think the arguments are clear insofar as they are arguments. It is really speculation at this point.

Pettengill Is there anyone who would disagree with this simple reply?

William Irvine Why doesn't Jupiter have rings?

Franklin First of all, in the Jovian case the temperature is high enough that you wouldn't expect an ice ring, but you might have a tenuous rock ring. And, while you could, of course, maintain ice at the distance of Uranus, it turns out the same arguments apply to the Jovian case as to the Uranian case—namely, the absence of a suitable satellite resonance within the Roche limit region. Of course, there is Jupiter V, but I don't quite know how to introduce it into the discussion since it has such a small mass.

Brad Smith But it is of the order of the mass of Mimas.

Franklin Oh, it is less, a tiny thing with an estimated 50-km diameter or so, perhaps much less.

Robert Murphy Perhaps as little as 13 km, but that is an albedo radius and could be considerably larger if Jupiter V is unusually dark.

Irvine And Io is too far out?

Franklin That's right. However, I should like to mention a curious type of resonance that results from the interaction between the mean motion of a satellite and the orbital precession of a close-in particle caused by the oblateness of the primary. Sometimes the period of this precession is the same as the orbital period of a distant satellite, yielding an important perturbation. In fact, I think you can build a case for the gap between Saturn's C and D rings arising from this type of resonance with Titan. The agreement is good, although I have to confess it is not as nice as one would like.

Pettengill Then you feel our understanding is complete in the sense that we think we know why Saturn has rings and also why no other planet has any?

Franklin I am not completely certain, but I am giving possible suggestions that have to be looked into.

James Pollack Have you looked at Neptune?

Franklin Neptune is a very curious planet with a large retrograde satellite. I was hoping to confine my arguments to planets with essentially regular systems of satellites. Of course, the argument applies in the Neptune case, but it is such a curious planet that I would be happy to ignore it, not because the explanation doesn't work, but simply because it is such an unusual case.

Pettengill There could be rings on some other planets that are too thin to be seen optically, I suppose. What are the limits to completeness in the argument?

Franklin Well, people suggested at one time that there was in fact a ring around Jupiter, but that one didn't see it because the inclination of the Jovian equator to the ecliptic was so small that it only appeared as a thin line against the planet.

Smith There have been claims of a photographic confirmation of the shadow of the supposed ring.

Robert Soberman How far out does it extend?

Smith You don't know how far out the ring goes because you only see it as a shadow across the disk.

Pettengill I assume that MJS will do something in the vicinity of Jupiter that might bear on this question.

Soberman In fact, that is one of its objectives. MJS is far better off than Pioneer, because it will pass closer to the equatorial plane.

Smith The time to look from the Earth would be right at the moment of passage through the ring plane; unfortunately, that happens next February when Jupiter is practically in solar conjunction.

Murphy Isn't some of this just speculation based on the possibility of an atmosphere circulating around Jupiter like the Titan hydrogen atmosphere around Saturn, in the form of a torus and lasting for some time, so that we might have a similar effect leading to a temporary atmosphere on Io? The phantom ring may, in fact, be the result of a temporary ammonia torus around Jupiter.

Smith But aren't we dealing with reported visual observations rather than speculation? For example, we have photographed very fine dark bands precisely on the equator at Jupiter, but as you watch them rotate they come to an end and are not continuous. So their origin as a feature in the atmosphere seems quite

clear. However, it is these features that may have misled observers in the past. *Charles Lillie* I was going to add that such observations are also one of the objectives of our experiment: that is, searching for atoms in the sky background in the vicinity of Jupiter that would be attributable to dust in the equatorial zone or near the libration points.

Pettengill Let us move on now to the question of the origin and stability of the rings. I think we have heard some comments on this from Dr. Franklin, at least, in an earlier presentation. Dr. Pollack, what questions concerning origin and stability could be asked? And what sort of answers could we expect to get? Is there any way we could attack this question from another point of view, and are we overlooking something?

Pollack This, of course, is one of the most interesting questions, but at the same time it is always difficult to tie down. For example, even with samples in hand, people still don't agree on the origin of lunar rocks.

For the rings, I have suggested that meteoroid bombardment may have produced a significant diminution in the mean size of the particles. There are two obviously important things that need to be done and that can be done with MJS to shed light on this. The first is to investigate meteoroid bombardment in the vicinity of Saturn; we have no idea of its magnitude. I think that just this piece of information by itself will help tie down whether my suggestion is sensible. I am sure this measurement would automatically result from TV photography of the craters on the satellites of Saturn, but I raise it just to emphasize this particular application and to point out the bearing on the possible origin of the rings.

The second thing, which again uses the TV, is seeing if there are any small parent bodies located near the rings that can be photographed. By "parent bodies" I mean pretty big objects, so I assume they could be photographed.

Pettengill You are suggesting that large remnants of the original material are still in the rings?

Pollack Yes. In the model I proposed, most of the mass, even after 4.5 billion years, still resides in the parent body. After excavation of the parent body's outer shell, you generate many small particles, but most of the mass still stays in the parent body. So we expect a few relatively large objects in the vicinity of the rings where they could be photographed.

Pettengill Do you think these will stand out as lumps against a smooth disk?

Pollack Yes, that's right, although a detailed picture is hard to draw because you can make a consistent model if you start out with just one relatively large object or a number of small objects, so it is hard to say. But let's put it this way: anything between a few meters up to 100 km would fill the bill. So, presumably, if these fragments lie between 1 and 100 km, they would be easy to photograph.

Hugh Kieffer I think there are a couple of areas of dynamics that could certainly be looked into profitably following this line of discussion. One of them is the stability of a model such as Dr. Pollack has suggested.

Another one came to mind this morning. We always talk about the rings being

planar because collisions would damp out inclinations very rapidly; however, let's just consider the following scenario and see what it implies. Suppose some object comes into the rings at moderate inclination, impacts, and results in a scattering event that generates a family of smaller objects which still have some finite momentum outside the orbital plane. This is a ping-pong-ball effect and generates more and more of these fellows. Each collision, presumably, will generate at least one, and perhaps more, small objects with finite inclination. But the number will increase and the rms inclination will decrease, and eventually we will have a nice plane again. There is certainly a relation between the effective mean thickness of the rings and the interval between and magnitude of these impacts. I am not aware of any discussion of this process, although Dr. Franklin may be.

What is the steady-state radial distribution for the rings. It seems to me unreasonable that they are all coplanar within a few microns, and, on the other hand, it is probably unreasonable that they are hundreds of kilometers thick. Someplace in between lies a steady-state solution that might perfectly well be compatible with the volume density requirements for the opposition effect. I think this is a fruitful area for a dynamical study that ought to give us some feel for what ring thickness results from the new material coming into the rings.

Franklin That is a very, very good suggestion. I think the big unknown is the degree of elasticity of the collision. Does anyone have any feeling for ice sphere collisions?

Kieffer Not since I was a kid! Certainly there is a range, and we could try to bound that range as a function of the model. It will obviously be different for iron meteorites than for fluffy snowballs.

At these temperatures, however, the elastic constants are not really that different. Particles that are more than a molecule or two in size are likely to be very elastic at those temperatures, and we must remember that the relative velocities are small. Suppose the ring is a kilometer thick. That means the impacting velocities are half a kilometer or a kilometer per day, or something like that.

Franklin Along those lines I might mention one calculation I did just for fun. I took Saturn, put a group of massless ring bodies with zero inclination on a fundamental plane, and considered a massive satellite, like Titan, which has an inclination to Saturn's equator of about a third of a degree. Then I asked how long do you require perturbation to give a spread of inclinations that might be significant. The answer is that after about 40 Titan periods, or almost 2 years, you find that this initially planar layer is spread out to about plus or minus 300 m. Here is a source of energy; but whether or not the particles would remain spread is the question you raise.

Pettengill Your feeling is that this possibility has not been exhaustively studied?

Franklin It has not been touched. All I did was to see if there was sufficient energy from this source; I didn't worry about long-term evaluation.

Pettengill Energy from Titan?

Franklin Yes, which I think would do the main stirring out-of-plane as far as satellites are concerned, but there might be random events from larger meteoroid bodies.

Pettengill What is the largest bound on the thickness which could be placed from the theoretical studies which have been undertaken so far?

Franklin I don't know of any.

Pettengill Even an upper bound?

Franklin No.

Pettengill Do you mean you don't know of any theory that allows it to be other than a monolayer?

Franklin Well, naively, Titan provides a driving force, and one can calculate an upper limit of sorts from its present inclination. But that, of course, is quite large. I don't know of any other upper limit.

Pettengill But this isn't an untouched field. Surely people have been working on it?

Franklin Well, the only comment I have ever seen is the remark of Sir Harold Jeffreys (1947) who pointed out that if you did have a thin ring and introduced particles into it with high inclination, they would quickly collide and contract and form part of that thin plane. His argument was simply a statement with no elaboration. I blush to say that I am unaware of any quantitative calculations.

Irvine But you have looked at the effect of satellite perturbations.

Franklin On a planar model, i.e., zero particle inclination, we looked at the radial structure but have done nothing on the vertical profile at all.

Pettengill Haven't we just identified a gap, then, in certain types of theoretical research that could profitably be pursued?

Franklin Yes, I think so.

Irvine What do you think, Dr. Franklin, about the questions associated with the use of a monolayer? In your calculations you placed particles such that they didn't collide. But if you had started out with a random distribution, I wonder if it is not possible that collisions would significantly alter the result? Because of differential rotation there is a redistribution of angular momentum in the course of time, and it is not clear to me what evolution one might get in the ring plane if you start out with as dense a monolayer as required to provide unit optical thickness.

Franklin Yes, I see what you are saying. But I know of no treatment of that type of problem at all.

Pettengill The observational information we have on thickness, from what I have heard in the last day and a half, is also incomplete. One appears to have an estimate on the order of 2 km based on measurements made at the passage of the Earth through the ring plane. But it seems to me these observations could, in fact, be associated with optically thin material, which is nearly invisible when looked at away from the plane. When one is in the plane, however, it might dominate because of the great thickness one is looking through at that moment. This might lead to an unusually large apparent thickness compared to that of the major mass of material.

Kieffer I should reiterate a statement that may not have been completely understood the first time I made it. The 2-km measurement could represent a very gross underestimate of the thickness because it does assume that when you look at the rings edge on, you are seeing high albedo material.

Pettengill You are saying that if the outer material in the rings is unusually dark, the rings could be much thicker than 2 km.

Kieffer Well, not unusually dark in terms of the solar system.

Pettengill No, but unusual in terms of the rings of Saturn.

Kieffer If the material at the outer edge of the A ring is normal compared to the rest of the solar system, then the ring thickness has been underestimated by an order of magnitude. That is a possibility which should be kept clearly in mind whenever you use this limit on the ring thickness.

Pettengill Let's ask about other methods that might exist to determine the ring thickness independently of the in-plane brightness observation. How else can we get at the problem of determining the thickness?

Kieffer Well, the same techniques of thermal measurement that are used to determine asteroid size could presumably be attempted. An infrared observation, when the rings were edge on, could be a useful piece of information.

Pettengill Assuming an emission constant?

Kieffer The same technique that is used to balance the energy. If things are dark, then they ought to be hot, assuming constant cross section.

The Hawaiian group may be able to make an estimate right off. Would you anticipate any signal at all if the ring thickness were 10 km or 20 km?

Murphy How large is 5 seconds of arc at that distance?

Franklin About 40 000 km.

Murphy Well, if you had a 1-km thick ring, you would have 40 000 km², which is sufficient area to give you a signal. In ring passage you should be able to make the observation at 20 μ m, but not until 1980.

Pollack I am going to comment on Dr. Smith's remarks today. He can probably not do much better from a spacecraft than we can do from the Earth in terms of placing lower limits on thickness if the spacecraft is constrained by the postulated D' ring. However, if you are willing to say the D' ring really isn't there, you could get a lot closer and make much better measurements. So the fear of a D' ring has a very strong impact on the spacecraft's ability in this area.

Smith It not only affects the measurement of ring thickness, but it constrains the whole family of possible trajectories past Saturn to look at Saturn and its satellites as well.

Pettengill Dr. Kieffer, to refer back to your statement, are you saying that if you could make simultaneous 20- μ m and optical observations during ring passage as seen from Earth, you could distinguish at least the possibility of the rings being thicker than 2 km?

Kieffer You could determine the albedo which, for the range from ices to asteroidal-type silicates, spans an order of magnitude. But I still have a feeling that the signal level in the infrared would be inadequate.

Murphy With a 1-km thickness, you would be seeing something like a tenth of

the area of Io; of course you have a colder object, but I think it is within the range of what can be done. It is not going to be an easy measurement, but it is possible.

Pollack Are you allowing for the fact that the rings would be a lot longer than they are wide?

Murphy I am using a 5 arc sec aperture. You could use a larger aperture and get more area, but you are not going to make up a factor of 10, I think. For a thickness on the order of 1 km I think you will have a signal.

Pettengill Let me ask a related question. Is it certain that the thickness would be the same throughout the radial distribution?

Franklin No.

Pettengill Based on dynamical considerations?

Franklin There certainly would be a thickening near the resonances. I have some figures suggesting that there is a slight thickening, say, at the one-half resonance of Mimas of the same order as the perturbations caused by Titan, i.e., about ± 300 m after 40 revolutions. But it is a very local phenomenon.

Pettengill Is it possible that the edge-on effects we infer are at that bulge rather than at the outer edge of the A ring?

Franklin It is certainly conceivable.

Pollack When you say "slight," do you mean less than a factor of 2?

Franklin I just did the calculation for this meeting in case it came up. The effect of Mimas in the Cassini's division region was very comparable to the thickening produced by Titan generally throughout the ring, and the region in which this occurred was quite closely confined to the one-half resonance where the particle density is very low. You might spray a few particles up and down, but whether you can get the observed light by this means is very dubious, I think.

Pollack Do you have any feeling as to whether or not you might have sort of a standing wave pattern, so that rather than a flat sheet you might have a wavy type of pattern in the ring?

Franklin I think it is conceivable that there might be some sort of a flapping mode excited in a manner similar to the one Toomre has developed for the galaxy as perturbed by the Magellanic clouds. I don't honestly know enough to say whether it would be a density wave that works its way along the ring. I suspect it would be only this flapping motion.

Pollack What do you mean by "flapping"?

Franklin It is a lateral distortion that follows a satellite around. The eccentricity of Titan is not vanishingly small. It might very well be a distortional warping, say, that follows Titan around.

Pettengill Returning to the original statement, even though the densities near the division were very low, seen edge on at the time these observations are made, the effect might still be important.

Smith Well, if the ring were thicker near the Cassini division, then, as seen edge on, there should be a diminution of the intensity of the ring outside the Cassini's division and enhancement interior of that. I don't think this is seen.

Franklin You are right. Edge on, the ring appears quite uniform.

Smith Yes.

Pettengill Let me ask another question. When you are observing the opposition surge with a resolution on the order of a second of arc, are you able to determine whether or not this opposition surge is uniform over the entire visible rings?

Franklin I was not able to.

Pettengill Did you look for this?

Franklin I did not look for it. I did total light photometry removing the effect of Saturn and was able to separate rings A and B, I thought. But even there, the values for ring A were poorer than for ring B.

Kieffer The figures you showed during your presentation seemed to give no indication of a major change in the shape. So certainly that much information is available.

Franklin Yes.

Pettengill So there is crude but not detailed information on the entire ring?

Franklin That's right. But when I look at the phase curve of ring A, I am a little bit worried that I haven't treated the scattered light properly and that there is a residual component. I wouldn't place too much faith in the phase curve of ring A, which appears very similar to that of ring B.

Pettengill If the phase curve is to be explained (and this may be subject to argument) predominantly as a volume density effect, then the fact that it remains constant while the brightness and optical depth vary a bit over the various rings would seem to imply that either the particle size or the thickness of the ring must vary.

Pollack I think you have to bear in mind what I said earlier about the Poynting-Robertson effect. Perhaps I should have mentioned during my talk that the small particles are affected more than the large particles, so that the smaller particles tend to segregate toward the inside.

Pettengill But if we looked and found no change with radius in the phase effect, wouldn't that partly undermine your statement?

Pollack The outer portion of ring A particles may be somewhat bigger than the inner portion of ring A, and similarly for ring B. I don't think we have sufficient resolution yet to see this variation.

Pettengill In other words, you are saying the barrier in the Cassini division would prevent passage from one ring to the other?

Pollack That's right.

Von Eshleman Is that a barrier to ring-particle spiraling from the Poynting-Robertson effect?

Franklin I would say so, yes. Material going from ring A into the Cassini division undergoes oscillation and has a very good chance of being returned to ring A. If one did try to interpret the rather low-quality phase curve for ring A, the only difference as compared to ring B would be a lower volume density; otherwise everything could be kept the same. You sharpen the phase curve and the volume density drops, but this gives you no information on the particle size. The difference between the phase curves of rings A and B could be explained

simply by a lower fraction of volume filled by the particles in ring A as compared to ring B.

Irvine But you didn't find that.

Pettengill Are we able to make measurements with sufficient resolution to look within a ring? Is that practical?

Murphy Well, Fred and I were just discussing the prospects of doing this with photometric scanning at Mauna Kea. The major drawback is that opposition occurs on Christmas Eve and we think that is an uncomfortable time to observe, but we think we can probably get something on the order of 1 second of arc or better resolution across the disk and will do so, presumably.

Pettengill That will give you a few elements within a ring.

Murphy Yes.

Irvine But what you really need to do is observe at increasing phase angles as well.

Murphy One has to observe for about a month about the opposition.

Pettengill It sounds to me like a worthwhile set of observations, which might very well give you information on the homogeneity in particle size and thickness over the rings.

Kieffer That schedule seems to conflict with comet Kohoutek observations.

Murphy Actually, it doesn't, because Kohoutek would be near the Sun, while Saturn is in opposition.

Irvine If you could get some spectral information, that would be very valuable.

Franklin Can you suggest wavelengths that are particularly interesting?

Irvine I would say that the widest wavelength spread that you can get would be desirable, along with the visual for comparison.

What is the inclination of the rings going to be?

Murphy Between 26° and 27° .

Pettengill Let's move on now and generalize the questions as to what we know about the ring distribution in size and space. I gather that there are uncertainties regarding its limits both inside and outside, which much of the instrumentation on MJS will help to answer. But are there any other types of observations we can make from Earth, at least in the interim, which could shed light on that problem?

Pollack I was wondering whether there is any way we could get spatial resolution using the occulting radio sources we were speaking about, but if there is a way it is not obvious to me.

Pettengill You couldn't get resolution? Why not?

Pollack I guess the question that worries me is whether in effect you would be illuminating just part of the rings at one time.

Pettengill If you use a point source, you have a high degree of resolution. The use of these sources in occultation is common in astronomy.

Pollack In that case I would say that would be a way of doing it. You have some potentially very rich particle size information, if you have a small enough source. In addition, you will be able to map the sizes as a function of radial distance.

Pettengill Are there any more conventional types of observations that have been

overlooked, or have people done about as well as the current techniques allow?
Pollack Well, I think radar observations are also quite significant. It seems you are getting a contribution from both rings A and B.

Pettengill Is there any current optical technique that can be improved?

Jacques Blamont I would say that more effort should be devoted to observing ring occultations; not very many have been done.

Pettengill Are people currently geared up? They seem to know when Io is occulting something; why not the rings? Isn't there a group at Herstmonceux that calculates these things?

Smith But the occultations are very rare.

Franklin They occur every 20 years or so.

Murphy There was an event last April which some Lowell people came out to Hawaii to observe. The only result of any kind came from John Thompson's box camera which was set on one of the extra telescopes. He has a picture made part way through the occultation that appears to show an image, very marginally, from which he estimated an optical depth for the A ring of around unity.

Pettengill You are saying modern instrumentation has not really been brought to bear on this?

Murphy As a matter of fact, and I think Dr. Franklin will bear me out, there has been no successful photometric observation of a stellar ring occultation. What exists consists of someone saying the star is going behind the rings, now it's fainter, now it's invisible, now it's more invisible, and then somebody tries to quantify these impressions. The Lowell people did observe using an early scanning photometer, but the event was something like 4.5 hr off the meridian. The image was jumping around, and they simply weren't able to make a measurement. The problem is to capture the event.

Smith These are faint stars.

Murphy Yes, this was an eighth magnitude star and just too faint.

Kieffer Has anyone recently tried to make observations in the Fraunhofer lines, taking a good star that has lots of energy in the calcium line or something and just looking in that line alone? Then you don't have to worry about the light scattered by the planet from the Sun.

Pettengill You would have to have a pretty bright star to do that.

Kieffer Well, it has to be bright, but I think you would probably get rid of some of the problems that are inherent in using main sequence stars.

Pettengill But I gather the probability is just plain low that this would happen.

Dr. Blamont, could I return to your suggestion and ask how you would propose to implement it, or what would you like to see done?

Blamont First initiate a prediction program. I don't know if there is anyone doing that.

Pettengill I think it is being done, but, of course, only for stars up to some limiting magnitude.

Torrence Johnson G. Taylor from Herstmonceux sent around a circular recently

to people who had been involved in satellite occultations, giving his predictions for the next 5 or 6 years. There aren't any ring occultations of any stars bright enough to be included in the catalog.

Pettengill And what is that brightness?

Johnson I'm not certain, but probably about eighth to ninth magnitude.

Pettengill Do I conclude that this doesn't seem like a promising endeavor?

Johnson Well, if occultations could be found it would be.

You have an additional problem that an individual occultation is not visible over too wide an area on the Earth. You may not have a big telescope in the right place.

Smith In addition to stellar occultations, Iapetus can also go behind the rings.

Franklin All you get from that is an optical thickness. That is nice, but I don't know that it is that important.

Irvine But it's never been done.

Franklin That's true. You might get optical thickness variation. The Herstmonceux people do these calculations if you ask them to, as a rule.

Johnson I am not sure about the satellite events, but I would expect that type of event to be listed in their regular publications.

Frank Palluconi In relation to observations that might be helpful, I would like to return for a moment to a comment Dr. Pollack made, stressing the importance of additional information about material outside the A ring in the D' region. The presentation I made earlier covered essentially all the observations there are. They come down to very few, and even these are questionable. The other point I mentioned involved silicon-imaging-photometer observations at a time when the rings are wide open. It's certainly a much more difficult undertaking if the material is uniformly distributed. However, the possibility exists of narrow rings outside of ring A with a local increase in density over what you might expect for the average. Dr. Alex Goetz is here, and he can describe this kind of observation as a means of assessing the hazard of passing closer to the planet than about 6 Saturn radii.

Alex Goetz The problem is getting a measurement of the brightness distribution. A lot of work has been done with photography, but the problem with photography is that you are limited in dynamic range and linearity. You have, in particular, a limited wavelength range that does not allow you to make measurements where you might be able to get rid of the scattered light problem. For instance, it is possible to take measurements in the methane band getting rid of the scattered light from Saturn.

Smith That is not the problem, though. It is the rings.

Goetz It is the rings, I know, but if you want to go out to 4 or more Saturn radii it is possible to determine what the scattered light profile is by turning the ball of the planet on and off and using the silicon vidicon. The data are in digital form and the dynamic range is on the order of 800.

It is possible to build models of the scattered light, which will be different for every point in the image plane, and reduce the data in such a way to possibly

get down a factor of about 1000 below what you might otherwise be able to do with photography.

Murphy Taking Kuiper's observation of the D' ring at something like 15th magnitude per square arc second, and considering that he may have been looking along the whole path length equal to the diameter of the rings, a quick and dirty estimate—including the fact that we are now looking at a high elevation angle—shows that the rings would be about 12½ magnitudes fainter now. If you had up to 15 to begin with, this is something well below the sky limit.

Smith Yes, if the material is spread out, what you say is absolutely true, and the only hope of seeing anything now is if it's not uniformly spread out but in some narrow external ring.

Goetz If you have a thin ring, it is possible to use the coherence of this ring in doing the processing to bring it out. But you are right; if the material is uniformly distributed we probably won't see it.

Palluconi In interpreting the photographic evidence, particularly Kuiper's, it should be noted that if there were, say, just one thin ring responsible for the total brightness, you would expect to see a radial change in brightness. You would expect to see more light from the ends, in the ansae. Now that is not really observed, and I don't know if it is completely ruled out by the existing photographic evidence.

Kieffer Is it worthwhile going back and looking at those plates using more exotic image processing?

Smith It is worth going back and looking at the plates with just the eyeball, because lots of things happen to images when they get copied and printed even on photographic paper.

Kieffer That might be straightforward.

Smith Very straightforward.

Palluconi The hazard associated with the presence of any material outside of ring A has a very definite effect on the types of trajectories that can be selected; currently the trajectories are not to approach the planet more closely than 4 Saturn radii. I think it would be helpful if some comment could be made about how appropriate a minimum approach of 4 Saturn radii is—how, for example, Kuiper's photographic results suggesting material out to the orbit of Dione at 6.3 radii affect this consideration.

Equally important is any kind of suggestion for improving the estimate that exists. The problem is an important one, and even though encounter with Saturn is 8 years away, things have a way of happening and getting fixed very early in planning for a project of this type.

Smith Like this fall!

Palluconi Any suggestions and comments, particularly on the model presently being used, would be quite helpful.

Pettengill Let's return to finding better ways of delineating this distribution. How far can the polarization approach be pushed, using the fact that the reflected light from Saturn's disk is polarized?

Murphy Are you talking about the transmission measurement?

Pettengill Yes.

Murphy It definitely needs to be done again because it's a very tricky measurement, but it needs to be done in several wavelengths. J. S. Hall and L. A. Riley are working on this, and we will attempt it next year closer to opposition.

Pollack I'd like to see it done with good spatial resolution.

What I personally like about this measurement is the fact that when you are dealing with polarization, it is the single scattering events that are really important; thus you would get around some multiple scattering problems present in other observations and their interpretation.

Murphy Bear in mind that the region where you are looking, the B ring, is just a little over 1 arc second wide.

Eshleman Wasn't there also the question of sensitivity when looking at greater radii?

Murphy In the next couple of years, as the ring tilt starts decreasing, we will be able to do the A ring and then look for subtle effects beyond the A ring. Is that what you are driving at?

Eshleman Yes.

Pollack Yes, that was what I was driving at, getting the A ring also.

Pettengill So there will be some further work done there, but mostly taking advantage of the change in geometry?

Murphy That's right. But if you are making final decisions on the trajectory this fall, isn't it too late to make these observations?

Smith Well, not entirely. There will be a pair of trajectories defined this fall and there will be another iteration after Pioneer 10, so that does allow winter-type Saturn observations to be folded into the trajectory selection.

Pettengill Are there any other optical techniques anyone can think of that bear on the distribution of particles in the rings?

Smith Well, there are the obvious 1980 observations.

Pettengill Let's move to the infrared observations and then on to the radio.

Murphy I think both Dave Morrison and I will be looking at the rings some more in the 10 μm and 20 μm windows to try to understand the difference between the temperatures at these wavelengths. Over the next 5 years, we shall also try to confirm the temperature variation with phase or with the Saturnocentric declination of the Sun. We shall also be working on the 30 μm to 40 μm region and trying to calibrate those measurements against something other than Saturn's rings.

The other possibility is archival; namely, to speak to Frank Low and try to get more information on his upper limit from 1965. We can also try to check out the rumors of another temperature measurement around 1967 or thereabouts. I believe he indicated another temperature—a positive measurement—and if that information can be obtained, maybe we can pin down the temperature variations.

Pettengill I just realized I might have been a bit hasty. What about the Lyman

alpha? We skipped over that in trying to move forward. Is there anything planned, for instance, to look through the hydrogen atmosphere that exists around the rings?

Lillie I think that W. Fastie and W. Moos at Johns Hopkins University are planning spectroscopic observations of Saturn in the next year or so.

Pettengill So that would be useful and relevant information.

Lillie They should be able to contribute information on the Lyman-alpha emission.

Pettengill Does anyone know about Skylab's instrumentation?

Lillie There is a possibility that something might be flown for comet Kohoutek. There may be a Lyman-alpha instrument on the last Skylab mission, and there is no reason why that couldn't be used to look at Saturn. I don't know the threshold of sensitivity, however.

Pettengill What is the appropriate group or the appropriate person?

Lillie That would probably be G. Carruthers and C. Opal at the Naval Research Laboratory.

Pettengill Do you communicate with them in this field?

Lillie I will be talking to them, yes.

Pettengill I think it is worthwhile asking whether they considered these observations.

Lillie I can ask and find out.

Pettengill Well, okay, let's get back to the infrared.

Murphy We should also find out Dave Morrison's plans. I am not planning any eclipse measurements, but I assume he is. Dave, what about 10 μm in particular?

Dave Morrison I have no plans, but I think it is worth asking the model builders to see whether it is worthwhile.

Murphy I am questioning 10 μm in particular where you can get closer to the shadowed region where the ring particles emerge from or enter Saturn eclipse.

Morrison I think such measurements should be made, but when I hear that people are likely to do them with the Hale 200 in., I worry that they will do them so much better that perhaps we shouldn't try. However, I think if you are suggesting observations, using the high spatial resolution of the Hale telescope for this kind of work at 10 μm it is a fairly obvious and useful experiment to do.

Pettengill Let's move on into the millimeter wavelength region and the general radio domain. There are a number of areas that have been identified from both a theoretical and observational standpoint.

Murphy Excuse me, could I back up to the region between the infrared and the radio? There is some other data which bears on this.

Pettengill You mean the 100 μm data?

Murphy Yes, the 100- μm Armstrong, Harper, and Low (1972) information. They indicated a decreasing brightness temperature of the combined rings and disk, suggesting that the rings were becoming transparent.

Pettengill Decreasing with wavelength?

Murphy Yes. As they went to longer wavelengths, they found a smaller brightness temperature and said the rings therefore must be getting transparent. There is, I think, a fundamental problem with flux calibration in that paper in

- that they assumed a temperature of 235 K for Mars. There is a paper by Logan, Balsamo, and Hunt (1973) which shows variations up to 285 K during the period of observations; as a result, I don't know how they got consistent results at all.
- Pettengill* If we believe that the brightness temperature go from 90 K at 20 μm to 20 K at 1 mm, and I may not have it quite right, there must be a lot to be learned in the intervening regions of the spectrum.
- Murphy* The temperatures do have to drop somewhere, but there are flux calibration problems in these measurements and there is also the fact that Saturn itself changes in brightness temperature, for which no allowance is made.
- Pettengill* Well, do you think it is such a difficult measurement that no one can make it well?
- Murphy* My quarrel is with the calibration.
- Pettengill* You are saying the measurement should be repeated and the older data reevaluated?
- Murphy* That would be my feeling, yes. If we wish to find out what is going on between, say, 35 μm and 1 mm, the present data will not suffice.
- Pettengill* In any event, this is an interesting transition region where a significant result can almost be guaranteed.
- Murphy* As a matter of fact, would someone care to conjecture just how it might constrain the model if we could determine a temperature at an intermediate point? By the way, this seems like a good experiment for the C-141 (a research aircraft based at the NASA Ames Research Center).
- Pollack* There are quite a number of people who are actively considering the C-141 and who will be measuring at long wavelengths.
- Pettengill* Does Ames have any program?
- Pollack* Yes. Ames, under Fred Witteborn, is going to have an interferometer on the C-141. The problem is that most instruments, including the Michelson interferometer, I think, only go out to 200 μm . You would really like to have something in the 200- μm to 1-mm range. I think that is where the action really would be, but I am not sure who will be working in this region. It is a very difficult region to work in because your telescope is starting to get too small.
- Pettengill* Is there any reason why Skylab couldn't have done some of this?
- Pollack* I think the problem is that the ultraviolet can make a stronger case since you actually have got to get above 50 or 100 km before you can do anything, while in the case of the infrared you only have to get up to 10 km.
- Pettengill* Are you saying there is room for improvement—instrumental improvement—and there are people who are willing to step into this field?
- What steps must we take to make sure the importance is recognized?
- Pollack* I think one thing that would be useful is to see who is planning to observe in the hundreds-of-microns range. I have a list of experiments in my office and I can take a look at it with this in mind.
- Murphy* I know that I. G. Nolt and company at the University of Oregon will be observing in that region. We are working together on some planetary problems.
- Pollack* On the C-141?
- Murphy* Yes.

Pollack I see. Do you know how far out Ira Nolt is going to work?

Murphy I assume at least to 150 μm , but I don't know.

Pollack Maybe it would be appropriate for Dr. Murphy to touch base with Dr. Nolt and mention the Saturn ring measurements.

Pettengill Since the C-141 is a NASA-supported project and I think it can be stated quite firmly that the data obtained from it will be relevant to MJS and Pioneer planning, it seems to me there would be a good chance of carrying out an increased level of activity.

Pollack Yes, certainly; I think the practical thing to do is to contact the person planning to work in the 100- μm range and point out the interest.

Pettengill It might help if copies of the writeup of this workshop were distributed to people with some likely usefulness in these areas.

Going on now to the ground-based radio work, I recall that, except for the 1-mm work, we can only place upper limits on the apparent radio brightness temperature of the rings. From all we know, this measurement gets harder as one goes to longer wavelengths. This seems to say that the measurement emphasis should be concentrated on the shorter wavelengths to the extent this is practical. I realize that some people will have equipment that works only at a given frequency and will want to use it, but it does look as though we have to try and get the sensitivity down to 1 K or 2 K or less in the centimeter region.

Sam Gulkis I just wanted to comment that the break between detection and non-detection seems to be in the 1- to 3-mm region and corresponds with a break in the type of instrumentation. The 1-mm work is done with a bolometer and the 3-mm work with a mixer receiver. There is a very real possibility of a difference in calibration procedure between these two wavelengths. It is something to worry about.

Pettengill Are you saying that the bolometer may be worse?

Gulkis No. I don't know which way it would go. In both of the 1-mm measurements, the intent was to use Saturn to calibrate the bolometer and, incidentally, the ring emission turned up in comparison with other planets. What hasn't been done is to use the 36-ft Kitt Peak antenna at 3 mm; the 3-mm work has been done with a much smaller antenna. So there is considerable gain to be had right there since Kitt Peak has receivers working in that wavelength range. Also, I think it is very important to go on out to 8 mm.

Pollack I know that Kitt Peak is where John Rather made the 1-mm observation; B. L. Ulich was also involved. I know that both are extremely interested in the rings of Saturn, and I think they are going to be doing a lot of work on them.

In my own mind, the best thing to have would be interferometers operating in this wavelength region. The closest we have, that I know of, is at JPL, where the 8-mm interferometer will be coming on the air sometime this fall.

Pettengill Which is the JPL interferometer?

Michael Janssen We are presently building an interferometer at Table Mt. that will operate at 8 mm. One of the first projects we will try to do will be to observe the rings of Saturn.

Irvine What about the Hat Creek interferometer?

Janssen They are building a new interferometer system with variable baseline.

That system will also be on the air, hopefully, early next year.

Irvine Won't that operate down to 3 mm?

Janssen It will operate in the K-band range, a little over 1 cm. Some time later it will go down to around 8 mm, and then in a couple of years to 3 mm.

There are a lot of if's about these short-wavelength interferometers. In particular, the question of phase stability due to atmospheric fluctuations is a problem that needs to be faced. Since visibilities increase with short projected baselines at low elevation angles, there is the danger of systematic error introduced by the longer atmospheric paths.

Irvine Do you run into problems with uncertainties in the model of Saturn's atmosphere?

Janssen That is much less of a problem. When you are looking for a source that extends out twice the radius of Saturn, the actual brightness distribution of the Saturn disk has very little effect.

Pollack That is why the interferometer is really the way to go. The other alternative is to do model fitting as Dr. Janssen described in his presentation. But to the degree that it is somewhat indirect, it's likely to be less satisfactory.

Irvine Aren't there a lot of problems with absolute measurements on the 36-ft antenna?

Gulkis There are a lot of problems with any antenna operating at that wavelength.

Janssen Yes, there are variations of the beam with zenith angle, and they are operating near the limit of that dish.

Pollack Dr. Irvine, I might say that the reason I tended to trust Rather's measurement was not because I believed his absolute temperature, but because of the distinctly greater brightness temperature for Saturn as compared to Jupiter. I think that was really the critical point.

Irvine But as I understand it, there are severe problems with thermal effects in that antenna.

Gulkis There are also source-size corrections which are quite large.

Janssen And there is an enormous extinction correction. The extinction at 1 mm is 50 percent. They made measurements over several days of the ratio of Jupiter to Saturn, with Jupiter at high zenith angle and Saturn at a lower zenith angle. Rather did say he was getting good repeatability from day to day, and he felt he was taking care of the extinction rather well overall.

Pettengill What in your experience would be the likely upper limit we could ultimately place on the brightness temperature? As low as 1 or 2 K, for example?

Janssen This involves systematic effects connected with the atmosphere, which are not understood.

Pettengill Do you have a feeling as to what we could hope for?

Janssen I would hope for a few degrees. Another possibility, which is not limited by the atmosphere, is to use a north-south baseline. In a measurement like that, you can look for higher-order visibility-function zero crossings, which are not

really affected by systematic effects in the atmosphere. That wouldn't give you a temperature, it would give you an optical depth for the rings.

Pollack What you are doing mostly is reducing the radiation from the disk of Saturn by the rings. I think the optical depth is an interesting quantity, but I don't think it is as interesting as the brightness temperature.

Pettengill What do you think of the importance of continuing the longer wavelength work? If we could find that the brightness temperature could be measured and that it bottomed out and held constant at a few degrees, even at 21 cm, I would consider that extremely vital information. Do you agree?

Pollack Yes, I think that would be really interesting.

Glenn Berge Once we get down to a few degrees, there is the problem of distinguishing between emitted and scattered radiation. If we could say that it is all scattered, then by comparing with the radar data we could find out about the anisotropy of the scattering, but somehow scattering must be separated from emission.

Pettengill I think we have already heard the plans for radar observations. Morris and Goldstein have, at least until the S-band Arecibo radar comes on, probably the only capability to see the rings. They are planning to look this fall using the present system, although they will have some instrumental improvements, such as frequency rather than on-off-source switching, which should give them 1.5-db signal improvement. They also have a lot more momentum going for them toward getting time on the equipment as compared to the last time. They have applied for some 60 nights and think it is likely that they will get about 10 nights during December because of pressures of many sorts, primarily from Pioneer and the Mariner Venus/Mercury spacecraft (which was successfully launched November 3, 1973).

They will also look again in 1974/1975 bistatically with Arecibo. That should give them something like a factor of 10 to 12 db better than they had previously, for a given observing time. This should help answer questions concerning what is really going on in the low-frequency portion of the doppler spectrum. They will also have, perhaps by the end of 1974, but more likely by the end of 1975 or early 1976, their X-band system. This will give them a measurement at about 3.6 cm, which will be a valuable adjunct to the 12-cm work they already reported on.

As far as I know, these represent the only available wavelengths for an actual backscatter ground-based radar measurement. I don't think there is any chance to do anything at 70 cm. It has been suggested that a somewhat similar type of measurement, with more wavelength flexibility, could be achieved through the observation of occultation of radio sources. This technique suffers from the same problem as the optical occultation—namely, a lack of desirable sources. But there has been an enormous increase in sensitivity in the past few years as well as considerable success in reducing source confusion through the use of interferometers. There is no reason the same techniques can't be extended to Saturn occultations. Dr. Gulkis has been kind enough to give me a curve showing the distribution of sources versus flux density as a function of wavelength. Of

course, you have to take a guess as to what the confusion limits are. But the up-shot seems to be that you may be able to use wavelengths longer than 10 cm, in other words, to fill in the missing region longward of the radar observations. In addition, we could get useful occultations as often as once a month, from about 10 cm to 2 cm, although this has to be looked at more carefully. Right now, our task is to find someone who will do it.

Irvine What sort of facility do you need?

Pettengill Arecibo is particularly good in the 70-cm range. They are instrumented, and they are planning to set up an interferometer of the sort needed for another purpose. There is no reason why they couldn't divert their attentions for the small number of hours per month required to look at the events as occultations take place. The Westerbork people may be interested, but I don't know in great detail what their plans are. Morris and Downs, who worked with the JPL radar observations, think that they may become interested.

Irvine Could you do this with the NRAO interferometer?

Pettengill You could do it there, yes. They don't have as much aperture, but they certainly operate an interferometer at low enough frequencies.

One problem I see here is that most people who are interested in doing this from a cosmological point of view are not oriented toward the solar system, and they might not pick this up. Conversely, most people who are interested in doing solar system interferometry haven't had, for the most part, a background in this type of technique. You have to find someone who has had a little bit of both.

Gulkis Before Walter Jaffe from Westerbork left, he gave me a new estimate. He said that at 50 cm he thought he could do one source about every 3 months at the 10-milliflux level, and that if this group were interested in it we should contact him. He wasn't positive of the potential value of the experiment, and he wanted this group to decide that.

Pettengill It depends to some extent on the model we believe is appropriate for the rings. But, from the preliminary evidence produced by the radar, I would say there is considerable information content in such an experiment if properly done. The reason is that very much the same scattering effects that cause the radar echo would enter into the optical depth observed as the rings occulted a source. You would also have something you don't have in the case of radar—a high degree of angular resolution—so that you would be better able to see the radial distribution. You can obtain the radial distribution to some extent with radar, but it is a different projection, so that this is an important and independent type of observation.

Pollack I would say anything at greater than 10-cm wavelength, if we could do it, would be of fantastic interest.

Irvine Do you think it would be a good idea to appoint someone to coordinate such observations?

Pettengill That sounds like an excellent suggestion, but it should be someone who is extremely interested in the idea.

Gulkis Mike Janssen and I are working together and we will write to Walter

Jaffe and try to get the program organized. I think we would be willing to look into the Westerbork array and perhaps contact other people as well.

Irvine Could you serve as a clearinghouse for information so that if someone wants to observe he can find out what other people have already done or what is being planned?

Pettengill I think the logical place for a clearinghouse is here at JPL.

Gulkis We can do it.

Pettengill You and Dr. Janssen take on this responsibility and I will keep in touch with you with respect to Arecibo, if you want.

Berge I think the major part of the effort is in surveying the track of Saturn beforehand, much of which effort will have no result.

Pettengill If we can find someone at Arecibo who is interested, I think this would be an ideal setup because they do this type of survey very accurately. They have done it in the case of lunar occultations.

Berge And once a suitable object is found, one would like to know how big it is, too.

Pettengill Not only how big, but precisely where, because otherwise we will not be able to determine where it is in the ring structure. Cal Tech's Owens Valley has a very good setup to determine positions once the sources are identified.

Berge Yes, I am certainly interested in doing some work on this myself.

Pettengill So if a source were found at Arecibo and its approximate coordinates communicated, would Owens Valley then be able to measure it to their usual 1 second of arc accuracy?

Berge I think so.

Pettengill That is what you need, about 1 second of arc?

Berge Yes, and as I said you also want to know how big it is.

Pettengill You must remove the confusion because you have to know how strong the source is before Saturn moves in to be able to interpret the change that is seen.

Irvine Wasn't that sort of thing done for the Saturn brightness temperature measurements at long wavelengths at Arecibo?

Pettengill In the case of Saturn, you are looking at a moving source whose intensity presumably remains constant, so you can remove the confusion that way.

Gulkis It is interesting to note that whether or not the rings are transparent affects the value of that Arecibo measurement. It is a question of whether or not the rings obscure the background because the background is very hot at this wavelength. This is a fairly subtle point which I think has not previously been considered.

Murphy When one removes the contribution of the disk to find out what the temperature of the rings is, does one assume no drop of temperature behind Saturn's rings because they will not be effective in reducing the solar input to the shadowed area?

Gulkis You ask that question as if the correction were done routinely. There is

only one measurement which indicates an apparent brightness temperature higher than you expect from the disk, and in that case a very simple subtraction was done.

Murphy No, in all cases where you have Saturn data, one assumes that the whole disk emits, with no shaded portion where the sunlight doesn't get in.

Gulkis All of the observations measure the flux and assume that it comes from a solid angle the size of the disk.

Mike Janssen presented different interpretations of that shadowing, but a correction has not been applied to any of the measurements yet, and, to be honest, I really don't know what to apply.

Murphy Well, perhaps it is irrelevant to know whether or not the temperature of Saturn drops when shadowed by the rings. I think the planet is just as hot behind the rings as it is anywhere else on the basis of the infrared data. But we don't know for sure, and if you cut down your insolation by 80 percent, you could have a drop in temperature. Again, you are probably working at an ammonia saturation level, and the shadowing may not be terribly significant.

Gulkis I don't really understand your question.

Murphy Well, I am only saying that you are looking at a region of the planet that is in the shade.

Janssen Are we looking at such a region or not?

Gulkis There is no spatial resolution that allows one to distinguish. The existing measurements simply assume that the emission comes from a solid angle the size of Saturn.

Irvine What infrared measurements do you have that bear on this question?

Murphy It would appear from present data going north-south that the temperature of the planet does not vary with latitude as it would according to the insolation level, but this is a very marginal result. If the temperature that one sees depends on the amount of sunlight striking the atmosphere, then behind the ring you have less sunlight. I don't know just what happens to the planet in terms of ammonia saturation.

Pollack What Bob Murphy is really saying is that the meridional transport in the circulation of Saturn's atmosphere is strong enough to render useless any sort of model that simply follows the insolation.

Smith I would like to make a comment on a different topic. I can look at the D' ring observation by Kuiper in two ways. One is to look at the original photographic material. The second is to compare his data with photographs we took, as far as I can tell, at precisely the same time. There are also photographs taken from the Naval Observatory within a day or so of that time.

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SATURN'S RINGS INVESTIGATION FROM SPACE

Gordon Pettengill It seems like the natural time now to move away from the surface of the Earth and ask what can be done from space.

Dr. Eshleman, is there more that can be done from MJS that would still be attractive as far as adding significantly to our knowledge of the rings?

Von Eshleman Well, I made my presentation in the light of some mission-related problems that we have had while trying to improve the capability of the occultation experiment. These problems arise mainly from the distance behind the rings and the ability to swing the antenna. I am frankly more concerned with asking this august group if we have missed some possibilities relative to the scientific aspects.

Pettengill The only radar-type measurement you are thinking of uses cw, isn't that right? You didn't consider any range coding?

Eshleman No.

Pettengill Would it help at all to have that capability, remembering you can code the telemetry and use a related code on the ground?

Eshleman Well, before things got way down the line, we were talking uplink capabilities and using 400 kw on this end instead of 10 W on the other end, but now we are beginning to get the project message that it is very difficult to do things uplink.

Pettengill What is the problem on the uplink approach?

Eshleman Well, it is conceivable that the ranging code would allow you to do something there. Have you thought any more about this, Dr. Tyler?

Len Tyler Well, it is not clear to me that it buys you a great deal for a circularly symmetric system. I think you might be able to do more using the doppler loci discussed in your presentation.

Pettengill And the fact that you have angular resolution.

I was thinking of the conjugate problem of observing from Earth, where you would dearly love to get some range information to help sort out the echo

structure; there are just too many different distributions that look the same in projected doppler space.

But you are saying that because of the changing bistatic angle and the angular resolution, and by assuming symmetry, that you now have a soluble problem with your presently planned equipment?

Eshleman There is so much we could do if we could point in all directions and look on approach and in occultation, all of which is beyond our present capability, that I think we may have stuck to the simpler techniques to enhance our arguments.

Pettengill So you think that represents a more fertile field to explore, namely the angular flexibility rather than the inclusion of range.

Eshleman In my opinion at this time.

Pettengill What about polarization?

Eshleman That is another possibility on our list that we would like to buy if we had that kind of money. In our case we would like switchable polarization on the S-band downlink.

Pettengill Would this be transmitted circular and received linear on the ground?

Eshleman A transmitted linear or circular option on the spacecraft at S-band and whatever you need to receive it on the ground.

Pettengill What is the advantage of transmitted linear versus transmitted circular if you receive linear on the ground?

Eshleman It is a question of observing Faraday rotation and you have to transmit linear to do that.

Pettengill Is there any chance of enough Faraday rotation to be seen near Saturn?

Eshleman Primarily near Jupiter and the Sun. That is why we are talking about adding that capability.

Pettengill What about Saturn, particularly Saturn's rings?

Tyler I think the question is whether or not switching polarization will give you some handle on the particle shape, assuming that you measure the complete properties of the received polarization, which can be done.

Pettengill I agree that filling out the scattering matrix completely may be useful, but I have a gut feeling that the first-order questions can be as easily handled with circular transmitted and linear received, and it seems to me a much more practical technique because you can adjust the plane of polarization on the ground so much more readily.

Eshleman All of these changes are charged to us.

Pettengill You would rather have the angular capability first and then the polarization flexibility, is that right?

Eshleman I would think so, particularly getting more of the scattering diagram than just the forward or just the backward. Then we would have the combination of radar and the direct Earth-looking occultation approach.

James Pollack How does the bistatic measurement work? Who transmits?

Tyler The spacecraft.

Pollack The spacecraft transmits a signal that bounces off the ring and is received on Earth, is that right?

Eshleman That is the mode we will most likely be considering.

Pollack That should yield very interesting results.

Sam Gulkis There is also the possibility of operating in the passive mode and measuring the brightness temperature of the rings. I think you mentioned this in your talk, yet I believe you don't have that capability at present.

Eshleman As you know, we hope to have the ability to look at the back of Jupiter and the back of Saturn too, as well as its rings.

Gulkis These observations could serve a number of purposes—improving our knowledge of the atmosphere, the satellites, and the rings. But at present the plans don't include the capability of making them, although some thought has been given to the problem.

Eshleman The kind of temperatures that I have heard here don't make observing the rings appear promising in itself.

Tyler Well, it is certainly much easier when you are close.

Pollack You are going to get an occultation of Titan. You could get a surface temperature from that observation if you had the capability for passive radio observations.

Eshleman I agree. Please make a recommendation.

Robert Soberman I don't know that it has been considered at all, but there is another radio detector on board the spacecraft, operating in the low frequency range but with a wide dynamic range. Dr. James Warwick is the principal investigator, and I am wondering about the possibility of uplink reception using that detector.

Pettengill What is the frequency?

Gulkis It goes up to a maximum of either 20 or 40 MHz.

Soberman I don't know if Dr. Warwick has looked at this, but this is a possibility.

Gulkis For receiving?

Soberman Yes, receiving signals transmitted from the Earth.

Gulkis I will bring that up.

Pettengill You mean for occultation?

Soberman Yes.

Pettengill That is a longer wavelength than could ever be used with natural sources.

Soberman Yes, here we have the possibility of using an Earth-based transmitter.

Pollack It sounds like a very interesting possibility.

Charles Lillie I know Dr. Warwick indicated that if he had a connection from his experiment to the spacecraft's command and control logic he could receive commands if they ever lost lock using the regular spacecraft system. So the sensitivity seems to be there for uplink information.

Gulkis I am on that team, so I will take note of these suggestions.

Pettengill Is there anything else this system could do in the vicinity of Saturn?

Gulkis We don't think so, because it is a fairly omnidirectional antenna over most of the frequency region down to 20 kHz, and the solid angle subtended by the planet is so small it doesn't look as if you can see the emission even from the planet.

Larry Evans Is there any chance of detecting signals from Jupiter with that antenna from Saturn, i.e., using Jupiter as an occulted source?

Pettengill I should think it would be better to use an Earth-based transmitter, at least at those frequencies which penetrate the ionosphere.

Evans Well, there may not be any available Earth transmitter.

Soberman One advantage of transmitting from the Earth, of course, is that you could modulate the signal.

Gulkis I think Jupiter should be visible at Saturn.

Pettengill But not much more visible, in fact probably less so than from the Earth.

Gulkis That's true.

Evans Although it could give you a different observing geometry with respect to the rings.

Pettengill It will give you a different angle with respect to the effects of Io, but that is a Jupiter measurement.

Pollack What Jupiter puts out in any case is fairly variable and unpredictable, so it would be very hard to get a transmission measurement.

Pettengill At any rate, I assume Sam Gulkis and Jim Warwick will look into the possibilities. But I don't see how the use of Jupiter would help in the observation of Saturn.

Lillie Do you think there will be a variation in the galactic background when viewed from close to the rings that could be used?

Pettengill You mean use the hot center of the galaxy? I don't know if the geometry is right.

Gulkis We have not looked at that.

Robert Murphy Could you transmit from one probe to the other as an occultation device?

Eshleman There is nothing inherently wrong with the suggestion, except the spacecraft are not instrumented to do so. The downlink frequency of one must become the uplink frequency of the other to do this.

Pettengill I would like to return now to our basic line of investigation and ask Fred Franklin about the ring mass. I gather that this is an important quantity in understanding the shift of the Cassini division with respect to the unperturbed one-half Mimas resonance.

Can you think of any ways to give us a better handle on this mass?

Fred Franklin I think possibly the most fruitful approach is to fly over the ring plane and observe the perturbation in the trajectory. That would be the most likely way of separating out the effects of oblateness in Saturn from the effect of the mass, say, of ring B.

Eshleman Why is "over" different from "under" here?

Franklin Either way is all right, just as long as you are not always in the ring plane.

I have committed myself to look at some of my calculations a little more carefully. With regard to the ring-mass measurement, I don't know what else you can do with a flyby except get close to the ring plane by skimming above it.

That would be very nice, and I will try to see if I can estimate what ring mass you need to alter the spacecraft's trajectory significantly.

Pettengill Are there any other methods, ground-based or otherwise, that bear on this problem?

Franklin None so far as I am aware.

Eshleman This also reminds me of the radio team meetings, since this is a celestial-mechanics and tracking question. The people on the team, John Anderson in particular, have looked carefully at this question and I think the possibility for success is not bright.

Tyler The limit on the ring mass for detectability is something like 10^{-5} the mass of Saturn.

Pettengill So, there doesn't appear to be any good likelihood of getting a significant improvement in the ring mass from the MJS flight.

Pollack In principle, we can take the indirect road. You might start off by making use of the mean particle size and combining this with the optical depth to give you the mass. Then there is the question of whether it is really a bimodal distribution; here the TV experiment comes in. If we have a superposition of these two different types of measurements, we might be able to give Dr. Franklin something.

Pettengill Was there any consideration with MJS of launching a satellite, just a radio beacon, to go in orbit around Saturn as a means of looking at the mass distribution?

Franklin You still have the problem of separating out the mass distribution of the primary from that of the ring.

Pettengill But if the satellite were in orbit just outside the ring and were inclined, wouldn't that help?

Franklin I suspect so, but I couldn't say with certainty.

Pettengill I realize the problems if you go further out.

Franklin It might require a fairly long time.

Pettengill Anyway, this is probably totally unrealistic from the standpoint of the MJS mission.

Eshleman It is most interesting to hear a lot of these suggestions. I am afraid that those of us who have been involved in MJS for a while feel beaten down. Incidentally, I don't want to discourage the discussion in any way. I am sorry if I gave that impression.

Pettengill I have been through Pioneer-Venus, and I know the problems.

Tyler I think Jim Pollack's comments should be amplified. If you can use radio occultation as a counting device to determine the number of ring particles, and if you know the volume density or the size of the particles, this may open the way to get mass.

Eshleman Would a relative mass distribution be of great interest?

Pollack I think the only concern is whether you have a bimodal distribution. If you do have a bimodal distribution, then TV offers you some hope of seeing it.

Tyler We may even be able to identify that possibility from radio occultation,

using phase perturbation effects associated with the cloud of small particles versus the amplitude effects of the large particles.

William Irvine How does the mass enter?

Pollack If, for example, I know what the mean particle size is, and if I know what the optical depth of the ring is, it is simple to get the mass.

Pettengill You are assuming a density?

Pollack Yes, you have to assume a density for the particles. But the biggest range is 1 to 3, so that is not very uncertain.

Murphy Well, can't we say now with some confidence that the particles are at least a centimeter in size and immediately get a mass estimate?

Pollack If you do a back-of-the-envelope calculation with what we know right now—if you say the particles are several centimeters in size—then the B-ring mass is something like 2 to 2-1/2 orders of magnitude less than what Fred would like. (See contribution by Franklin; a B-ring mass on the order of 6×10^{-6} the mass of Saturn is indicated.) The mass is linearly proportional to mean particle size for a fixed optical depth. If you are willing to make the particles meter size, then you get into Fred's category, or alternately you can have a bimodal distribution to get the mass up.

Soberman What reason do we have to assume that the mean particle size, or even the distribution of particle sizes, would necessarily be the same in all of the rings? And as related to my own experiment, if we measured particles in some fainter ring outside, is this measurement directly relatable to what you may expect in the main rings?

Pollack In the discussion of the radio-source-transmission experiment and in the discussion of Von Eshleman's radio experiment, we were trying to get spatial resolution of the rings to see if there was any variation in density.

As for your experiment, I suspect that it is not going to have very much direct bearing on the mass question. The reason is that the mass goes linearly with particle size and you measure particles that are very small.

Soberman We have a large dynamic size range.

Pollack Yes, but you don't have sensitivity above a millimeter, do you?

Soberman Oh, yes. In fact, if there are any centimeter-sized particles, our effective range increases. If there are meter-sized objects, we could measure them out to 10 km.

Pollack I see. If you really could come close to the ring system itself, you might even be able to see larger objects in the ring plane?

Soberman Yes, precisely. That is one of the purposes. But at several kilometers' distance we would have to assume a velocity to come up with a size estimate from the measured crossing time.

Pettengill Because the parallax disappears?

Soberman Yes, the parallax effectively disappears beyond a kilometer.

Lillie It is a large swept volume for large particles.

Soberman Right.

Murphy My bias is toward looking for kilometer-sized chunks that can be

imaged as we fly by. There is, though, the problem of allowing for the possibility of the classical model, which persists despite all this evidence. It would seem to me useful if the people who are doing scattering calculations would consider in more detail the effects of large particles. In particular, I maintain that, regardless of any refinements in the models I talked about, you are going to have to live with a bolometric Bond albedo of about 0.4, if you have a cloud of small particles, in order to support the infrared brightness temperatures we observed.

Pollack Why that value of albedo?

Murphy To get a temperature high enough. If you have centimeter-sized particles, they are going to radiate from both sides.

Pollack No, that is not true. I am sorry, but that is not true.

Murphy You maintain that you can have particles in equilibrium with one face?

Pollack Yes. Why not?

Murphy At 1 cm?

Pollack Why not? The thermal skin depth could be quite small.

Tyler Why would they stay sufficiently well oriented? And what is the thermal conductivity of these particles?

Pollack Well, it could be, particularly at the surface, very small. I infer from the infrared observations that the particles have a fluffy surface and that would have a rather low thermal conductivity.

Lillie Unless they are spinning rapidly.

Pollack Yes. But they might not be spinning rapidly.

Murphy If for some reason they are not rotating, then can you get one face hot and the other face cold?

Pollack I would have to sit down and do the calculation, but my offhand feeling is yes.

Murphy I still feel we have to explain the temperature variation, although we do need to look into Low's early data point in particular. I think it would be advisable to consider a lower Bond albedo and/or large particles.

Pollack Also, in terms of the Bond albedo, we have no idea what the phase integral is. For that reason we are in rather poor shape to predict a result.

Murphy If you want to push the Bond albedo down to around 0.3, then you can consider the geometric albedos we have been talking about.

Irvine You can't push the Bond albedo down too far. The rings are just too bright.

Murphy What if there is strong backscattering?

Pollack It is a question of what the phase integral is like. If it is 0.5, to take one example, what would the Bond albedo be in that case?

Murphy Well, the data that fit the infrared brightness temperatures best are those that Cook et al. (1973) derived. I think p was 1.1 or thereabouts. Integrating using the Lebofsky et al. (1970) photometry and assuming an Io-like behavior between 1 or 2 μm and a lunar value of q as they suggested, you come up with the proper brightness temperature. So, 0.6 or thereabouts for q is not out of line. But this assumes that you have a particle which is in thermal equilibrium, slowly rotating, and not losing significant heat off the backside.

If you go to small particles, my guess is that you are going to have to run the Bond albedo down to get the intrinsic particle temperatures higher, because you will lose about 15° or 20° with rapid rotation.

Lillie We should know quite a bit about Bond albedos after encounter, but not before.

Pettengill There are several mission-related questions that NASA needs information on. Perhaps we should spend a half-hour discussing these. The first is whether this group endorses the MJS ring investigation, and I think the answer to that is clear. Indeed we do. Then what specific experiments or objectives are most desired?

Pollack I certainly think the TV experiment is very important to see if there are at least a few large objects in the rings. In addition, I think that knowledge of the meteoroid environment of the rings, which you get from imaging some of the Saturn satellites to see what the crater population is like, is also important.

I am interested in meteoroids to determine whether there is sufficient bombardment to fragment a few relatively large objects into a lot of centimeter-sized smaller objects.

Soberman That might be a difficult extrapolation in that it assumes you can separate craters into those which are external and those which are internal to the satellite.

Pollack Well, I think we are very fortunate in the case of the Saturn satellites in that some of them are so small that it would be rather unlikely that they have any endogenic activity. At least the smaller satellites would be particularly useful in this regard. As a double check, you could compare the crater populations on satellites of different sizes.

Torrence Johnson I agree with Jim (Pollack) that it is, in general, interesting to look at the satellites. But some of John Lewis' (1971) models for the interiors of icy satellites would permit endogenic activity involving melting.

Pollack Yes, but doesn't that hold only for Titan-sized objects?

Johnson It holds for 1000-km-sized objects to get full melting. But that is with no input from tidal braking or anything of that sort; in addition, there may be complications if you have significant admixtures of things other than pure H₂O ice. The viscosity would be the same, but the scale for which you just get rid of small craters may create problems. Pure H₂O ice would be stiff enough to retain large-scale features. I don't know what happens to H₂O, ammonia, and methane clathrate.

Gulkis You asked whether or not people were interested in the rings, and of course everybody is. In considering whether or not this group endorses looking at the rings, I think one has to establish some kind of a priority list. How do you compare the ring studies with the other studies that can be done?

Pettengill The question, as I interpret it, is whether MJS appears to be a suitable vehicle for studying the rings.

Gulkis I thought I heard the word "endorsement."

Pettengill You did. But I think endorsement follows the decision that it is a suitable vehicle.

Gulkis I think to answer that question you should compare its capability against the rings relative to its capability for other things. For example, would you want to target to study the rings or to study the satellites?

Pettengill Well, we are studying here primarily the rings. We recognize that MJS has many other objectives. I don't think it is appropriate for us to say that we should exclude anything except those experiments which have to do with the rings, but we should make an attempt to underline the most important of these and to point out the value they may have. This is what I think we are trying to do, and in that sense we endorse studies of the rings. MJS does have important capabilities for studying the rings, and that is what the endorsement in my mind means.

Eshleman There are possible actions, though, that would improve its science value relative to the rings.

Pettengill That is true, and that is what I am trying to bring out. Some of this has already come up.

Perhaps Bob Soberman has some further comments on how the Sisyphus system could play a more important role in determining particle sizes, particularly the presence of larger objects.

Soberman Along with Jim (Pollack), I feel that establishing the meteoroid environment is critical to understanding the rings. I think nailing down the size distribution at the high and low ends is important. We can estimate the distribution from TV pictures of craters, but this approach was notoriously poor in the case of the Moon in giving agreement with the meteoroid distribution.

As far as the ring particles are concerned, we do have a very large dynamic range in the particle size that can be measured. My concern is that if one sees a particle 1 to 10 cm in size outside the ring structure, is this characteristic of the particles in the main rings? I guess the answer is that if one does not see any variation across the rings, then one has confidence that the occasional particles seen outside are characteristic of the size distribution in the rings.

Pollack It seems to me that in the case of both Bob Soberman's experiment and the TV experiment, the most critical requirement is to get the spacecraft as close to the rings as it is safe to do. Thus, we come back to the whole question of the D' ring.

So I guess the answer is to do everything possible to determine the hazard. If no hazard exists, then just go in and get real close to the rings.

Eshleman Of course, the project is asking this group just that question.

Lyle Broadfoot We are going to go as close to the rings as we can. The real question is how close is safe. I think we should identify what can be done to improve our knowledge of the particle distribution so that we can reduce the passage distance without compromising the mission.

Soberman Chuck (Lillie), if your numbers are indeed realistic for observations from fairly large distances, how early could you pin down whether or not the D' ring exists, and would there then be time to retarget the spacecraft closer to the planet?

Lillie There probably would not be enough time, but we could look at the problem. There is the scattering of light from Saturn as seen by the spacecraft en route, and the question really is, "What is the off-axis light rejection of the baffle system we have on our telescope?" We will look at the limitations, but I suspect that it is going to be less than 60 days before encounter before we will be able to say for certain what the density of the D' ring is.

Pollack Can they make a midcourse correction that late?

Thomas Burke I think they can make one as late as 4 days before encounter, but I think a big change in the distance of closest approach is very difficult.

John Niehoff There is a more important point here: if you change the closest approach at that late date, you are going to impact a lot of sequences that are already preplanned in the mission profile, and you will need more time to do an effective job than might be apparent. Satellite encounters, for example, are immediately affected by retargeting.

Pollack Of course, one of the safest courses of action is to retarget only the second spacecraft.

Niehoff There is another possibility, which is the next point Dr. Pettengill will get to: that the targeting of Pioneer 11 after Jupiter gives another possibility for covering your bets on the D' ring. Pioneer 11 will get to Saturn a year earlier than the first MJS mission, and it's conceivable that one could commit it to a "Kamikaze-type" passage to the edge of the A ring, or just outside, to determine what the depth of the D' ring is.

Pollack Presumably, you also have instruments that could detect the D' ring without having to go into a Kamikaze passage.

Niehoff Certainly.

Pollack It sounds as though you would want to follow that strategy.

Niehoff NASA is looking to this workshop as an assembly of people very interested in the Saturn's rings problem who will raise their collective voice for particular objectives as they bear on MJS mission planning. Therefore, it is important for the group to decide on the several most important Saturn's rings measurements to be made on MJS. Because there will have to be compromises between those experiments and all the other ones that are vying for control of trajectories, we must also order them to give guidance as to their relative importance.

Pettengill I think we are building up to a point where we can probably do that.

Let me just ask whether Pioneer 10 has a micrometeoroid detector.

Soberman It has two—mine and Bill Kinnard's.

Pettengill Won't these work out to the radius of Saturn?

Soberman Unfortunately, in the case of my experiment, no. If the Jovian radiation field is as high as the models predict, we will be failing at about $6 R_J$ on the way in due to irradiation of the electronic components.

Pettengill That is true for both flights 10 and 11?

Soberman Both F and G, yes. The penetration detector, by all estimates now, will have some appreciable number of cells left on it and should work on out to Saturn.

Pettengill Would this give any information of the sort that Jim (Pollack) was asking about?

Pollack Yes, it sounds to me as though it would. Also, the photopolarimeter would be very good for trying to see any faint rings.

Pettengill This leads naturally into the second question: how do you feel about targeting Pioneer 11 toward Saturn, after Jupiter, for investigation of the rings, with the particular objective of penetrating the D' ring? Can anyone say what systems will still be operating after it goes by Jupiter?

Soberman You would have to do some power sharing on Pioneer because you have a continual degradation in power.

Pettengill Why?

Soberman The RTG's run down. But, presumably, the penetration detector would be working and also the photopolarimeter. Another problem, as I recall, is that in order to retarget to Saturn, Pioneer 11 would have to go even closer to Jupiter than 3 planetary radii. We will find out in December whether Pioneer 10 can survive at 3 R_J .

Pettengill Three R_J is its approach distance?

Soberman That is the target distance.

Pettengill And where does the current trajectory take Pioneer 10 after Jupiter?

Soberman It goes on to escape.

Niehoff In the general direction of the solar apex. The present planning at NASA Headquarters prefers not to target Pioneer 11 for Saturn because of the radiation problem, and it would take a fairly strong voice to change that position.

Pettengill How far out would it pass with present plans?

Niehoff Well, it hasn't been decided precisely where it will go, but one possibility is to repeat the Pioneer 10 flyby distance or to move it even further out if Pioneer 10 gets fried.

Pettengill So it is 3 R_J or more depending on the results from Pioneer 10.

Now, to go to Saturn requires what distance?

Niehoff Somewhere between 2 $\frac{1}{2}$ and 3 R_J on the other side of the planet, I believe.

Soberman It would come in retrograde.

Pettengill So, perhaps we should recommend that if Pioneer 10 appears to survive its 3 R_J passage, this should be considered a safe distance and Pioneer 11 should be retargeted to Saturn.

Pollack Bob (Soberman), I missed a point. Is the imaging photopolarimeter going to survive passage by Jupiter?

Soberman If the rest of the spacecraft does. My experiment has to be the most vulnerable.

Niehoff Are there channeltrons in the photopolarimeter?

Soberman Yes, there are.

Niehoff They probably won't make it, then.

Pollack I am trying to find out if any instrument that measures brightness will survive.

Soberman Well, the only instrument is the photopolarimeter.

Pollack And John (Niehoff) says he doesn't think it will make it.

Niehoff Channeltrons are thought to get wiped out with the present estimate of the radiation environment for Pioneer.

Pollack At what distance?

Niehoff Three R_J . It is a 50-50 thing right now. It depends on whom you talk to because the models are so uncertain. That is the whole reason for Pioneer 10's targeting, but a change in Pioneer 11 is not something that you could plan on today. December 4, 1973, will tell us the answer.

Pettengill I would like to suggest as an answer that we recommend targeting Pioneer 11 to Saturn after Jupiter if the circumstances permit it. I would interpret favorable circumstances as being Pioneer 10's survival.¹

If they do retarget to Saturn, targeting through the D' ring is the next step, because even if the observing instrumentation dies it would still give us an understanding of the hazards of the D' ring.

Niehoff The objective there, of course, is to further improve the MJS mission.

Pettengill That's right. Although I am still worried that if Pioneer 11 does survive the D' ring it will be just by luck.

What spacecraft missions beyond MJS have been already considered which would improve our knowledge of Saturn's rings? What general class of experiments appear to make sense if one considers the post-MJS period? In other words, would we consider Saturn orbiters or probe-type experiments into the ring?

Niehoff One suggestion is Fred Franklin's objective to go skimming across the top of the ring plane after you have a better definition of its dimensions.

Pettengill That is one possibility. Are there others?

Franklin Am I right in thinking that there is a magnetometer on the MJS mission?

Soberman There is.

Franklin On both of them?

Niehoff Yes, and the Pioneers too.

Franklin It seems to me possible that Saturn may have a magnetic field but no charged particles because the rings act as a screen to absorb them.

Eshleman Then where the charged particles give out would tell you the location of the D' ring.

Pettengill What about probe measurements; that is, going into the rings with probes? I gather that going into Titan with a probe has already been discussed.

Franklin I have an impression one will learn more about rings A and B in the first mission than about the inner ring system, which may also be very interesting. I would expect the inner region to have a tremendously different average particle size. Maybe in later missions one would like to look into the internal structure—the region between the inner ring and the planet.

Pettengill What about an orbit where the periapsis passes through the inner ring?

¹ *Editor's note:* Pioneer 10 survived, and Pioneer 11 has been retargeted to Saturn after Jupiter passage.

Eshleman Inside the inner ring?

Pettengill Yes, that is what I mean.

Pollack The problem is that I am not sure how definitely you can say there are absolutely no particles between the inner edge of ring C and the atmosphere of Saturn.

Franklin There probably are particles there.

Pettengill There probably are, but that is what you want to find out.

Pollack Yes, but you might find out in a rather catastrophic fashion.

Pettengill A probe is so catastrophic that it dies. Why is this any more than a probe that might survive?

Niehoff You could do this with a subsatellite if you put yourself in an orbit initially outside of the rings and then, at the apoapsis of the orbit, drop a subsatellite into a much lower periapsis orbit.

Pollack I see. In other words, you would be willing to sacrifice it?

Pettengill Well, if you consider it a probe, yes. It is a probe that might survive.

Irvine Isn't it likely that we would consider probes entering Saturn's atmosphere as more valuable?

Eshleman But by the same token this one that passes inside could get low enough to do some exospheric sampling.

Pettengill Perhaps now, having identified the various experiments which MJS could do, we could assign them a priority. I think we have already listened to and discussed the experiments.

Murphy Would it be helpful to list them or restate them?

Pettengill We have the occultation observations which bear primarily on the particle-size distribution, is that correct?

Eshleman Size, thickness of the rings, packing fraction, or volume density fraction.

Pettengill These will be two-frequency observations.

Pollack We certainly want to put a high rating on these, and maybe we want to encourage the project in making bistatic radar observations as well.

Pettengill What other aspects of the radio observations are important? How about polarization?

Eshleman For this particular experiment you would probably use circular from the spacecraft, with dual linear received on the Earth. We would like to make the receiver work as a radiometer as well.

Pettengill Are any other aspects of radio science in the context of MJS?

Eshleman Occultation remains the main objective, along with the trajectory to optimize it.

Tyler It also optimizes the celestial mechanics experiments.

Pettengill What about the infrared experiment?

Burke We have the possibility of infrared spectral identification of particulate material, primarily water ice and silicates. There would be the radial and circumferential homogeneity of those, and then there is a possibility, depending on particle size, of thermal inertia measurements.

Pettengill There is the use of the television imaging to detect larger identifiable particles within the ring structure.

Pollack That's right and also, by studying craters on the smaller satellites of Saturn, to infer the meteoroid environment.

Soberman And I think that Sisyphus will also help determine the meteoroid environment near Saturn.

Irvine Don't forget the photopolarimeter.

Pettengill I would guess that there will be more new information on the structure of the rings obtained from the occultation and/or radar observations from two nearby frequencies than obtained from the photopolarimeter.

Irvine It depends a bit on the ground-based experiments done between now and the spacecraft's arrival.

Michael Janssen There is a lot of improvement to be made in the radio work by 1980. It is hard to say what new information would be added by then.

Pettengill Using infrared techniques, how much new information applicable to spectral identification will we be likely to get? And remember, we will have higher spatial resolution using spacecraft observation.

At a typical close approach, how does the spatial resolution corresponding to the spacecraft compare with ground-based observations?

Murphy The spacecraft's resolution is equivalent to half a second of arc as seen from the ground, and that means you can see the Cassini division fairly easily.

Pettengill That is a tough resolution to get in the infrared from the ground, isn't it?

Murphy You will never get it from the ground.

Pettengill How much will you miss it by?

Murphy Well, for compositional information you use the 1.4- μm region from the Earth, and there it is a question of signal levels. I don't know how much compositional information you can get at wavelengths between 10 and 250 μm . There would be some silicate lines there.

Burke There are silicates there. And there are also some water-ice lines in that region.

Murphy With the 200 in. at 30 μm you would have 2 seconds of arc spatial resolution, and you don't have the spectral resolution.

Pettengill Well, there is no doubt that these measurements might be highly useful, but isn't it a little hard to say that they will overturn many theories?

Irvine The thermal inertia measurement seen through the eclipse may do so.

Murphy That would seem to be the most important.

Burke I don't think so. That measurement should be done with a radiometer.

Pettengill You think thermal inertia is the most important aspect of that experiment as applied to the rings?

Murphy I think it is the most important thing to be learned.

Pettengill Do you agree with that?

Burke Yes, I think that sounds reasonable.

David Morrison I agree that a thermal inertia experiment would be most interesting.

Am I correct that there is some question about sensitivity levels? One is dealing with temperatures that will drop substantially below 100°. If there is any question now as to the sensitivity of detectors on that experiment, almost certainly the cooler portion of the rings will drop below the sensitivity threshold.

Murphy This basic mission could be performed by a simple radiometer, which might mean less weight and less telemetry.

Pollack The basic payload selection has already been made for MJS.

Pettengill But regardless of the instrument chosen, if you all agree, let's stress the importance of this measurement over the others.

Burke You are saying that the thermal inertia measurement can be made with a radiometer, but I am saying that the infrared objectives in total cannot be met by a radiometer.

Murphy But the main infrared objective for the rings could be met by a radiometer.

Pettengill Now, I am faced with the job of trying to rank these various experiments one with another. We must also discuss the importance of the bistatic and dual polarization radio capabilities, since they are probably not going to be included at all unless we stress them.

Pollack I think we have to give an honest answer here that these would give useful information, although the bistatic and polarization capabilities aren't as crucial as some other things are.

Pettengill For instance?

Pollack Well, the transmission measurements.

Pettengill You mean the occultation experiments?

Pollack Yes. I think we can say that the bistatic and dual-polarization experiments would be useful, and we encourage them, but you can't put great big flags all over them.

Pettengill Well, would you allow me to give the two-frequency occultation observations, more or less as they are now planned and described by Von (Eshleman), the first priority?

Pollack I think I personally would.

Pettengill The reason is that, while we hope to do natural radio-source occultations, these are not coherent and they are at a considerable distance behind the ring structure. This great distance changes the Fresnel zone from a rather small value to something that is hopelessly large from the standpoint of trying to determine particle size.

Pollack I am sorry, I guess I didn't understand your remark on particle size because that to me is the key thing.

Pettengill The size of the Fresnel zone for a source at infinity is set entirely by the planet-Earth distance. This increases the Fresnel zone as compared to that for the spacecraft to a size where we will never have a particle that comes close to filling it, so we won't have deep modulation of the signal in the sense Von (Eshleman) was describing. In addition, he has the coherence of the transmitted

signal which is vital for seeing the effects of any atmospheric structure that is present.

I think for these reasons I would stress the occultation as probably of highest interest because of its potential information on particle size and distribution. Beyond that, the particular choice of occultation trajectory is important in optimizing the data return from the rings.

Pollack Well, I think one would like to get as close to the rings as is safe.

Pettengill Well, that isn't the only aspect. You want a good occultation as well.

Pollack Yes, that is true.

Paul Penzo Those two objectives are somewhat incompatible.

Pollack I agree that the first priority on the choice of trajectory has to go with occultation. And then, after that, the second priority is getting as close as possible.

Pettengill Are there any other trajectory constraints which are relevant?

Murphy We should be prepared to say we have to get to such and such a distance in order to distinguish a 10-km-thick ring from, say, a 5-km-thick ring.

Pettengill How close do we have to get for anything useful?

Pollack I suspect that at any distance you will get something useful. I think the effect of distance is to introduce a gradual qualitative degradation.

Pettengill There is no hope of getting ring mass information?

Franklin I think it is very, very small. I would ask, though, what do you think of a sunlit versus a darkside passage of the rings?

Pettengill Do we have a preference?

Penzo For the best radio occultation, you have to pass on the sunlit side of the rings.

Pettengill Does everybody feel they can live with this?

Irvine I wonder; the other way you may get some information on the particle size that you can't get from the occultation, from photopolarimetry of small particles in the D and D' rings.

Franklin But you also want a good radial brightness profile of the ring. I would lean towards that before I would the darkside passage, although I would like both. Maybe you can get both.

Pettengill Which would you prefer if you had to choose?

Franklin I would say the sunlit side.

Pettengill How about the photopolarimeter?

Penzo I think their primary interest is in seeing stars through the shadowed portion of the rings, and going over the rings you can still look down at Saturn's shadow on the rings.

Pettengill So you would like to fly by the sunlit side and also over the shadowed area, if possible.

Penzo Yes.

Pettengill We have identified the thermal inertia measurement as important. Is there a need to mention optimization of the Sisyphus experiment, or is that covered by close passage to the rings?

Soberman Well, we would like to cross the ring plane in sunlight or come close to the ring plane in sunlight.

Pettengill That sounds in conflict with the desire to fly over the rings on the sunlit side and over the shadow, doesn't it?

Penzo It makes it more difficult.

Pettengill So, would you place that priority lower than the trajectory optimization for the other experiments?

Soberman We have placed this as one of our first priorities. Remember, there are two spacecraft.

Pollack They are going to launch two.

Soberman And there is maybe a 50-percent probability of arriving at Saturn with two operational spacecraft.

Franklin I am sorry, would you say it once again, Gordon (Pettengill)?

Pettengill We have a choice to make: a trajectory that flies over the plane of the rings on the sunlit side, passes over the shadow of the planet on the rings, and then does whatever the constraints require at that point; or one which attempts to pass through the ring plane in sunlight.

I suppose it is possible to fly over the rings on the sunlit side, down through the plane in sunlight and then pass into the shadow and look up. So, as I say, these requirements aren't necessarily in conflict, but they may very well turn out to be.

Murphy If you look at the shadow from the underside, you might have difficulty interpreting thermal inertia measurements with a multilayered ring.

Pettengill Do you have to see both sides of the ring to make the thermal inertia study?

Murphy What I am saying is that I think you would want to look at the hot particles and see what happens to them. If the optical depth is unity, the particles at the bottom are not going to be cooling at the same rate.

Pettengill Well, tentatively, I will assume that we will target the two spacecraft to the separate possible trajectories, so there isn't a conflict.

Now that leaves us with the TV particle imaging and the subsidiary elements of the radio science, such as the bistatic radar, circular polarization, and radiometric capabilities. We could put the thermal inertia measurements down as the third priority.

Irvine I think you would rate the improved radio capabilities ahead of the TV imaging.

Pollack It is such an apple-orange situation.

Pettengill I know. It always is on these things. But I think there is the point to be made that if the improved radio capability isn't emphasized it won't be carried, whereas the TV imaging most certainly will be.

Burke I think there is one thing that the group might address regarding the TV imaging and that is perhaps to recommend the 1½-m optical system as opposed to the 50-cm, because there is a significant difference in capability.

Pollack Yes, that is certainly a good point to make.

Pettengill I hear you, but I am just not sure what the implications of that would be. They might be such as to wipe out a third of the payload, and I am sure we don't want to do that.

Burke It is my understanding there are no significant weight penalties, and I don't think there is any power penalty. It just costs more.

Franklin I talked to Brad Smith about this, and it seems to be simply that this is not an item NASA usually uses; it is something special. There is a cost but not a weight problem.

Pettengill How would you play that off against the bistatic radar capability and the dual-polarization capability?

Irvine The big bonus for the TV is probably not so much on the rings.

Pettengill That's the point. I can't conceive of a factor of three making that important a difference in the results that we get from the optical imaging for the rings, so I would prefer to stress the radio.

Penzo I want to revise one of the arguments. For the best radio occultation, you have to pass above the rings. However, for the best shadow for the photopolarimeter you have to go under and not so close to the rings. The photopolarimeter's objectives are more consistent with passing through the ring plane before reaching Saturn, which means passing underneath the rings.

Pettengill I am tentatively putting a priority of three on the thermal inertia observations because of the information it can give us on the surface properties, particularly if it is combined with hard information on the size.

Penzo And that is best done from the sunlit side.

Pettengill Right.

I think we have all decided that the TV imaging of the particles, while important, can be moved down in priority. So it is really a decision between photopolarimetry and the advanced aspects of the radio experiment, such as the bistatic radar and the dual-polarization observations.

Broadfoot I think photopolarimetry is very much more flexible. The instrument is mounted on a scan platform and can look at the rings from all directions. I am not convinced that the trajectory will make that much difference to the photopolarimetry because of its flexibility.

Franklin Certainly it is true that radio provides the least ambiguous and most easily interpreted information.

Pettengill Probably because we haven't had much before for Saturn's rings.

Irvine But photopolarimetry may be the only way to get information on the D and D' rings.

Pettengill As I understand it, unless you can move the antenna, getting the required geometry is almost impossible. That is why they are fighting at the moment for an articulated antenna. They already have articulation on the photopolarimeter, and, because of that flexibility, they are less dependent on the trajectory that is chosen.

Pollack Excuse me, let me try to understand. Is it true that in the case of the radio observations you need to move the radio dish to handle the intermediate

case where the particle size is less than the size of the antenna?

Pettengill Yes, data on the different size domains are obtained from studying different aspects of the returned signal.

Pollack But for the size range of greatest interest, say centimeter to meter size, I believe they have to deflect the radio antenna, but not very much.

Irvine The question is, how much does the ray path have to be moved to get out of the diffraction aureole?

Pettengill I call that a bistatic mode. Once you have a bending in the ray path due to the radio energy striking something, that becomes a bistatic measurement.

Pollack Oh, I see. Then you are right.

Pettengill Of course, occultation and bistatic observation blend into each other. I don't mean to say I can put a neat label on each one, but the moment you bend the antenna and look for the energy returned by a different route, you are talking about a bistatic radar observation. And that requires antenna motion, so we are right where we were to start with.

Pollack That is the reason I raised it now. I think for Von Eshleman's group to be able to get the information for the size range of greatest interest at the present time he has to be able to move the antenna.

Pettengill That is their bistatic experiment and I am now asking what priority you place on it.

Pollack You have to rate it awfully high.

Pettengill We better back up, then. It is now down around number four. Would you rather have that than the thermal inertia?

Pollack Yes, because otherwise they can't interpret their experimental results. They can't do the complete experiment.

Pettengill But what happens to the thermal inertia if radio science is given precedence?

Murphy The thermal inertia measurement strikes me as very fundamental. What is the consequence of giving number three priority to the bistatic radar? That sounds like a question of money as opposed to the thermal inertia measurement, which is a question of orbit. So they don't really interact.

Pettengill We have already given priority to the orbit.

Murphy Yes. So I think thermal inertia is taken care of in that decision. If we were to lose the thermal inertia possibility to get bistatic radar, then we would have a problem.

Pettengill Well, what I will do then is put third priority on the bistatic radar.

Pollack Right, and by "bistatic" it is important to note that we imply being able to direct the antenna.

Gulkis You will have to move the spacecraft since they are not putting on an articulated antenna; isn't that right?

Penzo Yes. In fact, they will be moving the spacecraft to direct the antenna during occultation by Jupiter.

Pettengill Do they store the telemetry?

Penzo No. The signal is received by the Earth during the occultation by Jupiter.

The spacecraft itself does a roll-and-yaw maneuver in order to maintain the antenna pointing toward the correct portion of the limb of Jupiter.

Pettengill Yes, that's fine for an occultation, but there is a different problem involved in a bistatic observation. When you occult, you know you are going to lose telemetry. When you deflect the antenna for a bistatic observation, there is a strong possibility that you might be losing a considerable amount of signal otherwise available, possibly containing vital telemetry information. Are you willing to throw that data away by moving the whole spacecraft to get some bistatic radar information—a move that might compromise the telemetry?

Broadfoot I think we will be on a tape recorder during that period anyway.

Pettengill If that is true, I am sure the project will be happier about moving the spacecraft. It also makes me feel better about third priority for the bistatic radar, but I wouldn't put priority four automatically on the circular polarization. Maybe we should come back to the thermal inertia measurement and rank it fourth.

Now, we still have the TV imaging and the circular transmitted polarization questions. I think I would put five on the circular polarization and six on the TV imaging. The radiometry will come along for the ride, won't it, Sam (Gulkis)?

Gulkis Von Eshleman doesn't think that it will.

Pettengill But it is such a trivial modification.

Gulkis It might mean installing a Dicke switch, which Von (Eshleman) says they don't have money for. There are certain other modifications that would have to be made, and if the need is not emphasized it won't get put in.

Pollack That is the whole joy of space missions, there are just millions of times things like this come up.

Pettengill What happened to the photopolarimeter in this ranking? It doesn't have a number yet.

Broadfoot I haven't heard two for awhile. What was that given to?

Pettengill Two was the trajectory optimization.

Burke It seems to me there are two categories of things here, one category is a number of trajectory-related items, and the other consists of hardware-related items.

Pettengill Yes. But actually when we choose the mission that we feel is important, we really choose not only the hardware but also the trajectory. I think we should drop the trajectory ranking altogether, because that follows from the choice of experiment.

Murphy But that summarizes a lot of requirements in one fell swoop.

Pettengill First you say that I have to provide for occultation, and then you say I have to have a trajectory that gives it; that is not telling me anything new.

Murphy But the trajectory specification also provides requirements that affect the thermal inertia measurement.

Pettengill We have thermal inertia currently at four. If I move it up to three and move three to two, it says pretty much the same thing, doesn't it?

Irvine Would you put the photopolarimeter in four?

Pettengill No, I have the circular polarization there; maybe it shouldn't be there, but that is where I have it.

Irvine It is another case of having to modify the instrumentation. The question is not so much astrophysics as astropolitics.

Pettengill It sometimes seems that way.

Burke It would seem to me that you should rank the trajectory-related questions separately and then address the hardware-related questions.

Pettengill I think that is not completely possible. I would put the photopolarimetry ahead of the circular polarization and the TV imaging of the rings last. We also placed Sisyphus last.

Irvine Could you make it "less last" than the TV?

Pettengill All right, seven for Sisyphus and eight for TV.

Burke Could you run through the list again, please?

Pettengill Yes.

- (1) Radio occultation at two frequencies, with the objective of yielding particle size and spatial density.
- (2) The incorporation of bistatic radar observations, which implies a capability to move the direction of the antenna either on a separate mount or by moving the spacecraft. These observations will fill in some otherwise undefined particle size domains.
- (3) The measurement of ring-particle thermal inertia, using the onboard infrared systems.
- (4) The photopolarimeter, noting that there may be a conflict in the choice of trajectory that optimizes this as compared to the occultation requirements.
- (5) The incorporation of circular transmitted polarization and, of course, dual-linear Earth-based reception, to help interpret the bistatic radar observations as well as to permit Faraday rotation measurements.
- (6) The Sisyphus particle detector used as a ring particle analyzer.
- (7) The use of the TV imaging system to detect larger particles within the otherwise homogeneous ring structure.

Pollack And also to tell us about the meteoroid environment.

Irvine What about the radiometry capability?

Pettengill That is now number eight. And that is it, as far as my list goes. Have I missed anything?

Irvine Would we want to put the radiometry ahead of the TV?

Pettengill No, I wouldn't.

Irvine Do we want to say anything about the infrared spectrometer?

Pettengill Well, that is assumed in measuring the thermal inertia, as I understand it, although I agree there is more to infrared than that. But I have a feeling that infrared measurements will be fairly compatible with whatever trajectory is chosen, and the system has to be aboard for Titan and other measurements anyway.

Burke The sorts of trajectories that are good for thermal inertia measurements will also be good for spectral identification of components.

One thing that has been mentioned earlier today is the possible use of the planetary radiometry system as a receiver of Earth-based transmissions.

Gulkis That has to be studied, but it should be very far down on the list.

Soberman But I think it certainly should be studied.

Gulkis Whatever can be done will be done.

Soberman I think there is something else—the question of the Lyman-alpha ultraviolet measurements—which I think you may want to add to the list.

Pettengill Are they vital?

Soberman Well, specifically, I am thinking of Jacques Blamont's instrument.

It is not currently on the spacecraft, but he has been placed next in line, and I think that NASA Headquarters may be looking to this group for an assessment of his experiment.

Broadfoot Did he have an opportunity to explain just what he wanted to do with his experiment?

Soberman He spoke about possible hydrogen atmospheres.

Broadfoot He could do some of the same things as the occultation experiments, only with a field of view of a few seconds of arc. That gives him a lot better spatial resolution than we get with some of the other instruments, as far as the rings are concerned. It also gives him a particle-size detection capability.

Pettengill Refresh my memory. What sort of ultraviolet instrumentation is already onboard MJS?

Broadfoot There is an ultraviolet spectrometer with spectral coverage from 4 Å to 1600 Å for looking at planetary occultations of the Sun.

Pettengill Does anyone feel violent disagreement with the list as it stands? I am going to recommend it as our official list.

Burke May I make one statement, please? It turns out from a trajectory point of view that the best radio occultation trajectories occur very late in the mission window; in fact, they come after the end of the nominal mission. There are apparently cost guidelines which suggest that the mission should terminate in July, whereas the best radio occultation measurements would come in September of 1981.

Pollack What does "best occultation" mean?

Burke I think it relates the orientation of the trajectory to the major axis.

Penzo It is connected with the openness of the rings. There is an oscillation where this parameter stays around 5° or 6° during most of the mission, and then, near the end, it starts increasing radically.

Pettengill I think by giving occultation first priority, we have said as much as we can.

Pollack I think we have to be realistic anyway, because there are certain modifications we could recommend which have no hope of being implemented.

Pettengill I suggest we officially adjourn.

(Whereupon at 5:22 p.m. proceedings in the above matter were adjourned.)

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