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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.

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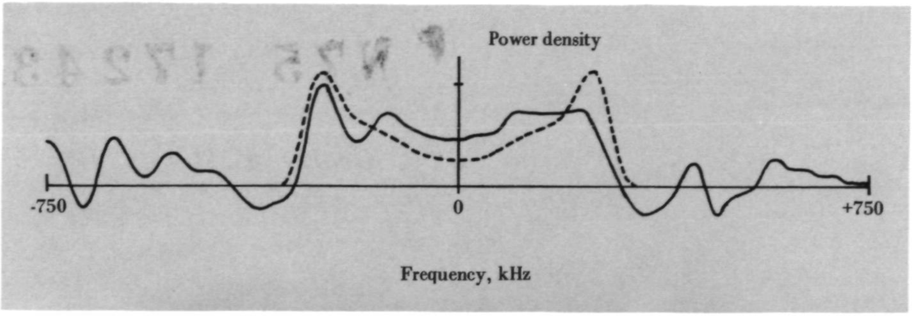

FIGURE 1.—*Radar spectogram of rings of Saturn, with received power density plotted against doppler frequency shift. The theoretical curve (dashed) is for a two-ring system, with the particles closely packed in the inner ring and the outer ring 50-percent filled. For this model, the radar cross section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

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[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 serveral 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 5 σ event and it was not at zero doppler; it was offset by 1 kHz or so.

Pettengill I remember something more like 8 σ.

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 explained 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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