GENERAL DISCUSSION

Woltjer: Dr. Field raised the issue of the discrepancy between some estimates of the amount of graphite. I wonder if any of the authors have a comment on this point?

Balick: I have the easy way out. I simply reported the number given in the Telesco and Harper paper of last year.

Bonilha: They used results from previous works too, which are proper for silicates, but not for graphite. The emissivity for amorphous carbon is much higher than graphite, so you need less dust to explain the infrared.

Field: In thermal equilibrium we won't get amorphous carbon. Thermodynamic calculations show that you will get graphite.

Mathis: It seems to me that the best mass estimate is by Nino Panagia. It was Panagia who suggested the particles have to be about 1 μ. He fit the far infrared flux and showed that the particles have to be that large to be cold enough. That leads to the mass because of all of the mass is in the large particles. He gets about 1% of the mass of the gas in the particles which is simply set by the temperature required by the far infrared spectrum.

Forrest: My spectrum fits an emissivity like $1/\lambda^2$ which is what you might expect for small graphite grains. I assumed then that the opacity was due to small spherical graphite grains; that gave me a mass. Panagia has 1 μ grains that reduce the mass by a factor of 10.

Woolf: Concerning the question of whether the silicates in the outer shell are outside, even from the first spectrum of NGC 7027 it was clear that if you tried counting out all the odd features, there was a curious depression at 9.7 μ that looked like an imperfect silicate feature.

Forrest: How do you explain the 20 μ radiation?

Woolf: You can do that by keeping the temperature in the 90°K region.

Panagia: If there is any absorption by silicates at 10 μ, then that dust cannot be responsible for the emission. The emission requires cold dust. This cold dust is almost entirely within the HII region, so whatever the material is, we need big grains. Big grains will not show any feature at 10 μ. So if there is any silicate, this must be outside the most important part of the emission in NGC 7027. I don't know whether it exists or not. I don't know whether it is interstellar or not, but it cannot be the dust that is responsible for the emission.

Miller: There were a series of papers claiming a correlation between spectroscopic properties of planetary nebulae, their physical appearance, and their kinematical distribution in the galaxy. I couldn't agree with the morphological classification, but then Cudworth found that there was no question that there was a kinematical difference between

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the various spectroscopic categories. This may be telling us something about a galactic feature of the formation of planetary nebulae. I think it is something that should not be ignored, even though it sounds so crazy.

Osterbrock: It's a very strange thing, a morphological scheme depending on symmetries. I discarded it; now I'll have to pick it up again.

Kaler: To me, the kinematical conclusions look rather real and it is rather hard to find any real hard criticisms. This should really be looked into more carefully now.

I would like to shift the discussion to the subject of evolution. Would anyone like to comment on the evolutionary tracks that have been discussed, and whether the planetary nuclei with cool central stars ($\sim$30000°K) really do evolve into planetaries with hot central stars ($\gtrsim$60000°K) or whether they are really two different types of objects?

Salpeter: In review, there is no doubt theoretically that the central stars must increase in temperature, I thought the only two real controversies were (a) how long does it take and (b) what happens to the total bolometric luminosity. Theorists disagree on that, but not on whether the central star will heat up.

Woltjer: Would you say that in the details it is primarily the physics which is uncertain or that there are computational problems?

Salpeter: It is a mixture of both. Computations are still very complicated, and everybody makes a short cut in the initial conditions. We have no idea how important the initial conditions are for later calculations.

Woltjer: Is there anybody who wishes to raise another topic? It's the last chance.

Aller: I want to put in one final word of encouragement rather than a final topic. Dr. Osterbrock recalled how things were 10 years ago; I would like to comment very briefly on how things were 40 years ago. At that time the subject of planetary nebulae was exciting as it is now but for a different reason. Before the work of the giants in the field, and I refer here to Zanstra, Bowen, Menzel, and to a man whose name for some reason hasn't been mentioned, Ambartsumyan, laid the groundwork for our modern excursion into the field, we had the "holy writ", Volume 13 of the Lick Observatory Publications. I'm glad to note that the Lick Observatory is still at the forefront and in there pitchin', but also that a tremendous amount of effort and expertise is now being devoted to this problem. In those days when I went as a graduate student to Harvard to work for Menzel, it seemed that planetary nebulae were going to be simple. The second thing was that they were going to be interesting. The first idea, as we have seen, was totally wrong. But I think we would all agree that the second is totally right.