GENERAL DISCUSSION – SIXTH SESSION

Minkowski: The absence of small planetaries from the size distribution might not be more than a selection effect if planetaries in an early stage of their evolution, where they are small, are not recognized as planetary nebulae. There is, as a matter of fact, a group of objects that have been classified as Bep by Merrill with a low Balmer gradient, but no, or in a few cases, very faint, forbidden lines. One of these objects was found to be a nebula of about 3″ diameter. It seems quite possible that these objects are not stars, but show an early phase of planetary nebulae with small diameters and, of course, very high densities.

Westerlund: Miss Webster found in the study of unresolved or nearly unresolved planetary nebulae in Henize's and my catalogue, several low-excitation objects which must have very small (radii 0·04 parsec) and small hydrogen mass. They represent most likely the youngest evolutionary stage so far observed for planetaries. Their spectra contain forbidden lines of [O II], [N II] and [S II], but not as strong as would be expected. No [O III] lines have been seen.

Abell: I am inclined to agree with the evolutionary sequence described by O'Dell, but am bothered about one circumstance. Some or all of the large faint nebulae studied by O'Dell and me are probably optically thin. Thus, the temperatures we derive for the stars are lower limits. Even these lower limits, however, show that the bulk of the stellar energy lies shortward of the Lyman limit. Thus, our computed lower limits to the stellar luminosities are too small by roughly the fourth power of the factor by which our temperatures are too small. Now since the energy required to keep a given spherical mass of gas ionized is proportional to the reciprocal of the volume of that gas mass, our underestimate of the stellar luminosities would go as the inverse cube of the nebular diameters, if all nebulae had the same mass and all of their stars the same luminosities. In fact, a plot of $\log L$ vs. $\log D_{\mathrm{neb}}$ for O'Dell's and my objects exhibits exactly this relation expected, due entirely to the nebulae enlarging and becoming more and more optically thin. We should make certain therefore, that the apparent evolution of the central stars is not partly due to our method of determining the properties of those stars. In fact, the central stars may not drop much in luminosity during the relatively brief times during which the nebulae can be recognized.

Osterbrock: Many theoretical ideas seem to be converging on long-period variables as the progenitors of planetary nebulae. I wonder what statements can be made about the galactic distribution of long-period variables with respect to the galactic distribution of planetaries; also, with respect to the relative numbers of long-period variables and of planetaries.

Osterbrock and O'Dell (eds.), Planetary Nebulae, 432–438. © *I.A.U.*

Feast: The distribution and kinematics of the Mira variables are remarkably similar to that of the planetaries, so that a connection between these two classes of objects would seem quite possible on these grounds – though, of course, it does not prove a connection. The Mira variables are much more frequent in the galaxy than the planetary nebulae so that the planetary phase would have to be much shorter than the Mira phase.

Westerlund: The emission-line spectra of symbiotic stars are of high excitation. Is it possible to explain how the low-excitation young planetary nebulae can follow in evolution immediately after the symbiotic stars?

O'Dell: The dilution is so much less in the symbiotic stars that this might account for the higher degree of ionization.

Boyarchuk: The search for related objects is very important for understanding of the evolution of planetary nebulae and their central stars. I think that the symbiotic stars are such objects. The nature of symbiotic stars is not understood completely thus far. However, one can explain most of the existing observational evidence on a supposition of their being binary stars.

According to this hypothesis symbiotic stars consist of a cool giant and a hot dwarf, both enveloped by a rather small dense gaseous nebula.

Investigations of continuous spectra of symbiotic stars offer a possibility to determine the relative brightness of the three sources mentioned. Consideration of absorption spectra as well as data on the galactic distribution of giants enable us to suppose that the luminosity of the cool component is equal to that of normal cool giants.

On the basis of this supposition one can determine the position of the hot component on the temperature-luminosity diagram. In Figure 1 it is seen that their position coincides fairly well with that of the central stars of planetary nebulae.

I think that symbiotic stars give us an example of such double-nuclei planetary nebulae. A hot component of a larger initial mass has reached the evolutionary stage of the planetary nebula nuclei, while another, initially less massive, component is still in the stage of a cool giant.

Abell: We can put a lower limit on the luminous energy required to remove $0{\cdot}2\ M_{\odot}$ from the surface of a star of radius R to infinity. The momentum required to remove a unit mass from the star to infinity is the velocity of escape. Thus the total momentum is

$$0{\cdot}2\ M_{\odot}\ \frac{2Gm^{1/2}}{R^{1/2}}\ \text{gm cm sec}^{-1}\,.$$

For $M = 1\ M_{\odot}$, we find this momentum to be $2{\cdot}5 \times 10^{40}/R^{1/2}$ with R in solar units. If this momentum is to be supplied by radiation pressure, we must have

$$\frac{Lt}{c} = 2{\cdot}5 \times 10^{40}/R^{1/2}\,,$$

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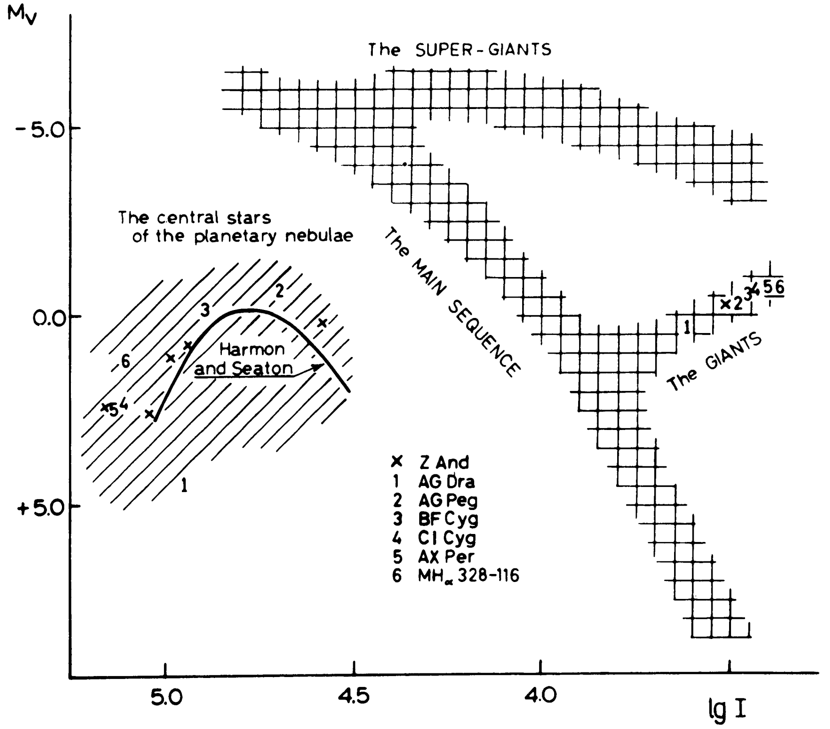

FIG. 1.

where L is the luminosity of the star and t is the time taken to transfer the momentum. If $t = 10^4$ years, and $R = 1$, we have $L = 6 \times 10^5$, solar luminosities, assuming complete opacity of the ejected material.

O'Dell: However, if the radius is as large as in late-type supergiants, a condition indicated several times in this meeting, then the luminosity requirements are much lower for "reasonable" timescales.

Abell: I should like to point out one other elementary fact. According to the vis-viva equation, the energy E with which the gas shell is ejected is

$$E = E_{es} + E_{\infty},$$

where E_{es} is the escape energy, and E_{∞} is the energy the material has at infinity. The nebulae now have, essentially, E_{∞}. Assuming 1 $M_{\odot}$ for the star and 0·2 $M_{\odot}$ for the nebula, we find

$$E_{es} = 7{\cdot}7 \times 10^{47}/R \text{ ergs},$$

with R in solar units. The ratio E_{es}/E_{∞} is thus about $400/R$. For extreme giants, $R \sim 10^2$–10^3, and E_{es} and E_{∞} are comparable; but if the matter were ejected from small stars, it would be most remarkable that E was equal to E_{es} within such narrow limits – one part in 400 for $R=1$, one part in nearly 10^5 for radii like those of the smallest central stars observed. Thus the evidence favors ejection from very large stars.

Kohoutek: Some years ago only one case of the binary nature of central stars was known: the optical binary NGC 246. At the present time:

(1) We have the following objects for which there exists evidence, more or less guaranteed, that they could be binary stars: NGC 1514, H_z 1-5 (V 377·1943 Sge), M 1-2 (VV8), K 1-2, IC 4406.

(2) There are also some stars, which are or could be variable: Hb 6=AS Sgr, H 3-29=S 5337, M 3-18, 12 central stars from Abell's list: A 14, 20, 30, 36, 41, 46, 51, 61, 63, 74, 78, and M 1-67, II 2149, NGC 2346, NGC 6891, II 4997, which show on AGK2 and AGK3 astrographic plates differences in brightness larger than observational errors.

(3) Some stars could be expected as not responsible for radiation of the nebula: NGC 3132, VV 68, NGC 2346; or some objects: II 2149, NGC 2392, NGC 6826, NGC 6572 show differences in temperature.

One can say that there are about 20 central stars (10% of all central stars known) which we could be suspicious of as being binaries.

Is this stage final, or could we expect – when collecting more precise photoelectric and spectrographic material – additional evidence for variability or the binary nature of planetary nuclei?

I believe that we have now a similar situation as in the problem of the binary nature of novae some years ago, and that the next years (and further observations) may show, that perhaps most of planetary nuclei are binary stars.

It is, of course, only an assumption now that planetary central stars are binaries, but if this assumption were correct, then it would bring a new idea to the problem of their evolution. Then we could explain the origin of planetary nebulae as a process connected with the evolution of binary stars.

Hekela and I suppose that the assumption of being binary may explain the origin of planetary nebulae much more easily than the assumption of a single star.

Let us assume moreover, that planetary nuclei are *close* binaries. Then, when the mass of primary components reaches the Roche critical surface, mass exchange will take place, the mass flows out to the second component (as calculated by Kippenhahn and Weigert, or for the larger mass stars by Plavec). Such a rapid process could cause a rapid change in hydrodynamic equilibrium of these stars, or an increase of radiation pressure. Then it is not very difficult to imagine that a part of the exchanged mass (0·1 $M_\odot$ is not more than about ten percent of the exchanged mass) could leave the central star and create the planetary nebula.

Reeves: Qualitatively, the $N^{14}+He^4$ reaction could well play an important role in amplifying thermal instabilities, because of its large temperature sensitivity. May I urge model builders to include this reaction in their models.

Rose: On the basis of the calculations of models for the nuclei of planetary nebulae that have been discussed it would appear that it is difficult to explain the short lifetimes and high luminosities observed for the nuclei of planetary nebulae unless the masses of the nuclei of planetary nebulae are greater than 1 $M_\odot$ or unless high rates of nuclear-energy generation have taken place in these stars immediately before the mass ejection. In addition, any mechanism for the formation of planetary nebulae must prevent the continuation of helium burning after the mass ejection if it is to explain the rapid evolution of the observed nuclei.

Weidemann: I was inclined to assume that white dwarfs of spectral type DA are the descendants of PN, since planetary nebulae and the atmospheres of the central stars are not hydrogen-free. However, arguments given by Salpeter and others favor models in which there is nearly no unburned material left on top of the C/O-PN nucleus star, opening the possibility that we have to tie the non-DA white dwarfs to PN. This would be all right as far as space density and mass is concerned (masses somewhat higher than the average DA mass of 0·6 $M_\odot$, say 0·8) but it then leaves us with the task to explain the majority of WD's in the DA sequence in another way. The masses of the DA sequence are almost certainly not higher than 0·6 $M_\odot$, as model calculations indicate, and as shown the best-determined mass of 40 Eri B is about 0·4 $M_\odot$.

Böhm-Vitense: It then seems to me that there is a serious discrepancy with respect to the mass because the theoreticians need masses of the order of 1 $M_\odot$, while the white dwarfs, which are supposed to be the descendants of the planetary nuclei do not have masses larger than 0·5 $M_\odot$. Where does the excess mass go?

Van Horn: If you believe the story about the crystalline nature of the interior of the white dwarfs, then there is a natural explanation for the apparent absence of the white dwarfs more massive than about 1·0 or 1·2 $M_\odot$. In such a star, the central temperature will fall below the Debye temperature of the crystalline material while the star is still quite bright, and the star will then cool very rapidly to invisibility because the specific heat quickly falls below the classical value of $3k/2$ per ion. Thus the apparent absence of such stars is not necessarily inconsistent with masses of the order of 1·0 to 1·2 $M_\odot$ for the central stars of the planetary nebulae.

Savedoff: On the problem of low-mass planetary nebulae it is difficult (as pointed out by Salpeter) to get any nuclear source for energy supply. A model of an iron star $M = 0{\cdot}631\ M_\odot$ computed by Savedoff, Van Horn, and Vila had a maximum peak $\log T = 8{\cdot}560$ at $M(r)/M = 0{\cdot}805$. A shell of helium could easily be ignited under these conditions. It is not suggested that this structure of iron core and helium envelope is very probable.

Reeves: The $C^{12}(d, \nu)O^{16}$ rate has been determined recently at Cal. Tech. The rate is such that the ratio of C/O at the end of helium burning is about 50–50 more or less independent of the stellar mass. May I suggest that such a mixture be used in models.

Rose: Abundance differences can significantly influence the evolution of a star.

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For example, the existence of a horizontal branch appears to be associated with metal abundance. Therefore, it is possible that if mass loss should take place on the horizontal branch (e.g. during the RR Lyrae phase) the ultimate mass of the star might be a function of initial chemical abundance and could be important in determining whether or not a star with a given initial mass evolves through a planetary nebula phase.

Aller: The brighter nuclei of planetaries that show absorption lines always show hydrogen. A possible example of a hydrogen-deficient atmosphere is that of the NGC 246 nucleus. The Wolf-Rayet stars may be hydrogen-deficient – we have no quantitative information on this problem.

Faulkner: Concerning the similarity of the compositions observed in planetary nebulae shells and in the interstellar medium, are we sure that the mass loss from planetary nebulae in the Galaxy is not sufficient to actually produce the current interstellar medium, and so determine its composition?

Salpeter: I believe that the statistics is such that an appreciable fraction but *not* a majority of star deaths go through a PN stage, so I feel the current interstellar composition is not strongly dominated by mass loss from PN.

Seaton: What can be said about the number of planetaries at earlier times in the history of the galaxy?

Salpeter: O'Dell in his calculations has taken into account ideas of an increased birthrate function at earlier stages of the Galaxy as estimated a few years ago by Schmidt and by myself.

Aller: To what extent can we always fix the conditions of the outburst that *only* the H-rich envelope is ejected, as Salpeter emphasized? A small residue is left. We might expect to find eventually some planetary nebula with an enriched heavy-element composition.

Mathews: The faint outer shells surrounding planetary may represent mass ejection during the next-to-last pulsation of an unstable star. Whether this feature represents a double ejection or it is a natural consequence of the nebular evolution remains to be seen.

Kippenhahn: I would like to know how the different groups which have computed evolution near the white-dwarf stage have dealt with the electron conductivity in the relativistic degenerate regions. We have been quite careless and have just extrapolated Mestel's formulae into the relativistic region. I wonder whether other people have been more careful.

Salpeter: I am not aware of actual calculations of electron conductivity with relativity included, but I would guess that the effects are not enormous for stars of relatively low mass.

Seaton: What happens to the models of Faulkner and Rose when neutrino processes are included?

Faulkner: I would expect the carbon burning to remain unimportant until higher masses, and the times of evolution would be shorter.

Rose: The presence of neutrino emission is of little importance for the evolution of the nuclei of planetary nebulae if the masses of these stars are less than or equal to about 0·7 $M_{\odot}$.

Salpeter: From the fact that only one PN has been observed among the globular clusters which have been looked at, one should be able to see if this fact is compatible with *all* stars in extreme population-II moving through a PN phase for 10^4 years, using methods of Sandage for 'semi-empirical evolutionary tracks'. My guess is that it will *not* be compatible, i.e. that too few PN are found in globular clusters.

Underhill: An expanding envelope similar to that in Population-I Wolf-Rayet stars might be detected in WR nuclei of planetary nebulae by looking carefully at the profile of C IV 5801·12 for shortward displaced absorption components.

Aller: We have not observed absorption lines of 5801, 5804 C IV in the central stars. The observations are very difficult. In the NGC 6543 nucleus, Lick coudé plates suggest that Hγ and Hδ may have P Cygni profiles. The Wolf-Rayet nuclei probably are ejecting material.

Menzel: Barnard, early in this century observing with the 40-inch refractor of the Yerkes Observatory, once reported variability and complete disappearance of the nucleus of NGC 7662. I took occasional plates of this planetary between the years 1926 and 1932, with the Crossley Reflector, but found no evidence of variability.

O'Dell: This reported variability is probably due to the low contrast between the star and the bright nebula, in the sense that under poor seeing conditions the stellar image blends into that of the larger nebula. This is described in detail elsewhere (*Sky and Telescope*, **29**, 1965, 85).

Perek: It is proposed that new discoveries of planetary nebulae be communicated to L. Kohoutek and their finding charts and coordinates published in the *Bull. astr. Inst. Csl.* in order to keep the list of planetary nebulae up to date.