ORIGIN AND EVOLUTION

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"It is said: He who is content with his opinion runs into danger. Blessing to the owner."*

Discussants have been asked to synthesize and prophecy rather than to summarize. The variety of models and views expressed here warn that any report is only another opinion. Blessing to the reader.

The major impression from these sessions is a general discontent with simple qualitative answers while encouraging more realistic exploration of both theoretical and observational questions. Let us examine some of the major questions raised.

What is the ancestor of the planetary nebula? Abell, Menzel, Paczyński, and Rose answer: Red giants – though for different theoretical reasons. It remains probable from the presentations that this is a phase differing from those currently discussed in evolution from the main sequence in having a very dense core and a large envelope. It is suggested that these cores lie between $0{\cdot}5\ M_{\odot}$ and $1{\cdot}5\ M_{\odot}$ and have envelopes comparable to the resultant planetary both in mass and composition. The Rochester group emphasizes that the major mass loss may occur prior to planetary formation and attempts to produce planetaries in relatively simple objects of uncertain parentage.

What is the energy source for planetary production? The energy is bounded by the observed kinetic energy, 10^{46} ergs, and the estimated 10^{49} ergs required to lift this much material from a degenerate core. Most agree that nuclear energy is involved, except Paczyński who seems to be depending upon the ionization energy of H and He.

Why are planetaries characterized by a low velocity of expansion compared to the velocity of escape of the central star? In Paczyński's view, this is a natural consequence of the positive energy of the extended envelopes, the velocity of expansion being equivalent to the energy available to a recombining proton and electron. What is expected from the pulsational instabilities discussed by Rose? It was suggested to me by Ledoux in May 1965 that pulsations in these models may build up until the surface layers at maximum expansion velocity exceed the velocity of escape. The added energy sink (mass loss) may limit the amplitude of the pulsation provided the input is not too large. Thus, if the energy in pulsation grows on a time-scale comparable to the

* Inscription on Afrasiyab-ware plate from Nishapur, Iran, 10th century A.D. Freer Gallery No. 57.24, Washington, D.C.

Osterbrock and O'Dell (eds.), Planetary Nebulae, 463–466. © *I.A.U.*

period, an equilibrium would be established. Perhaps novae reflect still larger amplification factors.

The only form of mass loss for which we have quantitative models is the solar wind. A complete story would need a theory of how the 'corona' was formed (from convection?) and the prediction of densities and velocities. Scaling from solar parameters, one finds excessive velocities if the critical potential is measured by the observed radius of central stars, and one is again led to a 'red giant model' in which the critical potential is that of a hundredfold larger radius than observed.

K. H. Böhm has called our attention to a new class of atmosphere problems in which the luminosity satisfies the inequality

$$L > 10^{4 \cdot 8} M/(1 + X),$$

all quantities being in solar units and specialized to electron-scattering opacity. Here radiation pressure dominates gravitation, and no purely static or even convective solutions are expected. I have not encountered discussions of this problem previously and would like to speculate on its dimensions. Do we need to study the 'real' problem or is there much to be learned from the 'grey' atmosphere approximation? Is the plane parallel approximation at all useful? How sensitive are the hydrodynamic solutions to starting-conditions, and to the little approximations that must be made?

Our previous discussion is concerned with the removal of the gas shell from the star. The dynamics of the gas shell itself has been the subject of papers here by Menzel, Gurzadian, Khromov, Kahn, and Mathews. Although there has been some isolated support for dominant magnetic fields and non-thermal processes, these seem not required by the observations reported at this conference. One should recall that, in the absence of an adequate description of how mass-loss occurs, the initial conditions for the gas-shell dynamics are the pressure, temperature, velocity of the gas at some initial time within some volume, the incident radiation and corpuscular fluxes (which are not only a function of time but may also be dependent upon frequency and position on the surface of the volume). Models fitting observed planetaries are obviously sufficient but not necessary in the presence of such vast unexplored degrees of freedom. Thus corpuscular fluxes and magnetic fields appear to be interesting deductions but, pending considerable investigation, unproven.

Some topics of more specialized interest in the area principally of the physics of white dwarfs require comment. In principal, the high electron conduction damps thermal memories of the star's earlier history so that we need only guess the composition. Even the composition is obscured by the low dependence of the equation of state of degenerate matter on composition ($\mu_e \sim 2$). The simplicity of evolution below $0 \cdot 1\ L_\odot$ permits interpretation of the rather weak observational statistics available. I will not discuss the mass-radius relation as it is difficult to assess possible systematic errors in the data for Sirius B.

Evolutionary models of white dwarfs permit prediction of the luminosity or temperature distribution of the white dwarfs. This has led Weidemann to estimate a production rate of 2×10^{-12} yr^{-1} pc^{-3}, a rate which is perhaps 2–20 times that of the planetary nebulae. This seems based on the present density of white dwarfs between $M_v = 11{\cdot}5 \pm 0{\cdot}5$, and the assumption of uniform production in time over the past 5×10^9 years. If we choose to base our estimates on all white dwarfs brighter than $V = 14$, in a particular band of color, we would eliminate the need for parallaxes. The evolutionary curves would provide an estimate of the volume occupied by the sample and the transit time. For this magnitude limit, even the brightest planetary nucleus lies within a scale-height of the galactic plane permitting the assumption of constant density. A color-dependence of the apparent production rate need not be interpreted as galactic evolution but would be related to details of the evolutionary tracks not observable in other ways. Aside from observational selection, four physical phenomena would be revealed in this way.

An absence of the bluest stars may result from the blue limit to the surface temperature of a contracting non-degenerate star which results from the onset of core degeneracy. This can be interpreted as evidence of too high a mass in the models. Savedoff, Rose and others have emphasized that the radius of these brightest models is strongly affected by small hydrogen envelopes. Thus the same empirical evidence might imply the mass of the hydrogen envelope has been underestimated. Again, neutrino losses have been shown to speed evolution in the range of colors bluer than U-B $= -1{\cdot}0$ for masses exceeding perhaps $0{\cdot}5\ M_{\odot}$. Alternatively, evolution in the range U-B $= 0{\cdot}0$ may be slowed by the heat of crystalization discussed by Van Horn. I have received a private communication from Ostriker noting that the specific heat of a crystal star below the Debye temperature is sufficiently small so that the evolution rate is increased and a deficiency of very red stars would be expected. This low specific heat allows a star to reach zero temperature in a finite time. It is not clear how these effects can be separated, but it would be useful if the observers could find the color distribution so that an attempt can be made to interpret the shape of the distribution in these terms.

Lastly, we must admit that the physics of matter and the contents of our models leave the theorists with some unfinished business. The calculations of the rate of the carbon-carbon reaction have been questioned by Reeves on the basis of recent measures by Fowler's group. The neutrino rates have been estimated by Salpeter to be correct to within 10 %, provided that the elementary interaction

$$e^+ + e^- \rightarrow \nu + \tilde{\nu}$$

exists and is correctly given by the theory of the universal Fermi interaction. These hypotheses appear untestable in the laboratory. It must be noted that the conductivity of the electrons in the region of relativistic degeneracy has not been treated, and although the conductivity is so high in this region that its effect should be unobservable, the present situation is a bit untidy. Model calculations are plagued by the expense resulting from relaxation oscillations and uncertainties arising from the physics of mixing. As always effects of rotation and magnetic fields remain on the edge of the calculable, although there are better ideas of the magnitude of the effects which may be expected.

In conclusion, we have many opinions of the explanation of the planetary phenomena and the evolution from the central stars into the white-dwarf regions, but one has little cause to be content with his own opinions.

DISCUSSION

Salpeter: Appreciable mass-loss by radiation pressure from stars of small radius (like central stars of PN) would require a lot of energy drain from the optical luminosity (via red-shift of photon). Can anyone give an upper limit to such mass-loss which would not contradict observed spectral data?