SECTION V

CHAIRMAN: H. JOHNSON

https://doi.org/10.1017/S0074180900098715 Published online by Cambridge University Press

EVOLUTIONARY ASPECTS OF WOLF-RAYET STARS

B. PACZYŃSKI

Institute of Astronomy, Polish Academy of Sciences, Warsaw, Poland

1. Introduction

Problems related to Wolf-Rayet stars were recently reviewed by Underhill (1968) and were discussed at length during the symposium held at the Joint Institute for Laboratory Astrophysics in Boulder (Gebbie and Thomas, 1968). Therefore, I shall not review here the historical development of ideas, and I shall not give detailed references in those cases when all the information is available in the sources mentioned above. I shall discuss first those observational data that are most important for the understanding of the evolutionary status of these stars. Later I shall discuss the problem of the interior structure and the origin of WR stars. And finally theories of the driving force for the observed mass loss will be presented.

2. Observations

There seem to be three kinds of Wolf-Rayet stars known: nuclei of some planetary nebulae, lighter components of massive binary systems, and single Population I objects, frequently surrounded with ring nebulae. Nuclei of planetary nebulae are believed to have masses close to 1 $M_{\odot}$, but there is no direct observational evidence to substantiate this belief. WR components of spectroscopic binaries have masses close to 10 $M_{\odot}$ and their companions are O or B type stars of ~ 30 $M_{\odot}$. Single Population I WR stars are probably massive objects as they are young, but it is not possible to make a more specific statement.

Absolute visual magnitudes of the Population I WR stars are between -4 and -6 and nuclei of planetary nebulae are considerably fainter. All these stars are hot and radiate most of their energy in the ultraviolet. For this reason the most reliable (at least in principle) estimates of the effective temperature are available for the objects surrounded by nebulosities. According to Morton (1969, 1970) these temperatures are 50000 K, 33000 K, 23000 K and below 35000 K for WN5, WN6, WN8 and WC8 stars respectively. Van Blerkom (1971) estimates effective temperatures for two nuclei of planetary nebulae, BD $+30°3639$ (WC9) and NGC 40 (WC8), to be equal to 32000 K and 34000 K respectively. There are 9 additional planetary nebulae with WR nuclei (Underhill, 1968) for which O'Dell (1963) and Seaton (1966) give effective temperatures. The mean logarithm of the temperature for these stars is 4.64 according to O'Dell, and 4.86 according to Seaton. Temperatures assigned by Capriotti and Kovach (1968) are even higher. Unfortunately, only one of these nine stars has a truly WR-type spectrum according to Smith and Aller (1969).

The effective temperature for γ_2 Velorum (WC8 + O7) as measured by Hanbury

M. K. V. Bappu and J. Sahade (eds.), Wolf-Rayet and High-Temperature Stars, 143–162.

Brown et al (1970) is equal to 29000 K. According to Lindsey Smith (private communication) and Baschek and Scholz (1971) the WC8 star is considerably fainter than its O7 companion. If this is true, then the interferometric measurements refer to the O7 star rather than to the WC8 component of this binary system.

Absolute visual magnitudes given by Smith (1968) and effective temperatures given by Morton (1969; 1970) and Van Blerkom (1971) may be used to obtain absolute bolometric magnitudes for the Population I Wolf-Rayet stars. It is surprising that $M_{bol} = -8.7 \pm 0.5$ is obtained for all the subtypes. The corresponding luminosity is $2.4 \times 10^5 L_{\odot}$. It would be surprising if the accuracy of this estimate were better than 1 mag., and in fact the error may be considerably larger. The mean bolometric magnitude of the nine nuclei of planetary nebulae with WR-like spectra is -4.3 according to O'Dell and -6.0 according to Seaton (1966).

When this luminosity of $2.4 \times 10^5 L_{\odot}$ is compared with the main sequence luminosity of a $10 M_{\odot}$ star ($L = 0.5 - 1.0 \times 10^4 L_{\odot}$) it becomes clear that Population I Wolf-Rayet stars are highly overluminous for their masses. This statement is true for WR binaries, but it is commonly applied to single WR stars as well. We should be aware that even in the case of binaries a certain amount of extrapolation is involved. We do know masses for binaries, but the best estimates of bolometric corrections may be obtained for the stars surrounded with the nebulosities. And these stars are single. WR nuclei of planetary nebulae with estimated luminosities of the order of $10^4 L_{\odot}$ are certainly highly overluminous for any sensible mass that may be assigned to them.

I think that everybody would agree that even if masses and luminosities of Wolf-Rayet stars are not very well known, these parameters may be obtained with fair accuracy for some objects at least. It is far less obvious that we can meaningfully assign a particular radius to any given WR object. For example, Castor and Van Blerkom (1970) estimate the photospheric radius of HD 192163 (WN6) to be 13 $R_{\odot}$ at λ 5500, while the line emitting region is supposed to be five times larger. Given the density of He II ions and the population of different energy levels, it is possible to find that line emitting region is optically thick in the helium Balmer and Lyman continua. In this case the photosperic radius at λ 228 and λ 912 may be as large as 70 $R_{\odot}$.

Wolf-Rayet stars show evidence of mass outflow with velocities in the range of 500–2000 km s^{-1} as estimated from the violet displaced absorption components of some spectral lines. The wide emission lines are most readily interpreted as originating within the radially expanding envelope (see, e.g., Castor, 1970; and Castor and Van Blerkom, 1970). This envelope must be much larger than the star itself as no occultation effect is observed. Acceleration of the outflowing gas must take place within this extended envelope as a correlation is observed between line width and ionization potential (see, e.g., Smith and Aller, 1971), and no lines were reported to show an occultation effect. The rate of mass loss is believed to be in the range of $10^{-6} - 10^{-4} M_{\odot}$/yr.

Planetary nebulae and the so-called 'ring nebulae' (Johnson and Hogg, 1965; Smith and Batchelor, 1970) are visible around some single WR stars. When the expansion velocity of a nebula is known it is of the order of 30 km s^{-1}. Masses of the nebulae are very uncertain, and estimates may range from a fraction of a solar mass up to 100 $M_\odot$ and more. Nebulosities classified as planetary nebulae are believed to have been ejected from their central stars. It is obvious that the ejection mechanism must have been different from that which is responsible for the gas outflow observed now, as there is huge difference between the observed velocities: 30 km s^{-1} vs. 1000 km s^{-1}. Nebulosities observed around Population I WR stars are called ring nebulae and are believed to have been formed from interstellar matter swept by gas streaming from the central stars. Perhaps it is possible to explain these nebulae in terms of mass loss by the central stars only. Some objects traditionally classified as nuclei of planetary nebulae may in fact be Population I WR stars. The best known, and the most controversial object of this kind is BD + 30°3639. NGC 40 may be another candidate. I believe the nature of these stars will be discussed during this symposium. The difference between the two kinds of single Wolf-Rayet stars may not be so large as is commonly assumed.

Wolf-Rayet stars seem to be hydrogen deficient (see, e.g., Sljusariev, 1955; Castor and Van Blerkom, 1970), but probably not all the investigators would agree with this statement. According to Castor and Nussbaumer (1971) the WC8 component of γ_2 Velorum is overabundant in carbon by a factor of ten compared to normal stars. The existence of the two spectroscopic sequences may indicate that there is a dichotomy in the carbon to nitrogen ratio among the WR objects. This is a very controversial statement too. Let me hope that everybody may accept the point of view that there is no strong observational evidence against the hypothesis that Wolf-Rayet stars are hydrogen-poor, and have peculiar CNO abundances.

3. Evolutionary Status

Let us attempt to find the evolutionary phases that could be fitted to at least some of the known characteristics of the WR stars. Let us not consider here the very difficult problem of the origin of their spectra. It should be emphasized that nobody has succeeded so far in explaining why some stars develop such a peculiar spectrum. Let us consider such parameters as mass, luminosity and effective temperature. These are most easily compared with the models of stellar interiors. It should be realized that ordinary evolutionary computations may provide us with time variations of these parameters when the initial mass and chemical composition are specified, but these computations do not tell us how the stellar spectrum should look. Almost all the published evolutionary sequences refer to the models in hydrostatic equilibrium. At the same time the observed layers of the WR stars are very far from hydrostatic equilibrium, and to make things worse their radii are not well defined. Fortunately, it is reasonable to assume that almost all the energy being lost by these stars is lost in a form of electromagnetic radiation. Therefore, we may assume that the luminosity of a static theoretical model may be meaningfully compared with the observed luminosity of a Wolf-Rayet star. It is also reasonable to assume that the mean radius of an unstable star is not smaller than the equilibrium radius, and therefore, the mean effective temperature does not increase as a result of instability. These assumptions indicate that we should look for theoretical models with effective temperatures equal to or larger than those assigned to the observed WR stars.

The evolutionary behaviour of single and binary stars as referred to in this chapter is based on the published review articles (Iben, 1967; Paczyński, 1971b), and on some of my papers (Paczyński, 1970a; Paczyński, 1970b; Paczyński, 1971a).

Let us concentrate first on Population I Wolf-Rayet stars. Let us look for a model with a mass of 10 $M_{\odot}$, a luminosity of $2 \times 10^5 L_{\odot}$, and an effective temperature higher than 30000 K. It is clear that such a model cannot be in a pre-main sequence contraction (cf. Iben, 1965; Larson and Starrfield, 1971). It is also by far too luminous to be on a hydrogen main sequence.

About 80 or 90% of the entire lifetime of a massive star is spent on the main sequence, i.e. in a core hydrogen burning phase. This lifetime depends mainly on the stellar mass and ranges from 10^7 yrs for 15 $M_{\odot}$ to 10^6 yrs for $M > 100$ $M_{\odot}$. It is also sensitive to the initial helium content. High helium content leads to high luminosity and short lifetime. The luminosity and lifetime are not sensitive to the abundance of heavy elements, at least for $Z < 0.05$. There is no reason why the Wolf-Rayet stars should be younger than 10^6 or 10^7 yrs, and the observationally estimated ages are consistent with the 'post main sequence' hypothesis (Mikulášek, 1969). It is important to note that the lifetime of the WR phenomenon may be orders of magnitude shorter than the age of the corresponding star as counted from the pre-main sequence contraction phase. It should be noted too that it is entirely impossible to distinguish observationally, by means of age estimates, in which part of the post main sequence evolution a particular group of stars may be. If there is an age difference between two subtypes of WR star it is most likely due to the variations in the initial mass and/or initial helium abundance. This is so because the duration of the whole post main sequence evolution is ten times shorter than the main sequence lifetime. This applies to binaries as well as to single stars.

It is well known that the luminosity to mass ratio changes very little during the evolution of a massive star, provided the stellar mass is constant and provided there is no large scale mixing that makes the star chemically homogeneous. This kind of evolution cannot explain high overluminosities of the Wolf-Rayet stars.

The luminosity of a homogeneous model increases strongly with the increasing mean molecular weight within the star. If a stellar model is being completely mixed it may become highly overluminous. In this case the main sequence lifetime is longer than if there were no mixing. The luminosity of a model that is not continuously homogenized depends mainly on its initial mass and initial helium content. If such a model is subject to a mass loss in a post main sequence evolution, then the luminosity does not change significantly as long as only the hydrogen-rich layers are removed. In this case the luminosity to mass ratio is increased too, and overluminous models are obtained. If considerable fraction of stellar mass is lost during the main sequence lifetime the picture is more complicated. The luminosity may decrease considerably, and the lifetime is increased.

Let us consider now a massive star that is a member of a close binary system. As soon as the star fills its Roche lobe, mass transfer to the companion will take place. If mass exchange takes place after hydrogen exhaustion in the core of the primary (the so-called case B) then this component will resemble a pure helium star after the termination of the mass transfer process. It has been suggested that this type of evolution leads to binaries with Wolf-Rayet components (Paczyński, 1967; Kippenhahn, 1969; Barburo *et al.*, 1969). In this case the overluminosity of the WR component is due to the mass loss. This star is more advanced in evolution than its companion as it was originally the more massive of the two. Large scale mixing may be excluded as a source of overluminosity in this case because it would lead to a very slow evolution of the would-be WR star, which could not become overluminous within the main sequence lifetime of the OB companion.

A star of 10 $M_{\odot}$ on the helium main sequence has a luminosity of 1–2 × $10^5 L_{\odot}$, and that is just what we need for a Wolf-Rayet component in a massive binary. If we believe in the universal Fermi interactions we can say that these stars are unlikely to be in the evolutionary phases following core helium burning. In this case the lifetime of the WR phenomenon would be less than 10^3 yrs as a result of severe energy losses due to neutrino emission (Paczyński, 1971a). Therefore, we may be almost certain that Wolf-Rayet components of massive binary systems are helium main sequence stars, perhaps with some hydrogen left in their envelopes. If there is no neutrino emission, then carbon burning cannot be excluded as an energy source for these stars. It is interesting that the relation between the helium models and WR stars was already suggested two decades ago by Salpeter (1952).

There is no direct observational evidence that single Population I Wolf-Rayet stars are overluminous as their masses are not known. If it were possible to demonstrate that these stars are hydrogen defficient than we could be sure that they are also overluminous. According to Castor and Van Blerkom (1970), HD 192163 is a single, hydrogen deficient WN6 star. Hydrogen deficiency might be due either to large scale mixing or to large mass loss. If it were possible to demonstrate that ring nebulae are ejected from their central stars, or that BD + 30°3639 is a Population I object, we would have evidence that massive hydrogen-rich envelopes have been lost by those stars. Notwithstanding such a poor knowledge of the most basic parameters for single WR stars, a large number of theoretical papers make the assumption that these objects are overluminous and helium-rich. It is mass loss rather than mixing that is currently used to produce the overluminosity. Vibrational instability (Simon and Stothers, 1970; and the papers referred to therein) and radiation pressure (Bisnovaty-Kogan and Nadyozhin, 1972) have been suggested as possible mechanisms responsible for that mass loss. Neither of these has yet been generally accepted as physically plausible. However, it should be emphasized that there seems to be a deficiency of luminous red supergiants (see, e.g., Schild, 1970). This deficiency was interpreted either as evidence of existence of the universal Fermi interactions and neutrino energy losses (Stothers, 1969), or as evidence of large mass loss from the luminous red supergiants (Barburo *et al.*, 1969). Some mass loss is indeed observed (see, e.g., Gehrtz and Woolf, 1971), though it is not clear if it is sufficiently rapid to be important for stellar evolution. It is tempting to suggest that very massive stars lose their hydrogen-rich envelopes as soon as they become red supergiants. The helium core of such a star is left as a hot and overluminous object, presumably a single Wolf-Rayet star (Bisnovaty-Kogan and Nadyczhin, 1972). Such an evolutionary pattern is not possible for a component of a close binary, as in that case a red supergiant phase cannot be achieved because of mass exchange.

Let us consider now those WR stars that are found in planetary nebulae. It is not known whether all the central stars of planetary nebulae develop Wolf-Rayet type spectra at some evolutionary phase. But it is reasonable to suppose that all these objects have masses not very different from 1 $M_{\odot}$. Their high luminosities may be most easily explained by models with degenerate carbon-oxygen cores surrounded with one or two shell sources. Models of this kind were studied by Rose and Smith (1970), Paczyński (1970a, 1970b), and Uus (1970). This kind of stellar structure is obtained if one follows evolution of a model all the way from the main sequence through the hydrogen and helium exhaustion in the core, up to helium shell burning phase, in the red supergiant region of the HR diagram. At present, the theory cannot explain how the envelopes of such stars are ejected to form planetary nebulae, or why some nuclei develop Wolf-Rayet type spectra. However, the observed luminosities of the central stars are so high that it would be most surprising if young nuclei of planetary nebulae were not in a helium shell burning phase. The origin and evolution of these stars has been recently reviewed by Salpeter (1971).

4. Mass Loss From WR Stars

According to the most reasonable interpretation of the Wolf-Rayet spectra these stars are losing mass at the rate of $10^{-6} - 10^{-4}$ $M_{\odot}$/yr. The electron temperature of the expanding envelope is estimated to be of the order of 5×10^4 K or 10^5 K. A large number of the observed spectral characteristics may be explained in terms of such a model (see, e.g., Castor, 1970b; Castor and Van Blerkom, 1970; Castor *et al.*, 1970; and papers referred to therein). As mentioned in one of the previous chapters, the gas is accelerated outwards within the extended line emitting region. It is possible that the photospheric radius is much larger in the ultraviolet than in the visual. The kinetic energy flux associated with the mass outflow is of the order of $10^2 - 10^4 L_{\odot}$, that is about one per cent of the total stellar luminosity. There is no satisfactory theory that could explain the mechanism responsible for this outflow of gas, and this is perhaps the most challenging of the problems raised by WR stars. It should be emphasized that the cause of this mass loss may be completely different from the cause of the large mass loss that presumably took place previously, when the progenitors of Wolf-Rayet stars had massive hydrogen rich envelopes. Let us consider various suggestions for the driving force of the gas outflow observed now.

According to Limber (1964) mass outflow from Wolf-Rayet stars might be due to 'forced rotational instability'. This model cannot explain the origin of violet displaced absorption components visible at the edges of some emission lines, and therefore, it has not found support among other investigators. Since Castor (1970a) has shown that the so called transit-time effect is not important for WR binaries there seem to be no serious objection left to the model with a radially expanding envelope. Let us consider it more closely now.

It has been suggested that WR stars are vibrationally unstable helium stars (Paczyński, 1967; Simon and Stothers, 1969; Simon and Stothers, 1970; Ungar, 1971). This instability is supposed to be due to the very high temperature sensitivity of the triple alpha reaction, and at large amplitude could lead to the mass outflow. Similar instability appears in models of massive hydrogen rich stars, and in that case might be responsible for the P Cygni phenomenon (Schwarzschild and Harm, 1959; Appenzeller, 1970a). Recent non-linear hydrodynamic computations (Appenzeller, 1970a; Appenzeller, 1970b; Ziebarth, 1970; Talbot, 1971a; Talbot, 1971b; Ungar, 1971) indicate that mass loss driven by the pulsational instability is possible, though the results are not conclusive yet. Talbot and Papeloizou (Trimble, 1971) find that pulsational instability almost certainly does not blow the star apart, because pulsation energy is dissipated in shock waves and fed into stable harmonics. Rather, material seems to be lost gradually. It is very interesting that at the limiting amplitude the mean radius of a massive hydrogen rich star increases by a factor of 4 compared with the equilibrium value, and as a result the effective temperature decreases by a factor of two. No computations of this kind have been published for helium stars, but the picture should be similar. Pulsational instability induced by the triple alpha reaction may appear in chemically homogeneous helium stars with masses exceeding approximately 15 $M_{\odot}$ or so (Stothers and Simon, 1970), with the period of oscillations close to half an hour. This limiting mass was previously thought to be close to 8 $M_{\odot}$ because only the opacity due to electron scattering was used by Boury and Ledoux (1966). If there is a small hydrogen rich envelope present at the top of the helium core then the critical mass is considerably increased (Van den Borght, 1969; Stothers and Simon, 1970). It is difficult to reconcile these rather large masses with those observed in WR binaries. However, it is interesting that vibrational instability driven by the triple alpha reaction was also found in some helium shell burning models (Rose, 1967), which may be relevant for the WR nuclei of planetary nebulae.

Radiation pressure in the continuum was suggested by Rublev (1964b, 1965b) to be responsible for the mass from WR stars. Rublev (1964a; 1965a) estimated the effective temperatures to be close to 10^5 K. Because of very large bolometric corrections the estimated luminosities of WR stars were found to exceed the critical luminosity which is defined as

$$L_{cr} = \frac{4\pi Gc}{\kappa} M.$$

With electron scattering taken for the opacity we have

$$\frac{L_{cr}}{L_\odot} = \frac{65000}{(1+X)} \frac{M}{M_\odot},$$

where X is the hydrogen content by mass. For $L > L_{cr}$ stellar gravity cannot keep the atmosphere in hydrostatic equilibrium, and mass outflow is forced by the radiation pressure. Unfortunately, it is not possible to construct an interior model in hydrostatic equilibrium with such a high luminosity. I know only one evolutionary sequence in which the surface luminosity exceeded the critical value (Rose, 1969) but it was due to the improper surface boundary condition used. No selfconsistent model of mass outflow driven by radiation pressure in the continuum has been published so far, but there have been many attempts to do this (see, e.g., Bisnovaty-Kogan and Zeldovich, 1968; Bisnovaty-Kogan and Nadyozhin, 1972; Kutter *et al.*, 1969; Finzi and Wolf, 1970, 1971).

A few years ago mass outflow from early type supergiants was discovered by Morton (1967a, 1967b). Radiation pressure in resonance lines was found to be responsible for this phenomenon (Lucy and Solomon, 1970; Lucy, 1971). This mechanism may lead to a mass loss rate that does not exceed L/c^2 (Lucy and Solomon, 1970), corresponding to 10^{-8} $M_\odot$/year for a Population I WR star. This is a few orders of magnitude less than the rate observed in these stars.

From time to time one hears the suggestion that some kind of stellar activity may produce a hot corona around a WR star. Such a corona may generate a stellar wind much more powerful than the solar wind we know. Gas outflow with the velocity close to 1000 km s^{-1} indicates that the coronal temperature is in excess of 10^7 K. This temperature is two orders of magnitude higher than that indicated by the observed spectra of the expanding Wolf-Rayet envelopes. The same problem arises in every model in which the gas pressure is supposed to be a driving force. It is likely that this difficulty may appear, at closer inspection, in the vibrating star model mentioned earlier.

It is clear that there is no satisfactory explanation for the observed mass loss from Wolf-Rayet stars.

5. Summary and Concluding Remarks

The available observations indicate that Wolf-Rayet stars are objects with very high luminosity to mass ratio. This indicates that there is very little, if any, hydrogen left within these stars. Apparently a large fraction of mass has been lost in the past either to the nearby companion or to interstellar space as indicated by the presence of planetary and ring nebulae. These nebulae expand with a velocity of about 30 km s^{-1}, and it is probable that single WR stars lost their envelopes when their ancestors were red supergiants. This problem may be closely related to the origin of planetary nebulae.

Population I stars that appear as Wolf-Rayet objects are most likely burning helium in their cores, while disc population Wolf-Rayet stars are almost certainly in a helium shell burning phase of evolution. These stars are losing mass at the rate of $10^{-6} - 10^{-4}$ $M_\odot$/yr with the velocities of outflow of the order of 1000 km s^{-1}.

Expanding envelopes are responsible for spectral peculiarities, but the driving force is not known. It may be due to the high L/M ratio, peculiar abundances, vibrational instability of the star, or most likely radiation pressure may be responsible for this phenomenon.

Acknowledgements

It is a great pleasure to acknowledge that my attendance at the Symposium has been made possible by generous travel grants from the Argentinian National Research Council and from the International Astronomical Union. I am also greatly indebted to Dr. Virginia Trimble for a careful reading of the manuscript.

References

Appenzeller, I.: 1970a, *Astron. Astrophys.* **5**, 355.
Appenzeller, I.: 1970b, *Astron. Astrophys.* **9**, 216.
Barburo, G., Dellaporta, N., and Fabris, G.: 1969, *Astrophys. Space Sci.* **3**, 123.
Barburo, G., Giannone, P., Giannuzzi, M. A., and Summa, C.: 1969, in M. Hack (ed.), *Mass Loss from Stars*, D. Reidel Publ. Co., Dordrecht-Holland, p. 217.
Bashek, B. and Scholz, M.: 1971, *Astron. Astrophys.* **11**, 83.
Bisnovaty-Kogan, G. S. and Zeldovich, Ya. B.: 1968, *Astron. Zh.* **45**, 241.
Bisnovaty-Kogan, G. S. and Nadyozhin, D. K.: 1972, *Astrophys. Space Sci.* **15**, 353.
Boury, A. and Landoux, P.: 1965, *Ann. Astrophys.* **28**, 353.
Capriotti, E. R. and Kovach, W. S.: 1968, *Astrophys. J.* **151**, 991.
Castor, J. I.: 1970a, *Astrophys. J.* **160**, 1187.
Castor, J. I.: 1970b, *Monthly Notices Roy. Astron. Soc.* **149**, 111.
Castor, J. I. and Van Blerkom, D.: 1970, *Astrophys. J.* **161**, 485.
Castor, J. I., Smith, L. F., and Van Blerkom, D.: 1970, *Astrophys. J.* **159**, 1119.
Castor, J. I. and Nussbaumer, H.: 1971, *Bull. Am. Astron. Soc.* **3**, 378.
Finzi, A. and Wolf, R. A.: 1970, *Astrophys. Letters* **5**, 63.
Finzi, A. and Wolf, R. A.: 1971, *Astron. Astrophys.* **11**, 418.
Gebbie, K. G. and Thomas, T. N. (ed.): 1968, *Wolf-Rayet Stars*, U.S. Government Printing Office, Washington, D.C.
Gehrz, R. D. and Woolf, N. J.: 1971, *Astrophys. J.* **165**, 285.
Hambury Brown, R., Davis, J., Hebison-Evans, D., and Allen, L. R.: 1970, *Monthly Notices Roy. Astron. Soc.* **148**, 103.
Iben, I., Jr.: 1965, *Astrophys. J.* **141**, 993.
Iben, I., Jr.: 1967, *Ann. Rev. Astron. Astrophys.* **5**, 571.
Johnson, H. M. and Hogg, D. E.: 1965, *Astrophys. J.* **142**, 1033.
Kippenhahn, R.: 1969, *Astron. Astrophys.* **3**, 83.
Kutter, C. S., Savedoff, M. P., and Schuerman, D. W.: 1969, *Astrophys. Space Sci.* **3**, 183.
Larson, R. B. and Starfield, S.: 1971, *Astron. Astrophys.* **13**, 190.
Limber, D. N.: 1964, *Astrophys. J.* **139**, 1251.
Lucy, L. B.: 1971, *Astrophys. J.* **163**, 95.
Lucy, L. B. and Solomon, P. M.: 1970, *Astrophys. J.* **159**, 879.
Mikulášek, Z.: 1969, *Bull. Astron. Inst. Czech.* **20**, 215.
Morton, D. C.: 1967a, *Astrophys. J.* **147**, 1017.
Morton, D. C.: 1967b, *Astrophys. J.* **150**, 535.
Morton, D. C.: 1969, *Astrophys. J.* **158**, 629.
Morton, D. C.: 1970, *Astrophys. J.* **160**, 215.
O'Dell, C. R.: 1963, *Astrophys. J.* **138**, 67.
Paczyński, B.: 1969, *Acta Astron.* **17**, 355.
Paczyński, B.: 1970a, *Acta Astron.* **20**, 47.

Paczyński, B.: 1970b, *Acta Astron.* **20**, 287.
Paczyński, B.: 1971a, *Acta Astron.* **21**, 1.
Paczyński, B.: 1971b, *Ann. Rev. Astron. Astrophys.* **9**, 183.
Rose, W. K.: 1967, *Astrophys. J.* **150**, 193.
Rose, W. K.: 1969, *Astrophys. J.* **155**, 491.
Rose, W. K. and Smith, R. L.: 1969, *Astrophys. J.* **159**, 903.
Rublev, S. V.: 1964a, *Astron. Zh.* **41**, 224.
Rublev, S. V.: 1964b, *Astron. Zh.* **41**, 1063.
Rublev, S. V.: 1965a, *Astron. Zh.* **42**, 347.
Rublev, S. V.: 1965b, *Astron. Zh.* **42**, 718.
Salpeter, E. E.: 1952, *Astrophys. J.* **115**, 326.
Salpeter, E. E.: 1971, *Ann. Rev. Astron. Astrophys.* **9**, 127.
Schild, R. E.: 1970, *Astrophys. J.* **161**, 855.
Schwarzschild, M. and Harm, R.: 1959, *Astrophys. J.* **129**, 637.
Seaton, M. J.: 1966, *Monthly Notices Roy. Astron. Soc.* **132**, 113.
Simon, N. R. and Stothers, R.: 1969, *Astrophys. J.* **155**, 247.
Simon, N. R. and Stothers, R.: 1970, *Astron. Astrophys.* **6**, 183.
Sljusarev, S. G.: 1955, *Astron Zh.* **32**, 346.
Smith, L. F.: 1968, in K. G. Gebbie and R. N. Thomas (eds.), *Wolf-Rayet Stars*, U.S. Government Printing Office, Washington, D.C., p. 23.
Smith, L. F. and Aller, L. H.: 1969, *Astrophys. J.* **157**, 1245.
Smith, L.F. and Aller, L. H.: 1971, *Astrophys. J.* **164**, 275.
Smith, L. F. and Batchelor, R. A.: 1970, *Australian J. Phys.* **23**, 203.
Stothers, R.: 1969, *Astrophys. J.* **155**, 935.
Stothers, R. and Simon, N. R.: 1970, *Astrophys. J.* **160**, 1019.
Talbot, R. J. Jr.: 1971a, *Astrophys. J.* **163**, 17.
Talbot, R. J., Jr.: 1971b, *Astrophys. J.* **165**, 121.
Trimble, V.: 1971, *Nature* **232**, 607.
Underhill, A. B.: 1968, *Ann. Rev. Astron. Astrophys.* **6**, 39.
Ungar, S. G.: 1971, *Bull. Am. Astron. Soc.* **3**, 242.
Uus, U.: 1970, *Astr. Council, Acad. Sci. U.S.R.R., Scientif. Inf.* **17**, 35.
Van Blerkom, D.: 1971, *Astrophys. J.* **166**, 343.
Van der Borght, R.: 1969, Preprint: *Vibrational Stability of Pure Helium Stars Surrounded by Pure Hydrogen Envelopes.*
Ziebarth, K.: 1970, *Astrophys. J.* **162**, 947.

DISCUSSION

Westerlund: Paczyński talked about the red supergiants being the cause of all planetary nebulae and during the coffee break I objected to it because the normal red supergiants are stars that have just evolved from blue supergiants and this would mean that the planetary nebuale would belong to the extreme Population I and hence their distribution should be quite different from what it is in the Galaxy. I understand, however, that Paczynski does not refer to those stars only so I would like to ask him to clarify the point.

Paczyński: By supergiants I do not necessarily mean MK classification Ia. I referred to red supergiants on two occasions. First, when I was discussing solar mass stars which are believed to produce planetary nebulae at the end of their evolution; these stars, according to model computations, reach very high luminosities of the order of $10^4\ L_\odot$. Such high luminosities are indeed observed in Mira variables which are obviously disc population objects and must have masses around 1.5 $M_\odot$. I am not sure whether the observers will classify Miras as red supergiants, perhaps the best is to call them Mira supergiants. They are supergiants of low mass, bolometric absolute magnitude about -5, and, most likely, the progenitors of planetary nebulae.

The second reference to red supergiants was related to very massive, very luminous red supergiants, in the 30 $M_\odot$ class. These objects should appear in very young open clusters, but in open clusters or associations, even younger than h and χ Persei, we find almost none of such red supergiants while relatively large numbers of blue supergiants are observed. And this apparent lack of red supergiants has been referred to quite frequently in the literature and different hypotheses were suggested to explain it. One possible explanation is to assume that as soon as a very massive, very luminous star approaches the red supergiant stage of evolution the envelope is lost by the star. This picture would fit rather nicely those observations that indicate that single, Population I Wolf-Rayet stars are frequently associated with ring nebulae.

So, perhaps these stars are very massive, single objects that have lost their hydrogen-rich envelopes. Ring nebulae are conventionally assigned to extreme Population I and there are so few of them because there are so few massive Population I stars.

Johnson: If you would explain the deficiency of red giants in some specific place, for example h and χ Persei, would you then expect to find there some of the ring nebulae which you want to bring forth from the red giants. I don't think that h and χ Persei has any ring nebulae or any kind of nebulae. So is there a contradiction between the deficiency argument and the lack of ring nebulae.

Paczyński: I do not think so. As far as I remember, Lindsey Smith at the Boulder Conference a few years ago, claimed that we do not observe ring nebulae around those Wolf-Rayet stars that are close to other early type stars. So, we should not expect to see nebulae in any association as there are so many stars which ionize the surrounding. However, do we observe any Wolf-Rayet stars in h and χ Persei?

General answer: No!

Johnson: It is an interesting fact that though there are OB stars in h and χ Persei there are no H II regions. In other words, it seems to be deficient in interstellar gas.

Paczyński: I would like to concentrate on the last topic of my talk, and this is the problem of the mass loss from Wolf-Rayet stars that we do observe now. And please, distinguish this mass loss from the hypothetical mass loss that has taken place in the past. These are perhaps two entirely different phenomena. This is most clearly demonstrated in the case of nuclei of planetary nebulae. In the planetary nebulae we do observe large masses of hydrogen rich gas expanding at a low velocity of about 30 km s^{-1}. At the same time Wolf-Rayet nuclei of those nebulae are losing gas at a velocity which is close to 500 km s^{-1} and it is hard to believe that the same mechanism might be responsible for these two kinds of mass loss which differ so greatly in the outflow velocity.

Van Blerkom: Can we not have the outer material decelerated by interaction with the interstellar medium?

Paczyński: No, because the gas density in planetary nebulae is so much larger than the density of interstellar medium.

Thomas: What is roughly the radius of a planetary nebula?

Paczyński: The largest is 0.6 parsecs; a typical one is about 0.2 parsecs.

Thomas: Could it be one parsec? Why not?

Paczyński: Because it is not observed.

Thomas: If you take your computations you have roughly a continuous ejection of between 10^{-5} and 10^{-6} solar masses, and in a rough way your star slows down to the order of 30–40 km s^{-1} after the amount of the mass ejected equals the mass encountered in the interstellar medium and that occurs at about one parsec.

Johnson: In the case of the best study (NGC 6888) it was 2.6 parsecs.

Thomas: No, that is the outer region.

Johnson: The shell is very thin.

Thomas: Sure, but if you start out with 1500 km s^{-1} and you take the point where it drops below 100 km s^{-1} (it is roughly a parsec or a little bit less) and thereafter it moves out very slowly because you are putting on no mass at all to the back of the big thing.

Johnson: At the present time the stuff is supposed to move at a rather constant velocity from the star to the shell which is very thin.

Thomas: 1500 km s^{-1}.

Johnson: Yes. It gets there after 2.6 parsecs. The rings really have a larger radius by at least a factor of 3 than the largest planetary radii, and if you do your calculations…

Thomas: You will find that your velocity drops to the order of less than 100 km s^{-1} after something less than one parsec. So, you assume that the planetary is at a much earlier stage of evolution. I do not see this.

Paczyński: According to the observations a typical electron density in a planetary nebula is 10^4 electrons cm^{-3}. Are you prepared to say that the density of interstellar matter…

Thomas: This is the swept up density.

https://doi.org/10.1017/S0074180900098715 Published online by Cambridge University Press

Paczyński: The planetary nebulae are not thin shells. This is almost a uniform density.

Westerlund: No!

Paczyński: I would say that in the typical young planetary nebulae, like those which have the Wolf-Rayet type nuclei, the part of the volume occupied by the gas is not much smaller than one. I mean fraction of the volume. If you go through all the planetary nebulae you find all kinds of filamentary structure.

Westerlund: No. I feel that the high densities for many planetary nebulae are not the average densities. I think (I do not remember any statistics now), that if you look at the Perek Catalogue or at my paper with Henize, you will see that a very great number of the planetary nebulae are very small spots of extremely large density. That is what is observed, there is no mean density obtained for a very large number of planetary nebulae, I am sure.

Paczyński: In the case of the few planetary nebulae associated with Wolf-Rayet nuclei, the estimates of electron density are available and those range from 10^4 upwards. I have done the computations many times, so I can tell off hand that you cannot put enough matter from the interstellar medium around the star to make the planetary nebula. Simply because there is not enough matter for that. So, you must assume that almost all the gas that you see is gas ejected from the star. Now, if you would like to eject this gas at a high velocity you need more mass from the interstellar gas to slow it down, and that would mean that almost all the nebular gas is from the interstellar medium.

So, I do not think there is any way to avoid the conclusion that in the case of the planetary nebulae with Wolf-Rayet nuclei we see two modes of mass outflow, one which took place some thousands of years ago, and another which is taking place now. Well, this is the most straightforward and simple, case, but you have to keep in mind the possibility that the same situation may be true for other Wolf-Rayet stars as well. In the case of binaries you can be quite sure that the major fraction of stellar mass was transferred to the companion and that this transfer had nothing to do with the mass loss that we do observe now. I am spending so much time on the subject because unfortunately it is quite frequent to find people confusing these two phenomena: the present mass loss which is observed now and more or less hypothetical previous mass loss. Now I am going to speak only about this mass loss which we do observe, now, the high velocity mass outflow. We do not know whether this phenomenon has an evolutionary significance, while we assume that the hypothetical previous mass loss was of a major evolutionary significance.

Thomas: Is the present information on mass loss coming from the widths of emission line profiles or do you include the violet absorption edges?

Paczyński: The observed rates of mass loss are in the range of 10^{-6} up to 10^{-4} solar masses per year.

Underhill: How do you get that? You need a density! Where do you set the density from?

Paczyński: I shall speak about this in a few minutes. At the moment we may just say that this is the value most frequently found in the literature. The velocity of outflow as observed in different objects is in the range of 500 to 2000 km s^{-1}. Let us take 1000 km s^{-1} as a typical value. This velocity is well established as the violet displaced absorption edges are observed. This velocity is independent of any model of the formation of the emission lines. In the model which I like most, and which will be presented in more detail tomorrow, the mass outflow takes place in a more or less spherically symmetric fashion, and the velocity of mass outflow may be deduced from the width of the emission lines. I know that this is a controversial model, and it is better to remember the violet displaced absorption lines, which as far as I know have never been interpreted in any other way. There is a correlation between the ionization potential of a given ion and the observed velocity of outflow. The low ionization corresponds to high velocity. This is conventionally interpreted in terms of excitation decreasing outwards.

Perhaps, the strongest argument of the outward increase of the velocity of outflow follows from the interpretation of the lines which show flat-topped profiles. As far as I remember those show the lowest excitation. And these show the largest velocities of outflow, so there can be little doubt that the gas is accelerated outwards.

Sahade: This last point is not settled yet. So, you are making an assumption.

Paczyński: No, it is not an assumption. I shall try to repeat the reasoning. We do observe the flat-topped profiles of some emission lines, and those do show the violet displaced absorption edges. The rectangular emission line profile indicates, that the line is optically thin, and is formed in the uniformly expanding region of low density. It is reasonable to expect that these lines are formed at a large distance from the star. These lines correspond to the highest expansion velocities as they are the widest of all, and they belong to the lowest ionization stages. The lines coming from the higher ionization stages are narrower implying lower velocities of expansion. I do not think it is possible to put those relatively narrow Gaussian shape profiles into a region outside of that in which the flat-topped lines are formed. I do not think there is any serious argument against the model of radially expanding envelope being accelerated outwards.

Sahade: In the case of binaries there is evidence that seems to suggest that the matter around the WR star decelerates outward; there is however, an acceleration further out which is indicated by the expanding evelope that shows in lines sensitive to dilution effect.

Underhill: We can discuss it more certainly after we have listened to the discussion of the theory of the spectra by Van Blerkom.

Paczyński: Let us consider now a radially expanding evelope that is accelerated outwards. I would like to stress a certain point that has not been emphasized in the past, but which I believe, is true. We observe that the profiles of unblended lines are symmetric. There is no indication of an occultation effect. It means that the line emitting region must be at least two or three times larger than the star. We have to assume that all the lines are formed in a region large compared to the star. At the same time we see the correlation between the ionization potential of a given element and the line width, i.e. the outflow velocity. It means that gas is being accelerated within that region in which the lines are emitted. Whatever is the force that pushes the gas outwards, it must operate within the line emitting region, and this is very important.

One more thing. In order to derive the mass loss rate we need the velocity of outflow, and this is fairly reliable; we also need the density of matter and the radius at the point where the density is estimated. The value of 10^{-4} solar masses per year is obtained of one takes the numbers given by Castor and Van Blerkom in their model of WN star envelopes. If someone does not like the high value of the rate of mass loss, this may be lowered a little. But I do not think that it may be lowered by more than a factor of ten in the particular case considered by Castor and Van Blerkom. If some other stars have less dense envelopes they may have smaller rates of mass loss. This lower density may be indicated by the lack of any bump in the Pickering decrement in some stars.

In any case it would be extremely difficult to have less than 10^{-6} solar masses per year, and that is much more than the rate observed in Of stars. This is most clearly demonstrated by the comparison of the WR and Of type sectra. These are similar in the ultraviolet. However, when you consider the visual spectra, you see nothing in the Of stars that would resemble the rich emission line spectra of WR stars.

The most obvious reason for this difference is the large difference in the gas density in the expanding envelopes. Wolf-Rayet envelopes are much denser, and as the outflow velocities are about the same, the rates of mass loss must be considerably higher in the Wolf-Rayet stars. What may be the mechanism for the observed mass loss from WR stars? Limber suggested the rational instability. This cannot explain the origin of the violet displaced absorption edges. In Limber's model the absorption should occur at the centre of the emission line, not at the edge. Another possibility is some kind of a hot corona, like the one we have in the Sun. By the corona I mean that part of the extended atmosphere where the gradient of the gas pressure is important and large enough to push the matter outwards, just as it is the case with the solar corona. The expansion velocity of the Wolf-Rayet envelope is of the order of 1000 km s^{-1}, and that implies a very high temperature, of the order of 10^7 K for the corona, if the gradient of the gas pressure is to be important. And this is independent of the gas density. We know from the observations that the acceleration takes place within the line emitting region, where the electron temperature is of the order of 10^5 K as indicated by the observed ionization equilibrium.

Therefore, we have here the discrepancy of two orders of magnitude in the temperature. Unless you are prepared to invent an exceedingly complicated model with dense low temperature clouds in which the lines are formed, and very hot intercloud medium which accelerates those clouds, you cannot explain the observed mass outflow by any mechanism involving the gas pressure.

Thomas: Without arguing for or against 10^7, how do you know it cannot be 10^6? Do you have enough observations to really exclude coronal ions like in the Sun?

Paczyński: If you have 10^6, how would you want to preserve the relatively low ionization?

Thomas: I do not have a uniform atmosphere, of course.

Paczyński: So you are in the position I have mentioned. You have to introduce a hot continuous medium with cool dense clouds.

Thomas: All I have to do is to have a radial distribution.

Paczyński: No.

https://doi.org/10.1017/S0074180900098715 Published online by Cambridge University Press

Underhill: What is wrong with that? The observations push you there.

Paczyński: Only if you are willing to accept such a complicated picture.

Thomas: Why do I need it?

Paczyński: Because otherwise you will not be able to produce lines in the same region in which the matter is being accelerated.

Thomas: I do not think we can claim consistency until we have solved the fluid mechanical problems coupled to the excitation better than we have.

Paczyński: I do not understand. You mean, I cannot have in the same region 10^7 degrees?

Thomas: No. You are saying that you cannot have the velocity excitation correlation that you observe and at the same time have anywhere in the atmosphere, regions at a million degrees. To me it is far more critical to ask observationally whether I can exclude a million degrees until such time as you have solved the fluid mechanical problem, to really show the correlation between velocity and excitation under some kind of a generalized stellar wind solution.

Paczyński: I am not sure I understand your remark.

Kuhi: I do not think there is any observational evidence for any coronal lines of any kind in a Wolf-Rayet star.

Thomas: Do you have rocket spectra?

Kuhi: No, I am talking about the optical lines identified in some symbiotic stars.

Thomas: Sure. In the solar optical spectra, whatever you see, you would not have seen it from a long way away.

Underhill: You see the solar coronal lines in one or two symbiotic stars but I believe you need not only very high electron temperatures but also low electron density, 10^9, or so.

Thomas: That is a quite reasonable thing.

Underhill: That is what we are talking about. When we, stellar observers, talk about coronal lines in the visible...

Thomas: Is there any evidence yet? Do I have enough rocket observations to exclude anything ionized higher than O VI?

Underhill: Not in the UV.

Thomas: Then my mind is open. That is all.

Underhill: So we have to refer our evidence to the forbidden lines.

Thomas: That is right.

Paczyński: You may look at the equation of motion for gas dynamics, and you will find that you have terms that correspond to the gas pressure, and kinetic energy of the gas. At a large distance from the star the gas pressure is negligible and you may estimate the kinetic energy from the observed velocity of outflow. Because of energy conservation, this kinetic energy has to come from somewhere. If you want to use the gas pressure to accelerate the matter you have to store this huge kinetic energy as the internal energy of gas in the region close to the star. No matter how complicated a picture you are going to introduce, you cannot avoid this problem.

Johnson: If you have 10^7 degrees in an atmosphere as large as the Wolf-Rayet star, and as dense, you should see it as an X-ray source, and I think so far these stars have not been found as X-ray sources although several people have written papers making tentative identifications but never a completely convincing one so far. There are papers by Wallerstein and by Stecher. It was a good idea.

Underhill: The amount of X-rays in those stars will never get to us from what we think are reasonable distances for these stars.

Kuhi: With regard to the paper by Wallerstein about X-ray sources, I do not believe at all in Wolf-Rayet stars as X-ray sources.

Paczyński: The kinetic energy flow associated with the observed mass loss from WR stars is approximately one per cent of the stellar luminosity. It is considerable, but it is not a dominant fraction of the total energy flux.

Thomas: It seems to me consistent to have mechanical energy a couple of per cent of the radiative.

Paczyński: Yes, but this indicates the existence of an efficient mechanism for transformation of the thermal energy into kinetic energy.

Apparently, not everybody is convinced that we may rule out the gas pressure as the agent causing the mass outflow. Let us consider how the radiation pressure may act on matter and cause its outflow: these are the radiation pressure in the lines and in the continuum. According to Lucy and Solomon, the radiation pressure in resonance lines is responsible for the mass outflow from the Of stars. They put a very strict upper limit to the rate of mass loss that may be achieved in their model. It is given as $-dM/dt \leqslant L/c^2$. This is independent of the velocity of outflow. The detailed computations made by Lucy and Solomon are consistent with this upper limit. It is unfortunate, that while using the resonance lines we may take out from the radiation field momentum rather than energy.

Morton: But many absorption lines participate in the momentum transfer.

Paczyński: If you have a few resonance lines you may increase the rate by a factor of few.

Morton: There is one point in that connection. Their theory predicts that usually only one absorption line dominates at a given temperature class, but the observations suggest that as many as 5 or 10 lines could be important so that there is a difference there between their very simple theory and the observations. A factor 10 correction may be in order.

Paczyński: This means that perhaps we may push the rate of mass outflow from 10^{-8} up to 10^{-7} solar masses per year in a star as luminous as Wolf-Rayet stars are believed to be, i.e. about 10^5 or 2×10^5 solar luminosities. Let us consider now the radiation pressure in the continuum. As far as I remember it was originally suggested by Rublev. He thought that Wolf-Rayet stars are so luminous that the radiation pressure on free electrons is responsible for the observed mass loss. The radiation pressure on free electrons can balance the gravity of a 10 solar mass star if the luminosity exceeds about 4×10^5 or 6×10^5 solar luminosities, depending on the chemical composition. It is not much more than the observed luminosity of a typical Wolf-Rayet star. From the observational point of view, considering the uncertainty in the estimates of masses and luminosities of those stars, we cannot be entirely sure that the luminosity is not higher than the critical value given above. However, if you look into the problem more closely from the theoretical point of view, you will find that it is practically impossible to achieve such a situation. First of all, none of the theoretical models that had been computed with the proper boundary conditions at the stellar surface ever achieved the surface luminosity higher than the critical. There is, in fact, a very good safety valve within the star: every time the luminosity approaches the critical value the star expands and the luminosity is lowered and the critical value is not reached.

Smith: I do not quite understand the safety mechanism.

Paczyński: Let us suppose that we managed to exceed the critical luminosity somewhere within the star. It may happen, for example, during the helium flash. And now we want this high heat flux to diffuse through the star to the surface. The flux heats up the matter on its way, the gas expands, the gas pressure is lowered, and the energy output is lowered too. We may look on to this problem from a different direction too. The mass outflow from the Wolf-Rayet stars continues for at least 10^2 yrs. and, therefore, it is more or less stationary. We may integrate the equations of stellar structure from the surface inwards, assuming there is a stationary outflow of mass. It turns out that if the radiation pressure in the continuum is responsible for mass outflow, the so called critical point of the flow is below the photosphere, at a large optical depth. And then it turns out that it is practically impossible to fit such an envelope to the stellar interior that is assumed to be in a hydrostatic equilibrium. At least nobody has succeeded so far in doing this in a consistent manner and it is almost certainly impossible, if the luminosity exceeds the critical value.

Morton: Do you have such a problem whether or not you were above the critical luminosity?

Paczyński: As far as I know no case was found so far. Perhaps the matching may be possible for the luminosity below the critical value.

Morton: That argument tells us that some of the mass outflow may not be included in our current theory.

Paczyński: The mass outflow is driven by the radiation pressure in the continuum. In principle, if a star had a luminosity above the critical value the mass would flow out. However, in such a case the whole star would be blown out on a dynamical time scale. It is so, because it is not possible to match a solution for the outflowing envelope with the stellar interior that is in hydrostatic equilibrium. I believe that simple minded models with the radiation pressure in the continuum as a driving force are out of question. I feel now that we have almost proven that Wolf-Rayet stars cannot exist, and I believe that it is time for me to stop!

Conti: I have heard several allusions to a Wolf-Rayet star observed for the last 2000 yrs. Could someone tell me which star this is?

Thomas: Gamma Velorum is in the Almagest.

Conti: But the O star dominates the spectrum; the Wolf-Rayet star is much fainter.

Thomas: You mean that one of my standard illusions is being shattered? You mean you can vary Gamma Velorum by a factor of 2 over the 2000 yrs history?

Conti: You can vary it by a factor of 2 and you would not change the visual magnitude.

Van Blerkom: Can you say what was the dynamical time scale?

Paczyński: It is essentially the same as the pulsation time scale, or the rotation time scale (i.e. a few hours or days). I should like to make clear that the theory is not yet in a position to prove convincingly what kind of abundance anomalies there should be present in Wolf-Rayet stars. We may expect hydrogen deficiency in those stars that have undergone mass exchange. It is difficult to make a quantitative statement because very little is known about semi-convection. The deficiency may be by a factor of 2, which would not be detectable, or by a factor of 10, which perhaps might be detectable, or even more than a factor of 10, if a subsequent mass loss, the one we do observe now, has removed all the hydrogen-rich envelope. Well, as this point is not clear I would like to concentrate on the carbon-nitrogen-oxygen abundances. We know that cosmic abundances are such that oxygen is more abundant than carbon, and nitrogen is less abundant than carbon. When the matter is processed through the CNO cycle these elements are redistributed among themselves. First, carbon is almost entirely transformed into nitrogen. The equilibrium abundance ratio is of the order of 1 to 100. Then on a much slower time scale oxygen is transformed into nitrogen. The first step takes place when less than one per cent of hydrogen is burned into helium. It means that at the very beginning of hydrogen burning almost all carbon is transformed into nitrogen. The second process is much slower. Almost all hydrogen has to be burnt into helium before oxygen is transformed into nitrogen. The zero-age main sequence star is believed to be chemically homogeneous, and to have cosmic abundances. Therefore, carbon, nitrogen, and oxygen abundances are in the ratio of 3:1:9 approximately. Combined they contribute about one half of the total mass of the elements heavier than helium. At the end of the main sequence life time, almost all carbon is transformed into nitrogen within the inner half of the stellar mass, i.e. within the region considerably larger than the convective core. The outer half of the stellar mass has the original cosmic abundances. Within the convective core all hydrogen is burnt into helium, and not only carbon, but also oxygen are transformed into nitrogen. Therefore, within the helium core we have a lot of nitrogen.

Now we enter the core helium burning phase. Within the helium burning region all nitrogen that has been left by the CNO cycle is transformed into oxygen. Also, new carbon and oxygen is produced from helium. Therefore, in the region where helium is burning we have no nitrogen, and plenty of oxygen and carbon, and we are closer to the original abundance ratios.

Let me clarify the problem of the time variations of the chemical composition by making a schematic table. At the top we have the initial abundances (by mass), at the end we have the products of helium burning (see Table I).

The abundance variations shown in the Table may be interpreted either as the variations from the stellar surface (top) to the helium exhausted stellar core (bottom), or as the time variations within the stellar core. In order to show the abundance anomalies at the stellar surface we have to remove a lot of mass from the envelope, or we have to mix the stellar matter thoroughly. It is important that as the

TABLE I

Variation of chemical composition with stellar evolution

Hydrogen	Helium	Carbon	Nitrogen	Oxygen	Heavy Elements
0.69	0.27	0.006	0.002	0.018	0.014
0.68	0.28	0.000	0.008	0.018	0.014
0.00	0.960	0.001	0.024	0.001	0.014
0.0	0.959	0.002	0.000	0.025	0.014
0.0	0.5	0.40	0.0	0.08	0.02
0.0	0.0	0.48	0.0	0.48	0.04

nucleosynthesis progresses we have either mostly carbon plus oxygen or we have mostly nitrogen. The carbon to nitrogen ratio changes by many orders of magnitude, and the change is always rapid. It is like the appearance of the Wolf-Rayet type spectra, where either carbon and oxygen lines or nitrogen lines are prominent.

Underhill: But you still have to get these from the center out to the atmosphere.

Paczyński: You have either to mix it or to remove the surface layers.

Underhill: And how to do this nobody has ever explained.

Paczyński: If we have a binary then mass exchange removes the surface layers. Once you remove more than one half of the stellar mass, and this is easy to do in all the models with mass exchange, then you are likely to see the products of the carbon-nitrogen-oxygen cycle, and you are likely to find some anomalies in the abundance ratio of these three elements. If hydrogen is very strongly under abundant then you may expect to see much more nitrogen than either carbon or oxygen. If you have no hydrogen and you see more carbon and oxygen than nitrogen, it means that this carbon and oxygen are the products of helium burning. No matter how difficult it may seem to get the products of helium burning to the surface, that is what must have happened. May I mention that R Coronae Borealis and the so-called helium stars are observed realities. In these stars hydrogen is underabundant by a factor of 10^5, and carbon is considerably overabundant. In these stars we see the products of helium burning mixed up to the surface. We do not know how the star is able to do this, but it obviously does so. The problem of mass removal or mixing is not solved for the Wolf-Rayet stars. Nevertheless we should be aware that the C:N:O abundance ratios are likely to be anomalous if either mass loss or mixing took place within a star.

Wood: I wonder about the universal Fermi interactions. What is the nature of their effect on your calculations?

Paczyński: There is a hypothesis that all the particles, no matter what their nature is, interact weakly with each other, just as they interact gravitationally. It is assumed that the constant which determines the strength of these interactions is the same for all the particles. Neutrino emission is a result of these interactions. It is very strong when the temperature and density are high. This is a different neutrino emission process than the one that appears in normal nuclear reactions. The latter will hopefully be detected from the Sun directly. It comes from the nuclear reactions which are reasonably well studied and their existence is beyond any doubt. However, if you consider the neutrino emission due to the universal Fermi interactions you find that it is not yet confirmed by any direct experiments.

Wood: What is the effect on normal reaction rates if these neutrino fluxes exist?

Paczyński: Neutrinos escape freely from the stellar interior. They carry out energy and, therefore, they act as a cooling agent. Strangely enough, if we have this cooling agent the star becomes hotter, just because it has to shrink more to compensate for the additional energy losses. In the carbon burning phase the amount of energy emitted in the form of neutrinos is a factor of 100 larger than the photon luminosity of the star. As a result the evolutionary life time gets shorter by a factor of 100 or so, and that is why the neutrino emission affects the late stages of stellar evolution so much.

Wood: These universal Fermi interactions are effective throughout all temperatures regimes?

Paczyński: Yes, but the phenomenon becomes really important once you are in the helium exhausted region of a star.

Thomas: What I want to do is to put together what it seems to me you have come up with, and to contrast that with what we had some time ago. I want to do this on the basis of the atmospheric framework that I proposed the first day: a classical photosphere, a non-classical photosphere, a chromosphere, a corona, and an exosphere. In the latter we have a mass transfer; in the corona, a momentum transfer; in the chromosphere, an energy transfer; in the non-classical photosphere, we have only population effects; and lowest there is the classical model with neither transfer nor population effects. It seems to me that you are calling for a mass loss ranging from 10^{-4} to 10^{-8}, considering the various estimates that have been made. But these various estimates are strongly dependent upon the momentum transfer; and here I worry both about the density and the density gradient, and I have a choice as to the kind of momentum transfer or support possible. We can have wholly a kinetic temperature support or some kind of a random macroscopic velocity; or an expansion. If we have an expansion, then the velocity of expansion has to be like ϱ/R^2. So we have these three possibilities. And in the energy situation, I must have somehow an energy supply sufficient to support the spectrum, and it seems to me that this must produce ionization exceeding O VI. I think that is the highest excitation we have seen in the visible spectrum. That is all one really knows. When we come down to the classical photosphere, it appears that we had better have the effective temperature in excess of 30000 degrees, if indeed we have a helium star. Is that correct?

Paczyński: That is provisional.

Thomas: All right. But it seems to me that we have trouble with the helium star, because various people continue to talk about something as low as 20 or 25 thousand degrees for the photosphere temperature.

Paczyński: The trouble with the helium stars is that they are too hot.

Thomas: But this kind of a minimum effective temperature is some kind of a concensus that people are talking about.

Paczyński: Yes, this is purely observational, not theoretical.

Thomas: All right, if you have a pure helium star, what do you like as a minimum? I thought you said one day a minimum effective temperature was 30000 degrees.

Paczyński: Yes, but from an observational point of view .Theoretically you cannot get an effective temperature of a massive helium star lower than one hundred thousand degrees if the star is in hydrostatic equilibrium and has no hydrogen envelope.

Thomas: Let me continue the empirical evolution in thinking. A long time ago I argued vigorously against an outward increase in expansion velocity, arguing rather for a decrease. And I argued for increase in ionization outward, rather than an outward decrease, with those two things observationally coupled. I argued for a high kinetic temperature in the outer atmosphere and probably an atmosphere supported either by a compound of material both moving up and moving down, or some kind of a turbulence. I never liked this word so I never liked the suggestion. The alternative if you wanted only a high kinetic temperature, was something near 10^7 degrees and I rejected that. I did not believe it. I have no basis for rejecting it, but it did seem to me an awfully big temperature. The argument for the suggestion that the degree of excitation and ionization increase outwards rather than decreasing was based on analogy with my thinking in the solar atmosphere. At that time I suggested the spicules were heating the solar atmosphere; and then observationally one had the interesting situation that the spicular velocity decreased outwards, from an original ejection, possibly a deceleration under gravity. At that time it worked reasonably well so far as explaining the solar atmosphere, because I would have an initial rise in ionization and excitation coming from the introduction of a mechanical heating, and then an eventual flow coming from the decrease in the spicular velocity. At that time there were two theories of heating mechanisms: one the spicular kind of thing; the other, the convective acoustic waves sort of thing. The accoustic waves seemed more realistic than the spicules and were generally adopted for the overall atmosphere. The relation between spicules and non-uniform heating of the chromosphere is, however, still a strong item of question. The simple acoustic wave mechanism of some years ago has now given the way to a discussion of how you avoid heating the lowest parts of the atmosphere; and it now appears that the production of aerodynamic oscillations occur much deeper in the atmosphere than the regional "surface bubbling" picture, and are stored most of them not propagating in the region of the temperature minimum. Then we get a certain amount of mechanical energy propagating upwards from the region of the temperature minimum outwards. Now I emphasize this picture for the following reason: Instead of talking about the production of the heating mechanism and propagation of the pressure waves at $\tau \sim 0.8$, what one does is to put in a kind of instability deep down in the solar atmosphere, and the mechanisms of the instability now being looked at include exactly also the same ones as cause the Cepheid variation, the opacity variation, the gradient in the ionization, etc. So the situation as regards origin of aerodynamic energy becomes much more similar between solar type and other types of stars. Now, what is the other evolution? It is simply that instead of having the corona beginning at a very great height (you remember people were talking of 50000 km, 20 years ago, then 5–10000 km). it now appears that at about 2000 km we have a rise to coronal conditions.

Chromospheric conditions begin already at something like 1000 km. So the heating and an abrupt rise to excitation conditions corresponding to 10^6 K in the atmosphere occur very very soon, within a couple of thousand kilometers from the surface. They occur within a few hundred km from where the propagation of aerodynamic energy sets in, at the temperature minimum. So with this model then what do we have for the picture? Over the majority of the non-classical atmospheres we have all corona and exosphere; the chromosphere is a very small region.

The second thing that happens in this same region, the region of the very abrupt rise, is that we no longer have any kind of a uniform atmosphere. In these inhomogeneities we find differences in electron temperature, by factors of two and three, ranging between something less than a million degrees to something near four and five million degrees. This reflects the energy balance in the atmosphere. Then we must admit the existence of an outflow of mass from the Sun in the form of the solar wind, with the driving mechanism being just this hot corona bounded by the interstellar medium.

Return now to the problem of distribution of ionization in the atmosphere of the WR star, how rapidly it falls off and how rapidly it increases. It seems to me that Kuhi has presented a very strong case for the picture that over a great extent of the WR atmosphere it looks as though one has a decrease of ionization outward, and an increase in velocity field outward, but the absolute level of kinetic temperature is very uncertain.

We have talked about 10^5 K in the region of line formation. We have talked about 30000 K for the electron temperature in the lower atmospheric region where it seems to me that we are more and more coming to the same general picture for the WR stars as for the Sun. We see the photosphere in the continuum. We have a very small region where T_e rises abruptly. Then we have an extended corona-exosphere. The difference between stars lies in how much mass is in these various regions. Thus, how easy it is and what you need to observe the several regions, varies from stellar type to stellar type. The same holds in the exosphere and mass outflow. For all very hot stars apparently we measure outward velocities of the order of a thousand kilometers per second. Not a couple of hundred but of the order of a thousand kilometers per second; some non-zero mass loss.

It is not an extended atmosphere in the sense of that height where you get the heating, divided by the radius of the star, is very large. On the contrary, it is very small for the Sun, and not obviously large for the WR stars. It is an extended atmosphere phenomenon in that the extent of the whole atmosphere out to the interstellar medium, is large.

So when I ask details of helium photosphere, I am clamped down here in a very small region. When I ask for details of mass loss and momentum transfer I have an enormous region, even over the solar atmosphere. Now what we get from Paczyński's talk yesterday is one strong thing. It is not clear what is the mechanism for getting these high velocities, nor mass loss for the hot stars. Equally obviously we have trouble with the mechanisms of heating; that has not even been discussed, because we are not involved with it. But I do have trouble with the high velocities.

Paczyński: With the rate of mass loss?

Thomas: I have trouble with the mass loss rate but I also have trouble with getting these high velocities. You discarded the pressure mechanism. If I am going to have any kind of a stellar wind argument, any kind of a pressure mechanism, the upper limit in velocity is going to be set by $(2/\gamma - 1)^{1/2}$ times the sound velocity.

If I want to ignore interchange between internal energy and macroscopic energy, the gamma is about five thirds, and the velocity is about $\sqrt{3}$ times the sound velocity, a very small number. If I want to tap the internal reservoir and say I would start out, for example, with all ionized helium and then expand the gas and let the helium recombine, gamma goes towards one and you can pick up about a hundred kilometers per second. If you want to go all to oxygen so the whole atmosphere is oxygen and take like a thousand volts for the ionization of oxygen, you can increase your velocities somewhat more. But you have to go to very heavy elements in your atmosphere before you can get something like a thousand kilometers expansion velocity.

The other alternative is to go to some catastrophic thing like the supernovae or the novae kind of explosion. That is something deep inside. It seems very hard to see how you can have a continuous catastrophe to provide this kind of ejection. So the real problem here, in addition to specifying what excitation level I really have, which I do not think you are going to know till you have rocket UV observations, is really the corona and the exosphere in terms of mass loss and momentum transfer.

I have always believed in Kopal and Mrs. Shapley's arguments as to the kind of density gradients you have in the outer atmosphere.

Now Su-Shu Huang has a paper which shakes the foundations for this. But it is an observational point as to what density gradient do we need here, just as we have to find out what excitation level we need here, just as the argument related to the mass loss is very fundamental. It is indeed true that all these mechanical transfer effects represent a very small fraction of the radiation energy of the star. But what you should compare it to is the luminosity of the star that can be tapped. I cannot just dam up the radiation field and use all of that to expel material from the star. So, saying it is a small value does not help. I must have a good model, energetically and thermodynamically, to know how much energy I can tap. I suggest that that is the real phenomenon the Wolf-Rayet star is: find out what we mean by the corona, the region where we have a non-thermal supported density gradient, and the exosphere, where we have mass ejection, although we do not know where it comes from.

Underhill: You mean the exosphere motions are just details of the kinetic energy distribution.

Thomas: In some way it is very vague. All I say is look at the evolution of our thinking here. We are pushing back close to the surface of the star all the increasing excitation phenomena.

Is O VI really the highest excitation in the WR atmosphere? I just do not believe it, but then you come down to the real problem you suggested yesterday, if the emission lines occupy such a big volume relative to the photospheric radius, how do I get that volume maintained involving mass loss.

Paczyński: I would like to clear up some misunderstanding about the 10^{-2} times the luminosity. I think that this number is small in the sense that the total luminosity of the star is predominantly in the form of radiation. This additional flux of kinetic energy does not affect the bolometric magnitude of the star. However, this number is very big if you want to explain it. We cannot find so far an adequate mechanism that could convert one percent of luminosity into kinetic energy flux. So I do not want to deny the importance of this.

Thomas: I was not arguing with you, I was just stressing again may be what you have stressed in another way. That it looks as though you have a big reservoir but that you do not. And I agree with you that it does not look as though it can bother much the bolometric magnitude, atleast that is the way it looks now. But all the recent work on IR bolometric corrections do bother me.

Underhill: The real problem, it seems to me, is to get a close estimate of the actual plasmas. What you are saying is that theoretically it is very difficult, if not impossible, with present models, to find a mechanism that will transfer enough of the available energy to give the kinetic energy that is carried off if the mass loss is significantly greater than 10^{-8} solar masses per year.

Thomas: Not in this. We are talking about velocity per particle.

Underhill: I am not bothering about velocity per particle. We observe it. We lose so much energy. We have decided on really very uncertain grounds that the mass loss is considerably larger upto 10^{-4} rather than 10^{-8} solar masses per year. Now it is quite conceivable that we are interpreting our observations of line strengths quite incorrectly. We see material leaving the star but we have really no estimate of the density of that material. We make somewhat plausible arguments about the density and size of the sphere possible but if those are out by a couple of factors of ten, then we are in trouble. If we cannot make a sufficiently efficient mechanism by any conceivable physical process, I would go back to our estimate of density and size of sphere because this is the region where we are indeed uncertain.

Thomas: This is the point. I do not care what the actual value of the mass loss is. To get this kind of an energy per particle is awfully hard.

Paczyński: When the rate of mass loss is as low as 10^{-8} solar masses per year, the radiation pressure in resonance lines probably can accelerate matter to these velocities. There is no fundamental difficulty as long as you keep the rate of mass loss at such a low value.

Morton: I think we have at least one feasible mechanism for hot stars, that will give these velocities. We have enough problems without worrying about how to get the high velocities. In the observational determination of high mass loss rates, we must remember that we measure the velocity in a few particular ions and then make some crude estimates about the relative populations of the other ion states and other elements which are not observed.