SECTION VIII

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SUMMARY OF PROBLEMS AND CONCLUSIONS ON THE NATURE AND PHYSICAL STRUCTURE OF WOLF-RAYET STARS

ANNE B. UNDERHILL
Goddard Space Flight Center, NASA, Greenbelt, Md., U.S.A.

1. Introduction

The Wolf-Rayet stars were the subject of a symposium held at the Joint Institute for Laboratory Astrophysics in Boulder, Colorado in June 1968. Here there was considerable discussion about whether one should talk about a class of objects called 'Wolf-Rayet stars' or whether it was more appropriate to talk about something called the 'Wolf-Rayet phenomenon' (Gebbie and Thomas, 1968). The point of view taken in this review is that it is more advantageous for obtaining an understanding of Wolf-Rayet objects to collate the available material under the broad category *Wolf-Rayet phenomenon* than to attempt to demonstrate that there is a homogeneous set of stars which can be called Wolf-Rayet stars and which occupy a significant stage in the evolution of stars, this stage being traversed routinely by all stars of a certain range of mass.

On the average, galactic Wolf-Rayet stars in binary systems have masses in the range 6 to 15 solar masses. Normally, hydrogen-burning stars with such masses would have a B-type spectrum or, if evolved somewhat, the spectrum of a late-type giant. Wolf-Rayet objects do not have such spectra, by definition. Why is this so? What special factor produces the Wolf-Rayet spectrum? These are questions to which we are seeking answers.

The words 'Wolf-Rayet' refer to a particular type of spectrum which Wolf and Rayet (1867) were the first to recognize as they surveyed the spectra of stars in Cygnus with a visual spectroscope. Since their original discovery, about 127 Wolf-Rayet stars have been recognized in our galaxy (see Smith, 1968a, for a catalogue). Fifty-eight are known in the Large Magellanic Cloud and two in the Small Magellanic Cloud (Smith, 1968b). Twenty-five objects with Wolf-Rayet-like spectra are now known in M33 (Wray and Corso, 1971).

At the 1968 conference, a Wolf-Rayet spectrum was defined to have the following characteristics (Thomas, 1968):

(1) The spectrum consists of emission lines on a continuous spectrum which has an energy distribution rather like that of an O or a B star.

(2) A few absorption lines may occur as shortward displaced satellites on the edges of some of the emission lines. There is no general absorption-line spectrum as known for normal spectral types. If such a spectrum is seen, it is attributed to a companion star.

(3) The emission lines seen in any one object represent a wide range of excitation

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and ionization, the excitation of the line spectrum generally being much higher than that estimated from the shape of the continuous spectrum.

(4) Most of the emission lines are broad with widths corresponding to hundreds to thousand km s^{-1}. The widths are not the same for all lines in any one stellar spectrum.

(5) Most of the spectra fall into two groups: (1) the WC stars in which the lines from ions of C and O dominate, and (2) the WN stars in which the lines from ions of N dominate. Both groups show strong lines of He II.

The galactic Wolf-Rayet stars are the prototypes of the class. Some central stars of planetary nebulae have a spectrum which satisfies the five criteria given above. The optical spectrum of the X-ray source Sco X-1 (Sandage *et al.*, 1966) and of WX Cen (Eggen *et al.*, 1968), which may be the X-ray source Cen X-2, have similar characteristics. In fact the spectrum of WX Cen looks quite like that of a WN 7 star except that He I is weak and H is strong. All objects with spectra satisfying criteria 1 to 5 may be classed as Wolf-Rayet objects. This review will attempt to indicate the nature of these objects. Criterion 2 excludes Of stars, Be stars and stars like P Cygni from the class 'Wolf-Rayet object'. The behavior of the spectra of Of, Be and P Cygni stars, however, has much in common with that of Wolf-Rayet objects as defined above and illuminates the physical theory required.

2. Wolf-Rayet Objects and the HR Diagram

It is true for stars with predominantly absorption-line spectra that the spectral type, when it includes an indication of luminosity, serves as a shorthand notation for the position of the star in the HR diagram. The spectral type can be correlated uniquely with effective temperature and the visual absolute magnitude may be used to indicate the radius of the star. If the stellar mass is known and the composition, normal or otherwise, is specified, one can transform the position in the HR diagram into a stage of evolution. The transformation from the M_V and spectral type diagram to the luminosity-effective temperature diagram to stage of evolution is done by means of model atmospheres and synthetic spectra and by comparing with computed evolutionary tracks which result from models of stellar structure and energy generation. The Wolf-Rayet objects are puzzling because no unambiguous set of rules has yet been established about how to do the transformations starting from a spectral type, the visual absolute magnitude and the mass of the star. One can argue from the visual absolute magnitude and the masses of stars in binary systems that the interiors of these stars can be represented by nominally pure helium stars burning helium.

The Wolf-Rayet stars can be put fairly consistently into a few spectral types which are defined by explicit stipulations concerning the relative intensities of some emission lines and the widths of the emission lines. The visual absolute magnitudes of the stars can be found from consideration of membership in groups of stars which contain stars of known luminosity and from consideration of the H II regions which surround some 56 Wolf-Rayet stars. A recent study of this subject by Crampton (1971) gives the following visual absolute magnitudes (see Table I).

TABLE I

Visual absolute magnitudes of Wolf-Rayet stars

Spectral Type	M_v	No. Stars	Spectral Type	M_v	No. Stars
WN3, 4, 5	−3.7	7	WC5, 6	−3.6	3
WN6	−4.8	7	WC7	−4.4	3
WN7	−6.5	4	WC8	−5.0	1
WN8	−5.0	2			

It is interesting that in each sequence there is a trend of visual luminosity with spectral type. One of the problems concerning the nature of Wolf-Rayet stars is to decipher what this trend means. Is it a reflection of differing bolometric corrections? Where do the central stars of planetary nebulae with Wolf-Rayet-like spectra fall on this sequence?

A further point that requires clarification, in view of the fact that the emission lines seen in Wolf-Rayet spectra seem to be the optical result of a hot plasma generated by mechanical effects, is how the bolometric correction that should be defined. The bolometric correction classically is a measure of that part of the radiation field from a star which does not fall into the visual band. It is an expression of the law of conservation of energy when the simplifying assumption is made that energy appears only as radiation. In objects like Wolf-Rayet stars, however, part of the observed radiation field is a consequence of mechanical effects.

Radiative and mechanical energy may be conserved within a system of two or more objects, for instance the two stars of a binary system or one star which is shedding material into the circumstellar medium and thus interacting with the circumstellar medium. In such cases a careful look must be taken at the meaning assigned to a bolometric correction and subsequently to the visual absolute magnitude. Certainly the meaning is not the same as for normal absorption-line stars. One wonders whether an effective temperature estimated from the shape of the continuous spectrum over a short region in the visible spectrum represents accurately enough the total rate of energy generation and emission by the star. The transformations which are used to develop a picture of stellar evolution from the positions of normal stars in the HR diagram would seem to be of doubtful validity for Wolf-Rayet objects.

The magnitude of the problem can be seen by the following simple calculation. If the bolometric correction of a Wolf-Rayet star is −3.0 and the visual absolute magnitude is −4.4, the radiative luminosity of the star is $6.3 \times 10^4\ L_\odot$. Such a star radiates at a rate of about 2.4×10^{38} erg s^{-1}. Suppose that the particle density in the expanding atmosphere is 2×10^{12}, which is a value like that in an early type supergiant atmosphere, and that the average molecular weight is 0.5. Then the density is about 1.6×10^{-12} g cm^{-3}. If each cubic centimeter moves at 1000 km s^{-1}, the kinetic energy of each cubic centimeter is 0.8×10^4 erg. Suppose that the effective outer radius of the dense expanding atmosphere is 40 $R_\odot$. The kinetic energy transported each second across this boundary is then 8.5×10^{37} erg s^{-1}. The ratio of the kinetic energy lost per

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second from the star to the radiative energy emitted per second is then about 0.35. These numbers are rough estimates and they do not take into account the excitation and ionization energy contained in the ejected matter.

This example makes it clear that galactic Wolf-Rayet stars may radiate mechanical energy at a rate comparable to the rate at which they lose energy in the form of radiation. In the case of a central star of a planetary nebula, if the density of the expanding atmosphere is an order of magnitude smaller than that of a galactic Wolf-Rayet star, about the same result may be attained because the luminosity of such a star is at least a factor ten smaller than that of a galactic Wolf-Rayet star. For precise consideration of the *total* energy radiated by a Wolf-Rayet star, the amount of energy leaving the Wolf-Rayet star as kinetic and excitation energy should be included in the sum. The change in the bolometric correction to take account of a factor x in the total energy emitted in all forms is $2.5 \log x$. This factor can easily become significant in comparison to the bolometric correction estimated from purely radiative model atmospheres.

Those central stars of planetary nebulae which have Wolf-Rayet-like spectra have visual absolute magnitudes fainter than about -3; see Webster (1969) and Smith and Aller (1971). Their masses are considered to be of the order of one solar mass. These stars are smaller and less massive than normal Wolf-Rayet stars. According to Gatewood and Sofia (1968), the X-ray star Sco X-1 is an evolved compact object of the nature of a white dwarf. The visual absolute magnitude lies in the range $+6$ to $+7$ and there is no evidence of a companion star. Mook and Hiltner (1970) have reviewed all the available information about the distance of Sco X-1. If it is more distant than Gatewood and Sofia estimate, the visual absolute magnitude is brighter than given here. Gatewood and Sofia suggest that the star once had a mass in excess of 10 solar masses and that it has shed sufficient mass to become an evolved compact object. Nevertheless, the spectrum of the star has characteristics similar to those of Wolf-Rayet objects and to old novae. The emission-line spectrum varies in a matter of days (Sandage *et al.*, 1966) and there is some suggestion of a shortward displaced absorption component associated with C III 4650.

These observations are cited to demonstrate that the factors causing a Wolf-Rayet-like spectrum to appear are not limited to one area in the HR diagram. In fact the position of an object in the HR diagram does not seem to be a constraining factor on causing a Wolf-Rayet spectrum. It is, however, clear that Wolf-Rayet-like emission lines are seen only in objects which have blue continua rather like the continua of B and O stars. This fact together with the fact that lines from the second, third and fourth ions of carbon, nitrogen and oxygen are seen indicates that one is observing radiation from a hot plasma with an electron temperature greater than 3×10^4 K.

The mechanism for heating the line emitting regions of a Wolf-Rayet atmosphere is unknown, although it seems inevitable that the deduced high electron temperatures come from a conversion of mechanical energy to heat. In addition, the matter in the atmosphere must be accelerated to the outward velocities which are observed. No satisfactory mechanism for achieving the needed acceleration has been proposed.

The following remarks may help to define the problem. (1) A range in outward directed velocities is observed, those ions with the lowest ionization potential having the highest outward directed velocity component. If the acceleration mechanism provides kinetic energy equally to each particle, the resulting velocities should vary inversely as the square root of the mass of the ion. The observed trend is compatible with this suggestion. If the mechanism gives a constant velocity to all particles (such as might occur with the acceleration of a condensation in the atmosphere), some differential accelerating process is required which operates differently in the regions where each of the observed ions occurs predominantly. (The outward directed motions are determined best from shortward displaced absorption components, none of which is narrow enough to define sharply a locally changing acceleration.) (2) It is considered that the presence of magnetic fields is essential for the efficient heating of the solar chromosphere and corona by the deposition of mechanical energy. There is no observation which excludes the presence of a magnetic field in the atmosphere of a Wolf-Rayet star, nor, on the other hand, is there any spectroscopic observation demanding the presence of a magnetic field. Magnetic fields have been measured in some B type stars and these fields are believed to be remnants of the interstellar magnetic field which was compressed as the star formed. A magnetic field might be attached by the same process to the proginator of a Wolf-Rayet star as it formed. Since stellar magnetic fields are believed to decay with time, an old Wolf-Rayet star (one showing the Wolf-Rayet phenomenon at a late evolutionary stage after a fairly lengthy lifetime) might have a negligible magnetic field. The presence or absence of a magnetic field might affect significantly the spectroscopic behavior of the line-emitting regions of the stellar atmosphere.

Whatever the evolutionary stage of Wolf-Rayet objects, the physical conditions in their outer atmospheres must be such as to generate the observed spectrum. Because Wolf-Rayet objects are rather few in number, it seems that the appropriate physical conditions do not occur with large probability. Because Wolf-Rayet objects, except objects like Sco X-1, seem to be brighter than $M_V = 0$, one would not expect to see a companion star of type later than A0 unless it were a bright giant or supergiant. We literally do not know whether Wolf-Rayet objects can occur in close association with late-type stars because the range in visual absolute magnitude between Wolf-Rayet stars and late-type stars is too large to permit late-type companions to be observed.

3. Gas Distributions Around Wolf-Rayet Stars

The first systematic study of the association between Wolf-Rayet stars and H II regions was made by Smith (1966) who demonstrated that a number of WN stars were surrounded by a ring-like nebula as well as being associated with H II regions. The ring nebula can be understood as a sweeping up of the interstellar material by the material ejected from the star. These observations have been reviewed and extended by Crampton (1971) who also lists the radio sources known to be associated with some of these H II regions and with Wolf-Rayet stars. It is common for Wolf-Rayet stars to be immersed in interstellar gas. The radio radiation seems to be thermal in origin.

Spectroscopic observations of binary stars [for instance Kuhi (1968a) on V444 Cygni and Cowley and Hiltner (1971) on CV Serpentis] demonstrate that the amount of material which emits line radiation in the vicinity of Wolf-Rayet stars varies in an erratic manner. The spectral variations occur at irregular intervals and they involve enough material to cause quite conspicuous changes in the line and continuum intensities. Whatever the cause is of the ejection of matter from Wolf-Rayet stars and the excitation of this matter to radiate the observed emission-line spectrum, this cause is not a fully regular, spherically symmetric process.

Any effects of the tidal force of a companion star on the gas in a binary system should be periodic. The observed changes in spectrum have not been shown to be strictly periodic in any Wolf-Rayet star.

The detailed spectrophotometric observations of Kuhi (1968a) on V444 Cygni confirm the following characteristics of Wolf-Rayet atmospheres.

(i) The Wolf-Rayet envelope is optically thick in many lines.

(ii) The emission lines behave in a completely individualistic manner, without regard to ionization and excitation potential. This means that a model consisting of stratified layers with ionization potential decreasing or increasing outward through a circumstellar shell is inappropriate for Wolf-Rayet stars.

(iii) The effective size of the emitting region is enlarged by electron scattering.

(iv) Simple models of uniform radial expansion are inadequate.

The suspected X-ray star WX Cen (Eggen *et al.*, 1968) shows irregular short-term optical fluctuations, a pronounced UV excess and night to night optical brightness variations with a total range of 0.4 mag. In these respects it is like some of the more irregular Wolf-Rayet stars. In the spectrum of Sco X-1 the complex emission band at 4630 to 4655 Å and the hydrogen lines undergo striking variations (Sandage *et al.*, 1966; Westphal *et al.*, 1968). The X-ray sources with Wolf-Rayet-like spectra appear to show more conspicuous changes in their optical spectrum than do most galactic Wolf-Rayet stars. This may be because the optical thickness of the part of the atmosphere emitting the optical spectrum is less for an X-ray source than it is for galactic Wolf-Rayet stars, with the result that the emission lines from an X-ray source lie predominantly on the linear part of the curve of growth. Such lines are more responsive to changes in the physical state of the atmosphere than are optically thick lines.

Insufficient information exists about the spectra of central stars of planetary nebulae with Wolf-Rayet characteristics to place these stars in the picture as regards spectral variations. One might expect central stars to have an emitting atmosphere of intermediate optical thickness in the lines. Such an atmosphere would tend to reveal smaller fluctuations in the density and exciting conditions of the line-emitting region than are detected for galactic Wolf-Rayet stars. An emitting region that is optically thin in all lines of optical wavelength is the most sensitive indicator of changes in the physical conditions in the atmosphere. Such a region is only readily detected for galactic Wolf-Rayet stars when the more dense regions are occulted by a companion because the sensing systems used are biased to embrace the dynamic range about the intensity of the most intense, thus optically thick, lines. The lowest line intensity that

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can be observed is closely limited by the brightness of the continuous spectrum in the neighborhood of the line.

4. The Spectroscopic Problems Posed by Wolf-Rayet Stars

The information required for making identifications in Wolf-Rayet spectra is now in a pretty good state with tables of the spectra of the carbon ions (Moore, 1970), of N v and N iv (Hallin, 1966a, b) and of O iv (Bromander, 1969) and O v (Bockasten and Johansson, 1968) having appeared. Two of the greatest difficulties remaining are recognizing emission lines which have low intensity (10 to 20% of the continuum) and resolving blends, particularly when the Wolf-Rayet spectrum is blended with the spectrum of an O or a B star. These problems are aggravated by the width of each line and the variation of the intensities of the emission lines. Since weak lines are on the Doppler part of the curve of growth, they will change more in apparent intensity than will moderately strong conspicuous lines for a given change in the abundance of the carrier ion.

In WC spectra the following identifications have been made:
definitely present – He I, He II, C II, C III, C IV, O III, O IV, O V, O VI, N III, N IV, S III,
probably present – H, N v, Al III, Si IV, Ti IV,
possibly present – Mg II
In WN spectra we have:
definitely present – He I, He II, C IV, N III, N IV, N V, O V, Si IV,
probably present – H, O IV,
possibly present – N II, O III, O VI, S III.

There is a considerable difference between the subclasses of any one sequence. Which lines are conspicuous depends upon which spectral range is observed and how the strong lines of the carbon, nitrogen and oxygen ions are distributed over wavelength. The line widths have a distinctive trend in WC stars, in fact this trend is one of the classification criteria. Among the WN stars, however, the case is different. Some WN stars have both sharp and broad lines in their spectra, others have only broad lines. These differences in line width indicate that the various emission lines may be formed under quite different physical conditions.

Ultraviolet observations of γ_2 Velorum (Stecher, 1968) show that a low density shell [C III] 1909 surrounds this WC 8 star. Observations (Bless, 1971) of HD 50896, WN5, with the OAO-II spectrum scanner show the [N IV] 1486 line which implies a low density shell containing N^{+3} ions around this WN star.

In addition to the emission line spectrum, which appears to be due chiefly to recombination and cascade, with some superposed selectively excited emission lines, a few lines appear as shortward-displaced absorption cores. These are invariably lines which arise from levels which will be strongly populated in a low density gas through which a radiation field not in equilibrium with the kinetic temperature of the gas is flowing. The superposed, selectively excited emission lines are the lines which appear in emission in Of stars.

The designation of a WC sequence and a WN sequence for Wolf-Rayet stars has often been considered to imply gross abundance differences in carbon and nitrogen between the two types of star. This idea can be supported no longer. The ultraviolet observations of γ_2 Velorum by Stecher (1968; 1970) clearly show a strong emission at about 1720 Å. There are no lines of C III and C IV in the neighborhood of this wavelength; the only probable identification is as N IV 1718.55. The N III spectrum can be detected in WC8 and WC9 stars at several places in the spectrum, see Underhill (1959; 1962), thus the conclusion of Kuhi (1968b) that the only plausible explanation of the differences between WN and WC spectra is gross differences in the carbon and nitrogen content of these stars is unjustified. It is widely recognized that the C IV spectrum appears in WN stars. Ultraviolet spectral scans from OAO-II show C IV lines in the spectrum of the WN star HD 50896.

The reason why WC spectra look so very different from WN spectra is that the WC spectra are dominated by lines of C III which has a spectrum full of quite strong lines in the spectral ranges observed with ground-based instruments. The spectra which are prominent in WN spectra are C IV, N IV and N V in addition to He I and He II. All of these contain only a few conspicuous lines in the usually observed spectral range.

Kuhi (1968b) has mentioned weak unidentified lines in WC stars at 7426, 6503 and 5700 Å. The first two may be blended multiplets of O V, see the line list of Bockasten and Johansson (1968). A line at 5700 Å would be masked by broad C III 5696 in most WC stars. It is not clear to what Kuhi is referring in this case. The unidentified lines Kuhi mentions in WN stars at 10430 and 5200 Å are due to He II (6–13) and to the N IV 2^3P^0–3^3D multiplet, respectively.

Before returning to the problem of understanding the implications of the groups of ions represented in Wolf-Rayet spectra and of the relationship of Wolf-Rayet spectra to the optical spectra of X-ray sources and of central stars of planetary nebulae, let us summarize briefly the theoretical studies that have been made to understand the meaning of the shapes of the emission lines in Wolf-Rayet spectra.

Most of the emission lines have a rounded shape and only a few lines are flat-topped. Any adequate theory of the formation of the Wolf-Rayet spectrum should be able to explain this characteristic. A number of attempts have been made to develop a detailed theory for the formation of the emission lines seen in Wolf-Rayet spectra. In a recent study, Castor (1970) has used the escape probability method for treating the transfer of radiation in a stellar envelope which is in rapid radial expansion. Castor has applied the method to a two-level atom and has calculated representative line profiles for different distributions of the density of the absorbing atoms in the case of a fixed velocity distribution. He finds that flat-topped profiles, with or without violet absorption, depending on details of the model, are produced by an optically thin hollow sphere of emitting material, which, indeed, is confirmation of an old result. An optically thick expanding envelope produces a rounded line profile. This work demonstrates that although sufficient, it is not necessary to postulate large (of the order of 700 km s^{-1}) chaotic velocities to account for the rounded lines which are commonly observed in Wolf-Rayet spectra.

Two possible simple models for Wolf-Rayet atmospheres can be considered. (1) A radially expanding atmosphere which is optically thick in most lines. Such an atmosphere may be like that used as an example in Section II for estimating the loss of kinetic energy from a Wolf-Rayet star. That model implies a mass loss of 2.5×10^{-4} solar masses per year. The outer part will become optically thin in a few lines which are not excited in the inner dense part of the atmosphere. These lines will have a flat-topped profile. One may account for the observed flat-topped lines by a suitable adjustment of the ionization level as one moves outward. (2) An inner, dense atmosphere in which large, randomly directed motions have been generated by some unspecified process, is surrounded by an exosphere which is visible optically in a few lines, all optically thin. This model is rather artificial; it can, however, be adjusted to imply a rather small rate of mass loss if that is considered desirable. Both models ignore the irregularities known to exist in the radial flow from a Wolf-Rayet star.

Castor and Van Blerkom (1970) have applied the theory developed by Castor to the He II spectrum of two WN6 stars. They find that the observed intensities of He II lines and the rounded shapes of the lines can be represented by plausible combinations of temperature, density and radial flow. This work indicates that models of Type 1 are relevant for Wolf-Rayet stars. Castor and Van Blerkom estimate suitable, representative values of N_e and T_e in a WN6 atmosphere to be (2.5×10^{11}, 5×10^4 K), (2.5×10^{11}, 10^5 K) or (5×10^{11}, 2×10^5 K). The suggested electron temperatures are higher than have sometimes been suggested for WN stars while the electron densities are fairly low. The adopted radiation temperature of the photosphere which provides the radiation illuminating the expanding atmosphere is set to be 40000 K. It is assumed that typically the radius of the line-emitting atmosphere is 3 times the radius of the photosphere.

The calculations of Castor and Van Blerkom indicate that the populations of the higher levels of the He^+ ions in a Wolf-Rayet atmosphere relative to the number of He^{+2} ions have the distribution expected according to the Boltzmann law at the electron temperature. That the high levels will be in thermal equilibrium with respect to the number density of the next higher ion at temperatures and pressures of astrophysical interest was first shown for hydrogen-like ions by McWhirter and Hearn (1963). We shall assume that this is true for the upper levels of all the lines one observes in the normally observed spectral region of Wolf-Rayet stars. For example, to understand the strengths of the lines in the C IV spectrum, one requires the populations of the upper levels of these lines in the C^{+3} ion. We assume that these populations can be found from the abundance of the C^{+4} ions and use of the Boltzmann law.

The fraction of the light elements in each stage of ionization at a given electron temperature has been calculated by Jordan (1969) for a thin plasma in which the radiation field is negligible taking account of collisional ionization, collisional excitation followed by autoionization, direct radiative recombination, radiative recombination via bound levels and dielectronic recombination. The fraction of any element in each stage of ionization in the case of thermal equilibrium at a temperature T and electron pressure, $\log P_e$, can be calculated by successive applications of Saha's law.

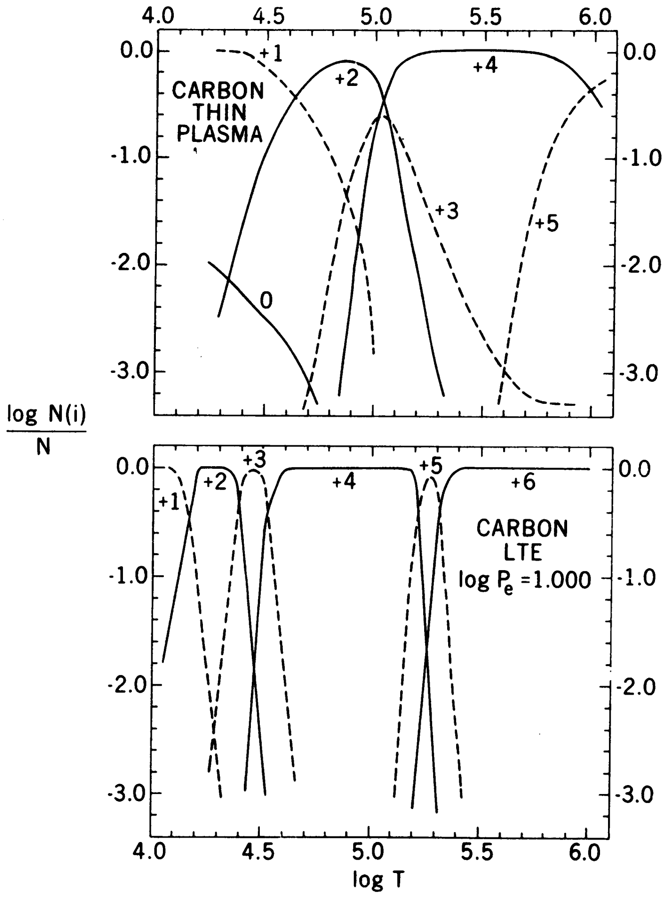

Fig. 1. The fractional ionization of the carbon ions as a function of temperature. Upper panel: in a thin plasma (Jordan, 1969); lower panel: in a plasma in LTE with $\log P_e = 1.000$.

These two cases bracket what may be expected to occur in a Wolf Rayet atmosphere. The results for carbon, nitrogen and oxygen for some temperatures of interest for Wolf-Rayet stars are shown in Figures 1, 2 and 3. The LTE results have been taken from Sparks and Fischel (1971), see also Fischel and Sparks (1971). They are displayed for the case $\log P_e = 1000$, which implies an electron density of 3.6×10^{12} when the electron temperature is 20000 K and 3.6×10^{11} when the electron temperature is 20000 K.

Thermal equilibrium ion ratios can only be maintained in an optically thick plasma. The true case for a Wolf-Rayet atmosphere lies between the two extremes presented here. Note that the electron temperature required to produce the maximum ionization fraction for any ion is greater in a thin plasma than in a plasma in LTE. Also the temperature domain in which the ionization fraction of any ion is greater than a set level, say 10 percent, is considerably wider in a thin plasma than in an LTE plasma.

Consider a WC7 spectrum. Here the C III and C IV lines are strong. N III and N IV

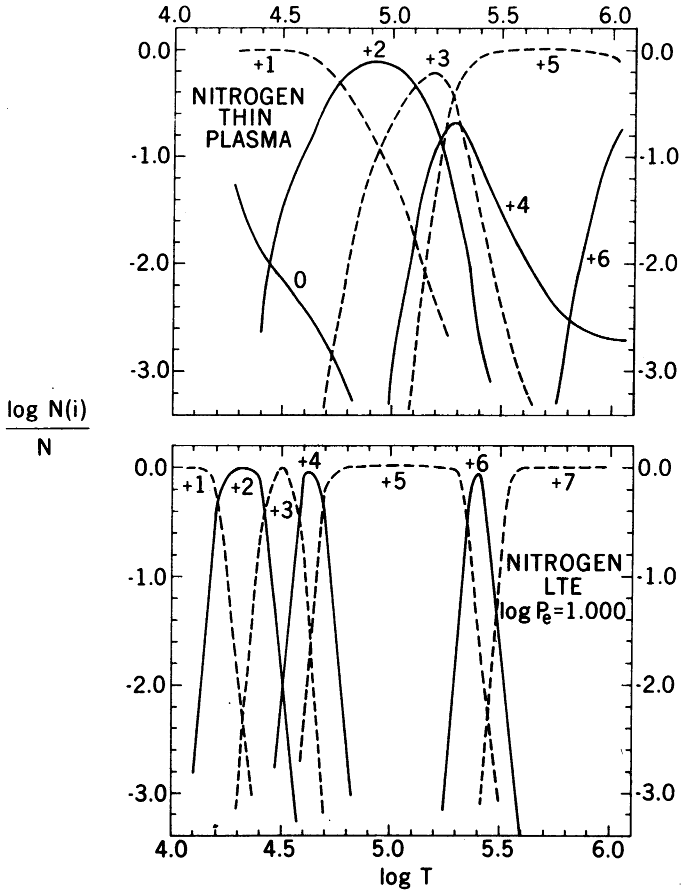

Fig. 2. The fractional ionization of the nitrogen ions as a function of temperature. Upper panel: in a thin plasma (Jordan, 1969); lower panel: in a plasma in LTE with $\log P_e = 1.000$.

are weak and N v is not seen. Lines of O III and O IV are seen, but neither O v nor O VI is conspicuous. To obtain these spectra in emission one would like to find a temperature where the C^{+3} and C^{+4} ions are abundant, the N^{+3} and N^{+4} ions are moderately abundant but N^{+5} is not very abundant, and finally that O^{+3} and O^{+4} are moderately abundant, but O^{+5} is not very abundant.

If the plasma is thin the following temperatures are needed:

Carbon $4.95 < \log T < 5.10$
Nitrogen $5.10 < \log T < 5.25$
Oxygen $5.20 < \log T < 5.40$.

If the plasma is in LTE and $\log P_e = 1.000$:

Carbon $4.45 < \log T < 4.60$
Nitrogen $4.52 < \log T < 4.62$
Oxygen $4.68 < \log T < 4.78$.

In the case of WN stars one requires that the carbon appears predominantly as

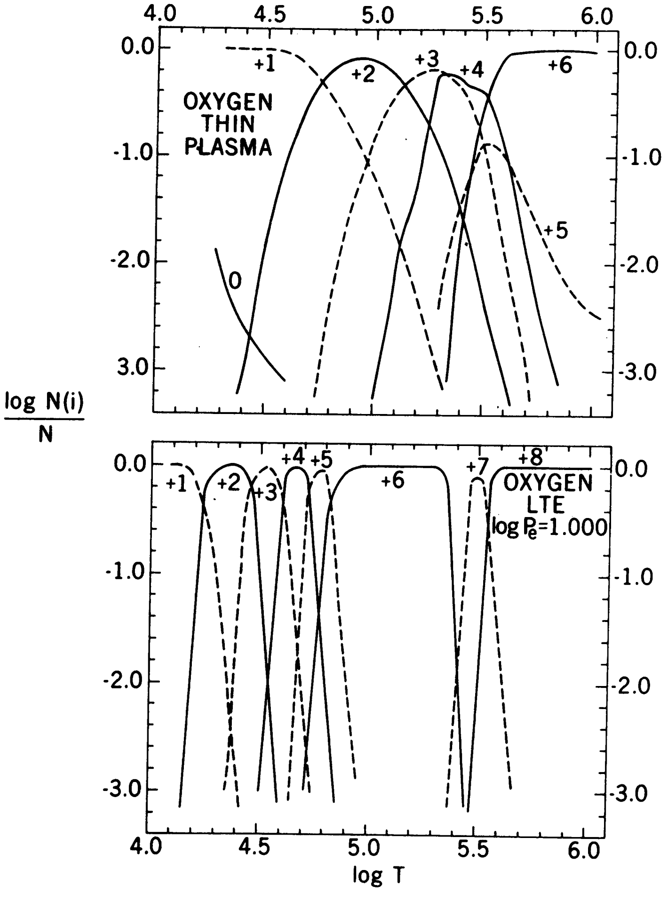

Fig. 3. The fractional ionization of the oxygen ions as a function of temperature. Upper panel: in a thin plasma (Jordan, 1969); lower panel: in a plasma in LTE with $\log P_e = 1.000$.

C^{+4}, nitrogen as N^{+4} and N^{+5} with some N^{+3}, and oxygen as O^{+4} and O^{+5}. In the case of a thin plasma:

Carbon $5.20 < \log T < 5.90$
Nitrogen $5.30 < \log T < 5.90$
Oxygen $5.50 < \log T < 5.70$.

In the case of a plasma in LTE with $\log P_e = 1.000$:

Carbon $4.60 < \log T < 5.10$
Nitrogen $4.65 < \log T < 5.20$
Oxygen $4.65 < \log T < 4.85$.

Clearly the electron temperature in the extended atmosphere of WC stars is lower than that in the atmosphere of WN stars. Thus, the WC and the WN stars do not form parallel sequences. Very roughly, if the thin plasma calculations are appropriate, the electron temperature is approximately 1.6×10^5 K in WC atmospheres and 5×10^5 K in WN atmospheres. The lower limits given by the hypothesis of LTE are approximately 4×10^4 K in WC stars and 6.5×10^4 K in WN stars. These temperatures are higher than may be inferred from the shape of the continuous spectrum. They indicate that the outer atmospheres of Wolf-Rayet stars are being heated by mechanical energy.

If an object shows the O VI lines in emission, as some WC stars do, the temperature must lie in the range $5.4 < \log T < 6.0$ if the plasma is thin or $4.8 < \log T < 5.4$ if the plasma is in LTE. Temperatures such as these are sufficient for the star to generate soft X-rays by bremsstrahlung. However, since most galactic Wolf-Rayet stars are in the galactic plane, this X-ray emission would never reach the earth in observable quantity. The peculiar object HD 6327 which only shows emission lines of He II (Hiltner and Schild, 1966) according to these ideas has an atmosphere in which the electron temperature is so high that all the carbon, nitrogen and oxygen atoms are in stages of ionization too high to show lines in the visible spectrum. A temperature of more than 10^6 K would be required.

According to Kestenbaum *et al.* (1971), the X-ray continuum intensity distribution of Sco X-1 corresponds to a plasma at 8×10^7 K and an electron density near 10^{16}, yet the emission-line optical spectrum (Sandage *et al.*, 1966; Johnson *et al.*, 1967; and Westphal *et al.*, 1968) corresponds to a fairly low level of excitation with strong H and He I and no C IV. The optical thickness of the line-emitting region of the spectrum seems to be sufficiently small that very little of the X-ray energy is degraded into optical radiation. The optical observations of Mook, Hiltner and Lynds (1971) give evidence of a sporadic mass flow at 900 km s^{-1}. Since there is no evidence for fluctuations in the X-ray intensity of Sco X-1, Boldt *et al.* (1971) have concluded that the energy input into the plasma must be essentially continuous compared with the time scale of the optical variations. The irregular variations in optical wavelengths would seem to be generated by density fluctuations in the ejected gas and these density fluctuations seem to occur independently of the energy input causing the X-ray emitting plasma.

Optical observations of WX Cen (Eggen *et al.*, 1968) indicate a somewhat similar pattern. Here the optical plasma corresponds to a level of excitation like that of a WN7 star, except that He I is surprisingly weak and H is strong. The faint lines shown shortward of He II 4686 correspond to N V (effective wavelength near 4605 Å) and N III and C IV (effective wavelength near 4642 Å). The line at 4931 Å unidentified by Eggen, Freeman and Sandage, is surely a blend of O V and N V lines. Eggen, Freeman and Sandage argue that there are no good reasons to couple a variation of the X-radiation with the optical fluctuations. One may infer that the optical variations are largely caused by irregularities in the flow of matter through a thin outer shell.

The optical spectra of X-ray sources indicate that a major difference between X-ray sources and Wolf-Rayet sources is the optical thickness of the outer atmosphere which generates the optical spectrum. A radial flow of matter which contains density irregularities is seen for X-ray sources and for Wolf-Rayet stars. The ultimate source of energy for creation of a high temperature plasma may be the same in both cases. In X-ray sources the high-temperature plasma is visible owing to the thinness of the fringe atmosphere; in Wolf-Rayet stars the high-temperature plasma is barely visible, if at all, and the line emission of the fringing atmosphere dominates the optical observations. The irregularities in the density structure of the expanding atmosphere remind one of the coronal condensations and coronal plumes of the Sun. Possibly they are related to a magnetic field structure in the stellar atmosphere. The presence of a magnetic field seems to be necessary for deposition of mechanical energy in the solar chromosphere and corona.

The spectra of two Wolf-Rayet-like objects in M33 (Wray and Corso, 1971) seem to be WC-like. The relative weakness of the He I and He II lines in comparison to the C IV lines is a bit strange. It is premature to discuss these spectra now; their existence demonstrates one more place where the Wolf-Rayet phenomenon occurs, but the relative line intensities of galactic Wolf-Rayet stars are not seen.

5. Summary

Any acceptable model of a Wolf-Rayet-like object must take account of the following characteristics of Wolf-Rayet-like objects:

(1) Galactic Wolf-Rayet stars have masses in the range 6 to 15 solar masses; Wolf-Rayet-like central stars of planetary nebulae presumably have masses an order of magnitude smaller as do the Wolf-Rayet-like stars which emit X-rays. We have no direct evidence on this last point.

(2) The optical light from Wolf-Rayet-like objects may fluctuate in intensity and the optical spectrum may change in an irregular manner. For galactic Wolf-Rayet stars the changes are small. For stellar X-ray sources the optical changes are fairly conspicuous but they are not accompanied by detectable changes in the X-ray spectrum. No changes have been noted for the Wolf-Rayet-like central stars of planetary nebulae, but the observations are not sufficient to rule out the occurrence of small changes.

(3) There is a low density outer atmosphere around the inner atmosphere. In the case of galactic Wolf-Rayet stars the inner atmosphere is optically thick in most lines.

(4) There is a radial outflow of matter from the star; irregularities in the density of the outflowing gas occur.

(5) In WC stars the electron temperature may lie in the range 4.0×10^4 to 1.6×10^5; in WN stars the range is 6.5×10^4 to 5.0×10^5 K. If a model corresponding to a thin plasma in which radiative excitations are not important is appropriate, temperatures near the top of each range may be expected.

(6) The electron temperature in the line-emitting regions seems to be higher than that in the regions which give the continuous spectrum at optical wavelengths. In the case of X-ray sources, the temperature in the regions where the continuous X-ray spectrum is formed is of the order of 10^7 K.

(7) The electron density in the optically thick line-emitting regions lies in the range 10^{11} to 10^{12} particles per cm^3.

There is no reason not to assume a normal stellar composition for the atmosphere of a Wolf-Rayet-like object. The theory of spectrum formation in a Wolf-Rayet atmosphere is not yet sufficiently well developed to permit accurate deduction of

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abundances, but qualitatively the observed line intensities follow the theory of spectrum formation in a hot plasma of normal stellar composition at the temperatures and densities indicated above.

The problems which require further study (in addition to the problems of the theory of line formation) are related to the fundamental question still requiring an answer, namely, what special factor produces a Wolf-Rayet spectrum for stars in the range 6 to 15 solar masses or 0.6 to 1.5 solar masses? The action of this factor is not limited to one region in the HR diagram. This factor is not active for every star in the specified mass ranges. In fact, only a few stars of the specified masses show a Wolf-Rayet spectrum. Either being a Wolf-Rayet object is a relatively short-lived stage of evolution traversed by all stars of the correct mass, or some special combination of physical factors produces the hot plasma which is required to generate a Wolf-Rayet-like spectrum.

One suspects that the high electron temperatures required to produce a Wolf-Rayet spectrum are the result of converting mechanical energy to optical radiation by high speed particles impinging on a low density gas. If this is so, then it is not obvious what meaning is to be attached to the trend of visual absolute magnitude with spectral type found in the WC and in the WN sequence. This problem raises the question of how the bolometric correction should be defined when the star may be ejecting as much energy in the form of kinetic and excitation energy of particles as it is loosing in the form of radiation. What spectral features, if any, are true indications of effective temperature when effective temperature is defined to represent the total rate of energy loss from the star?

The Wolf-Rayet-like objects seem to be somewhat similar to each other in nature, their spectroscopic differences reflecting merely a somewhat different visibility of the three major parts of their atmosphere. These three parts are (1) a dense hot plasma, (2) a moderately dense expanding atmosphere and (3) an exosphere in which radiative excitation is not important.

5.1. Stellar x-ray sources

(1) A high-temperature plasma ($T \approx 10^7$ K) which radiates X-rays is seen. X-rays escape through the thin overlying atmosphere largely unimpeded.

(2) The expanding atmosphere is a thin fringe which radiates an optical spectrum corresponding to an electron temperature in the range 3×10^4 to 8×10^4 K. The density in this expanding atmosphere varies irregularly with time.

(3) An exosphere has been observed for the X-ray source WX Cen. The exosphere is seen most clearly by the forbidden C III and N IV lines in the ultraviolet. Observations of adequate spectral resolution to resolve these lines have not been made for stellar X-ray sources so far.

5.2. Wolf-rayet-like central stars of planetary nebulae

(1) The dense high temperature plasma is partially obscured by an overlying expanding atmosphere. Hard X-rays may be degraded to soft X-rays which escape and excite the surrounding nebula.

(2) The moderate density expanding atmosphere may have an electron temperature near 10^6 K in some regions. This very hot plasma will radiate O VI lines; other Wolf-Rayet lines would be radiated from cooler parts of the plasma. In order to have O VI lines and lines of N V, C IV and lower ions present, it is necessary to have plasma at 10^6 K and at 10^5 K or cooler.

(3) Ultraviolet observations have not yet been made at adequate resolution to confirm the presence of an exosphere. The planetary nebula can be considered to be an extreme extension of the exosphere.

5.3. Galactic Wolf-Rayet Stars

(1) A high density, high temperature plasma, barely visible through the overlying expanding atmosphere, may be postulated. This may be the source of the UV excess of WN stars. It is doubtful if a sufficient X-ray flux would escape and be observable at the earth.

(2) The optical spectrum is given by an extensive, fairly dense expanding atmosphere with electron temperatures of the order of 10^5 K, the temperature being higher in WN atmospheres than in WC atmospheres.

(3) An extensive exosphere has been observed in C III] 1909 for γ_2 Velorum, WC 8, and N IV] 1486 for HD 50896 as well as by means of flat-topped lines and shortward displaced absorption cores. The exosphere eventually meets with the surrounding interstellar medium, creating in the case of some WN stars a visible ring-nebula around the Wolf-Rayet star. Because ring nebulae are not seen around WC stars, one must conclude that the energy transferred from the gas of the exosphere to the interstellar medium is insufficient in the case of WC stars to create a nebula observable in the hydrogen Balmer lines or that the filamentary material has dispersed. There does not seem to be any significant difference between the distribution of WC and of WN stars in the interstellar medium. Thus, the absence of detectable ring nebulae around WC stars should be construed to mean a difference in the energy carried by the exosphere of WC stars from that carried in the exosphere of WN stars or a difference in stage of evolution.

A three-part model of the general type given above will account for the observed spectrum of all Wolf-Rayet-like objects. The hot, dense plasma is the origin of the energy which causes the overlying layers to radiate a Wolf-Rayet type spectrum. One may speculate that this hot plasma is the outer part of a shell in which hydrogen is burning. Why such a shell should occur so close to the stellar surface that it is visible or nearly so is a problem in the theory of stellar structure. It may be because of mass exchange in a close binary (Paczynski, 1967). However, the stars which show ring nebulae or planetary nebulae seem to be single stars. Studies which take into account hydrodynamical flow in the outer layers of stars will be required to provide a proper understanding of the mass flow from Wolf-Rayet-like objects and of the cause of the density irregularities which seem to appear in the flow. The Wolf-Rayet stars are still peculiar objects, but some pattern for understanding them is beginning to appear.

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DISCUSSION

Kuhi: It is very dangerous to extrapolate the conditions that we have in a binary system, such as V444 Cygni, γ_2 Velorum, and so on, to the single stars. I think that it is the fact that the Wolf-Rayet star is in a binary system that causes all or most of the peculiar structure, intensity fluctuations, etc., that we see. This is not to deny that there could be iregularities as well in the single Wolf-Rayet stars, but observations on that point are sadly lacking. The reason I feel this is that the O star in the system is a

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very hot object that is very close by, and clearly must influence the ionization structure, etc. of the Wolf-Rayet star. The observations also suggest flows of material from the Wolf-Rayet star to the O star, and perhaps streams in an outward direction as well. I just wanted to make a little bit clearer that those particular observations refer to the binaries and that I am not sure that we can really safely extrapolate them. Then, just one question: the suggestion of the rings with the WN stars and the nuclei of planetary nebulae being somehow related in an evolutionary sense, raises an interesting point. The nuclei of planetary nebulae that are Wolf-Rayet stars, seem to be primarily WC stars; I think without exception, is that right?

Smith: There is just one exception. M1–67 has a WN8 nucleus.

Kuhi: We know that the ring objects are entirely Wolf-Rayet stars, so that if we are going to put planetary nebulae into the same category as the ring objects, are you implying some sort of evolution from WC to WN, or not?

Underhill: That follows as a deduction, yes.

Kuhi: I wonder what Paczynski might say about that.

Underhill: What Paczynski made clear to us is that the changes in the chemical composition in the inner part of the stars during their evolution, make it possible that you can have WC in two different stages of the star's evolution, as you uncover deeper and deeper parts of the star.

Paczyński: It is quite possible that there is an evolutionary sequence. I am not prepared to discuss this as the problem appeared just at this Symposium and I would rather spend some time looking at those objects, before I comment upon them.

Thomas: It seems to me that what you were willing to discuss is that point that Lindsey Smith and I had some interchange about: the fundamental difference between the Wolf-Rayet star in the planetary nebulae and that one which was not in the planetary nebulae; that is, the small mass and the big mass. The biggest item in your mind is, maybe there is not a difference in mass; at least, if I understand well, it is open to question whether there is really a difference in mass. Is that right?

Paczyński: What I really had in mind, when I spoke a few days ago about these problems, was that we know fairly well masses and luminosities of those Wolf-Rayet stars which are the components of binaries. The evolutionary status of those stars and the population assignment does not lead to much doubt. However, we have two other classes of objects in which the Wolf-Rayet type spectra are found. These are: nuclei of planetary nebulae and central stars of ring nebulae. If we assume that these objects are related to each other rather than related to either normal planetary nebulae or to Wolf-Rayet binaries, then we have to start from the very beginning. Their luminosities, temperatures, masses, their evolutionary status was assumed to be known because those objects were believed to be related to planetary nebulae and binaries respectively. And now, if we assume that they are related to themselves we just cannot say anything about them, at the moment.

Thomas: Can I summarize what you are not willing to say about them? One, you are willing to say, for those masses like 5 plus or minus something, where the Wolf-Rayet 'class' lies on the evolutionary scale, you are not really willing to try to make a fine distinction between different WN types or between the WN and WC types, other than a very qualitative speculation. Is that correct?

Paczyński: Sorry for making so much confusion! I believe that as a result of the discussion we had here a few days ago, our knowledge about the nature of central stars of planetary nebulae and central stars of ring nebulae which show Wolf-Rayet type spectra is smaller than it had been one week earlier. And, therefore, I would not like to comment about the evolutionary status of those stars, their masses, or their population assignment. You have to distinguish the stars which do show nebulae around them from those stars which appear in binaries. As far as I know, there is no star in common to the two categories. And, therefore, I would not like to discuss the evolutionary nature of the stars that are in nebulae. Previously, I thought that some of them are like nuclei of planetary nebulae, and then their evolutionary status would be very clear. And I thought that the others are related to massive Wolf-Rayet binaries, and their evolutionary status would also be more or less clear. Now, if we are willing to put the two kinds of stars which show nebulae around them together, you have to break their relation to the Wolf-Rayet binaries and 'normal' planetary nebulae, because Wolf-Rayet binaries and 'normal' planetary nebulae have nothing in common with each other. Therefore it is very difficult for me to say anything about the evolutionary status of those Wolf-Rayet stars which show nebulae around them, until I convince myself that these are either similar objects or dissimilar objects.

Thomas: You talked perpendicular to what my question was! If I decide to divide WR stars into 3 classes, those whose mass is around 10 (which we only know from the binaries); those single stars which have some nebulae around them, and those single stars, central stars of the planetary nebulae, then what I asked was: looking only at these, if I understood what you said the other day, you were willing to discuss where, on the evolutionary scale of a massive star, the Wolf-Rayet spectrum phase is liable to occur, namely when it becomes roughly a helium star. That is what you associated this spectrum with.

Paczyński: I would like to stress again that no stellar interior calculations can provide you with spectra.

Thomas: All right. Let us associate you with what Lindsey Smith said. In some way, by some train of logic, without worrying about right or wrong, she suggested that possibly the Wolf-Rayet atmospheric phenomenon is associated with the central star, which is a helium star. Now, once you adopted that, then you were willing to discuss the evolutionary configuration which contributed the helium star. That is all I have said.

Paczyński: Yes.

Thomas: Now, if I go off again by some chain of logic, from Lindsey Smith or anyone else, and say, of these Wolf-Rayet stars, there is a set of WN's and there are some WC's, then I can make some conjecture about what this may or may not mean. You are willing to again engage in some kind of conjecture; you say, all right, there is a helium star, maybe there is a phase of C burning also, and that would make some kind of a behaviour on the surface. I understand; you do not want to get involved with the atmosphere, you only get involved with the interior. So, you are willing to discuss where the helium configuration occurs, and something before that and something after that, in terms of the change in effective temperature or luminosity or something like that. Is that a good way to put it?

Paczyński: I am not sure.

Thomas: You are awfully cautious on the last day, as compared to the earlier days!

Paczyński: No, I believe all of my discussion referred only to the Wolf-Rayet binaries.

Thomas: Let us distinguish, then, two things about a binary: First, for binaries, I know what the masses are, and second, there is a mass exchange problem in them. You have two distinct things. If I had a separate star, whose mass is like 10, then you are willing to say when the helium phase might come.

Paczyński: Yes. There can be little doubt that Wolf-Rayet stars in close binaries are burning helium and are largely helium stars. And this is based on the fact that they have masses around 10 solar masses, and the luminosities above 10^5 solar luminosities, and fairly high temperatures.

Thomas: And a way of getting rid of hydrogen from the outer part.

Paczyński: Right. And I am really in a very different position when we go to single stars.

Thomas: We are just asking conclusively, what you are willing to do for binaries in terms of mass exchange, and speculatively, in terms of single stars.

Paczyński: Yes, as far as binaries are concerned, mass exchange does only one thing, it removes the massive hydrogen envelope from the star. Therefore, the star becomes overluminous.

Thomas: Right. Do we understand that that is your main point?

Paczyński: Yes.

Thomas: Now, if you let me put that aside for one minute, and say – because I keep trying to come back to Lindsey Smith's speculation if you now go to the single stars, then maybe I have two kinds of single stars that I can talk about, and maybe only one. I have two alternatives, either there is a class of single stars with masses like 10 $M_\odot$, that are like the Wolf-Rayet components of binaries, except that they remove the possiblity of mass exchange. Then, maybe I have stars with one solar mass, which will somehow exhibit essentially a Wolf-Rayet spectrum. Or, maybe, these two classes of single stars are the same, only I just do not know the mass well enough; they may all be mass 10, 1 or 5 $M_\odot$. Now, given those possibilities for the single star, if I have no way of removing the excess hydrogen from the envelope, then you are worried and you do not really want to talk. If you could find some way of removing that excess hydrogen from the envelope, then you would be willing to say that the 10 $M_\odot$ ones, at least, might be like the stars in the binaries.

Paczyński: I will put it in a different way. We do observe that at least some hydrogen was removed from those stars, as we do observe nebulae expanding away from those stars.

I am willing to speculate that those stars lost most of their hydrogen envelope. However, as long as we are not sure whether we can relate those stars to other kinds of stars we know, we are not sure about their masses. As long as we do not know their masses we cannot say anything definite about their internal structure.

A few days ago I thought that some of those stars are burning helium in the core, and others are burning helium in the shell. Today I do not know, because I do not know the masses.

Thomas: That is right. But if you were told by someone that they really had a mass one, then, maybe you would go off into your discussions about the C burning as well as the He.

Paczyński: It would be helium shell burning.

As I mentioned previously, if we believe that there is neutrino emission due to the universal Fermi interactions, then carbon burning is out of question. It lasts for too short a time.

Thomas: That is all for the stellar interior standpoint. Now, when you say you are sure that these things have ejected the hydrogen shell, then there comes the question: I have those Wolf-Rayet stars which do not give us evidence of shells or nebulae, for whatever reason, but there is no real difference in the spectrum, so far as the presence or absence of hydrogen, between these stars and those stars with nebulae. Is that a correct observational statement? All I am really doing is being the devil's advocate, in trying to follow these lines of thought. Paczynski concludes that these things have a mechanism ejecting the outer part of hydrogen shells, because they have nebulae around them, so that the star itself does not have any hydrogen in the surface, and hence can be overluminous. My comment is, these stars, spectroscopically do not really differ that much from those with no shells, with regard to those observed facts that led Lindsey Smith to say that there is no hydrogen in the big Wolf-Rayet stars – not the planetary nebulae ones, but the big Wolf-Rayet stars. So that the common characteristic of those stars, in your mind, is no hydrogen, is that it?

Smith: My hypothesis is that lack of H is a necessary condition for a WR spectrum in both kinds of star. The facts are: (1) the single, Population I WR stars with nebulae are of classes WN5, 6 and 8; I have reported their H/He ratios and they are all very low. (2) The Population I binaries appear to have the same H/He ratios as the single stars of the same subclass. (3) The planetary nuclei are all WC's except one. The spectrum of the only WN nucleus is WN8 and appears to visual inspection, to be identical to the spectra of the Population I WN8 stars, so I presume its H/He ratio will also be low. The WC planetary nuclei have the same problem as the Population I WC stars, regarding H/He ratio determination – I have given my indirect reasons for believing that they have no hydrogen.

Underhill: I think it is essential that a spectroscopic method of estimating the hydrogen to helium abundance with some precision be devised. At the moment we have only guesses. I will say no more about that. The second point I brought up, which I think is important to do, is to determine if those central stars of planetary nebulae with Wolf-Rayet-like spectra, do indeed belong kinematically to population II. This is a straightforward, observing program, though a difficult one, but I think it is essential, because we do not know whether stars of Population I can have such dense envelopes around them that they look like planetary nebulae.

Thomas: Can you really do a kinematic study from five or six stars?

Underhill: For those stars for which you have accurate measured proper motions and radial velocities directly.

Thomas: Would you believe it if you had five or six stars?

Underhill: For each group you could tell whether they are high velocity objects or not. These stars are presumably rather faint intrinsically. Whether they are at the limit of the instrumental capabilities for such measurement, it is very difficult to say. I think it is of sufficient interest to make it a worthwhile problem to look into. It is not easy to get the proper motion; it may be completely impossible, but if it is all possible, I think it would be very worthwhile doing because it is a big generalization that you have been making all the way through, when you say of the central stars of the planerary nebulae that they may be all belong to one polulation. The very few stars we have talked about actually are not spread very much in galactic latitude. So, they may be Population I objects. It is just something to be investigated.

Conti: Yes, I would like to come back to the point Thomas raised. You were worried that these single stars without nebulae do not show any evidence of having ejected any material and, therefore, you say since the spectra looked like those which have ejected material you do not know whether the hydrogen has come off or not. Well, it seems in astronomy that one often takes another field of astronomy for granted. For example, if the radio atronomers tell us something, we say that is all right. It may well be that the reason that they do not have any nebulae, is not so much that they have not ejected or lost all the material, but rather that the conditions around the star, in the interstellar medium, are such that you just do not see the nebulae.

Thomas: Yes, that is the point we raised earlier. That is indeed a point that I really feel one should look very carefully at, both that one and the non-spherical nature of the ejected shells, and I hope that you or somebody would push that point further.

Conti: That is a nice problem for somebody who knows something about the interstellar medium.

Thomas: No, it is a problem for somebody who wants to learn something about the interstellar medium. It seems to me, this is the kind of thing I would pose to the people at Berkeley. Johnson, you are used to this material. Could you sit down right now and measure the ellipticity of the nebulae, and then come up with a conclusion on the differential distribution of the interstellar medium and throw it up to the Berkeley people and say, does this make possible sense?

Morton: One always wonders when you see a non-spherical nebula, how much of it is due to the interstellar medium, and how much of it is due to some effect like the rotation of the star or the ejection mechanism, particularly if the nebula is elliptical.

Smith: (added after the Symposium) Losinskaya has shown that the major axis of NGC 6888 is parallel to the direction of elongation of the Cygnus arm. This implies that magnetic fields restrain expansion along the minor axis – a very simple explanation in this case.

Thomas: All you can do, is examine the possibilities. And I agree, is there some way that one can make some investigation of rotation possibilities here? With these kind of lines?

Underhill: That is difficult, but the idea of uneven ejection, in different rates, of course, is very well documentated. That it occurs, was known actually in 1905 or so. Very definitely there were bursts of gas coming out of Nova Aquilae, which were followed for many years. They were not spherically symmetrically distributed about that star.

Kuhi: That happens for most novae, and, in general, one can say that the ejection is not spherically symmetric. And certainly Nova Delphini 1967 is another good case. But, I think that with novae, one is dealing with a much more catastrophic event which takes place on a short time scale compared to what we are talking about here.

Thomas: One should just look at all of these possibilities. Possibly, sitting down and listing these possibilities and investigating them, one by one by one.

Conti: I would like to bring up a slightly different topic now, which concerns the binary frequency for Wolf-Rayet sstars. What I want to make is a statistical argument and only a statistical argument, that it could be that all the Wolf-Rayet stars are binaries, and the arguments that are important are the following: first, if you suppose that you have a Wolf-Rayet system which had equal masses, then you can realize immediately, that you would not observe the lines of the companion, because the Wolf-Rayet star is some 2 or 3 mag. overluminous for its mass. Normally an absorption line star has to be within 1.5 mag. of a brighter companion, otherwise you do not see the absorption lines. So, you see that there is some room for a more massive companion than the Wolf-Rayet star, but let us just suppose they are equal masses. Then, you do not see the companion as far as any absorption lines are concerned. So then, the only other way to detect whether the Wolf-Rayet is a binary, is in variations in the emission-line velocities. And now, of all the binary systems that we know about (some of which have been measured much more carefully than Wolf-Rayet stars that appear to be single, because we already see the absorption lines), the smallest velocity amplitude (2K), in any known Wolf-Rayet system for the emission lines, is about a hundred kilometers per second. Now, it is also true that of all the known Wolf-Rayet binary systems, I do not think any of them have the inclination less than 30°. If you think of random orientations of axes and inclinations, then sin i is also proportional to the fractions which have those inclinations. Therefore, since $\sin 30° = 0.5$ you could also easily imagine that there is another 50% of the Wolf-Rayet stars, that have sin i less than 30°, and, therefore, the maximum velocity range may well be less than 100 km s^{-1}. We have not detected these systems. And Dr. Kuhi has already said something like 60% or 70% Wolf-Rayet stars are already detected as binaries. So that, I would just want to point out that you can make this statistical argument that says they could all be binaries.

Thomas: Except, let us ask Paczynski one question. To be overluminous, a star has to shed a lot of mass. Let us talk about the possibility of any configuration you want to start with, then what can I say in terms of this mass shedding, as to the mass of the secondary, when I reach the Wolf-Rayet stage of the primary? Can they be as small or equal?

Paczyński: If you assume that there is not much mass loss from the system as a whole, then the mass of the original primary, after the mass transfer, is smaller than the mass of the other star, the non-Wolf-Rayet star.

Thomas: What does it do to your theory, Conti? Is that not embarrassing?

Conti: I am not so sure, because I could imagine the situation where you start with a 20 $M_\odot$

star and a 5 $M_\odot$ star, and then the Wolf-Rayet star loses half of its mass. Is that sufficient or not?

Paczyński: Oh, sure, it can lose ....

Conti: And then they each have about 12 $M_\odot$. I mean, that is an example, or start with 25 and 5.

Morton: I wonder if we can turn Conti's argument around. He paints such a pessimistic picture of finding binaries among Wolf-Rayet stars, and we find so many. Is the fact that we are so successful in finding binaries really trying to tell us something about the nature of the systems?

Conti: Yes, I agree that is an alternative explanation. What I think you are saying is, does the fact that we find them so easily, mean that really there has always been such a large mass exchange, that the new primary ends up massive enough so that we almost always see it. Am I right?

Thomas: Right! So, if it is not seen as a binary, it probably is not a binary.

Underhill: No! Suppose your original companion, is a low-mass A5 star and you have this great big mass of a Wolf-Rayet that generates a Wolf-Rayet spectrum at a certain stage as it starts to get rid of matter in a hurry. Now if you only have a little star there to catch the matter, I wonder if the loss from the system might not be considerable. So, when it has caught some mass it still is not big enough to be seen. It is a point I skipped over in my summary. To shorten it, I did not mention the fact that the only companions we possibly do detect are very bright ones.

Thomas: Until one solves the problem of how much mass is captured. Has that been done?

Paczyński: It will not be done!

Thomas: It will not be done within the range of methods that you see at the minute, but always somebody will. Anything which can be done, will be done, it is just a matter of when.

Paczyński: As far I remember, there are single line Wolf-Rayet stars, which do not show O-type spectra, but which do show orbital motion. Is that right?

Smith: That is right.

Paczyński: So, we have observational evidence that there are binaries, whose O-type component is invisible. No matter what is the theory of mass transfer and mass loss.

Sahade: The companion star does not have to be O.

Paczyński: We just do not see it.

Thomas: So, in the argument now, which way are you going?

Paczyński: From purely empirical point of view, there are binaries with only the Wolf-Rayet spectrum visible. Therefore, if such a binary has an inclination of the orbit so low, or the period so long that you cannot detect variations in radial velocities then you have no way to say that this is a binary. So this is in favour of Conti's suggestion.

Smith: Yes, if you had a significant amount of mass loss from the system, which is what you suggested for formation of the nebulae, then the companion would be low mass and very difficult to observe. So, that there still is a very strong possibility that all WR stars are, or were, binaries.

Paczyński: Well, we do observe outflow velocities from planetary nebulae to be as low as 30 km s^{-1}. It implies that if there is a binary, its velocity of orbital motion must be less than 30 km s^{-1}. Otherwise all the matter would be captured by the second component. And that implies that if it is a binary then it is very wide as not to interfere with the mass loss.

Thomas: Except, if it were elliptical, rather than circular.

Smith: Yes, I would not argue particularly strongly that the nuclei have to be binaries. They are quite distinct objects from the ring nebulae and have obviously a different evolutionary history. Stars that become planetaries obviously have their own mechanism for lifting off the H atmosphere; one does not need the binary mechanism.

Kuhi: I would just like to say something about the undetected binaries by making use of the correlation between single WN stars and those found in binaries by Hiltner and Schild, viz., that the line widths in the binaries were narrower than those in the supposedly single stars. In the case of HD 190918, the orbital separation was, I think, the largest of the WN binaries and it has the broadest lines among the binaries. Therefore, I would suggest that the 'undetected binaries' must be very widely spaced binaries indeed, according to this correlation, because the presently single stars, do indeed have much broader lines. Therefore, that would make them very wide binaries, with very low orbital velocities, and consequently we have not found them.

Sahade: I just wanted to say that the criteria that Virpi Niemela mentioned yesterday are powerful means to detect the binaries, in those cases in which it is very difficult to detect them by radial velocity variation.

Van Blerkom: One thing that has not been mentioned is that the mass exchange mechanism should never leave you with two Wolf-Rayet stars. Is it true Virpi Niemela found a binary system in which one star is WC and one star is WN?

Niemela: No, I just suspected it.

Smith: You know, there are two stars which are classified as WN6-C7 and WC7-N6, respectively. I remember that I said flippantly that it looked like a double exposure on the plate, and Virpi Niemela said, I do not know how equally flippantly, that may be it was a binary with a WN6 and a WC7 star.

Thomas: It would be very interesting if it turned out that way.

Smith: It would be rather surprising.

Underhill: There is one star, HD 45166, we have not really discussed; I think that we have mentioned it once. It is a Wolf-Rayet star, that was and was not, and now appears to be some sort of very hot Of type object, plus an A star i.e., a binary. I bring it up because on the low dispersion spectra, which Carol Anger had at Harvard, there were very definitely broad N and C complexes in the λ4600 region, and then it changed. The fact that it changed and how the spectrum varied, is being investigated by Sara Heap on some more modern spectra. I will not go into the details, but it is particularly interesting in understanding the significance of the material in the Wolf-Rayet atmosphere, that we did get these changes. It looks much more like an Of star at the moment than a Wolf-Rayet star, casually said. But I think, we are fundamentally dealing with a spectroscopic phenomenon, and a fairly small, maybe, change in density and extension of the velocity field is going to give us quite a different spectrum.

Thomas: I agree, it seems to be one of the most interesting points to push things in the direction which Lindsey Smith summarized. If I remember well, she said it shook her a little bit, to see the tendencies for some of the Of and other central stars of the planetary nebulae, not to differ much from their counterparts in the non-nebular cases. So I would personally be very much interested, as always looking for something which shakes what you are happy with, in this present sort of euphoric state where we think we understand things. If we can, through the planetary nebulae, introduce discord, either by having shown the ordinary stars really have small masses, or that the Of stars blend into the Wolf-Rayet stars in the planetary nebulae, that would be very nice.

Conti: I would like to make a comment on my paper, where I distinguished between a Wolf-Rayet star and an Of star by the fact that the latter had absorption lines which were not violet-displaced. And so, I would ask Anne Underhill, if, by her comment on HD 45166, she means that now you see absorption lines or whether it had to do something about the emission lines widths or strengths?

Underhill: I think the original classification was on strengths and widths of the emission lines. They were objective prism spectra, and my suspicion is that Miss Anger really did not see the absorption lines. The hydrogen absorption lines that are present, are indeed A-type hydrogen lines, like A0, i.e., broad and diffuse. They may very well not have appeared on the objective prism plates. Now things look sharper; when you say the spectrum is an Of on the whole you mean you observe broad hydrogen lines and fairly sharp emission for all of the characteristic lines that you mentioned and no others. It is an interesting point, you see, that the star appears to be a binary, and as far as I remember, Sara Heap was telling me, you can make quite a good argument that one component is an A0 dwarf or just about on A main sequence. That gives you a visual absolute magnitude of 0 to -1, at the most. So, the companion that is producing the emission lines, if this is indeed the proper interpretation of the combined phenomenon we see, must have somewhat the same visual absolute magnitude, which pushes it down into the region of some central stars of the planetary nebulae. As far as I know, there is no particular sign of this star.

Van Blerkom: I would just like to make one request to people who are going to publish observational data, and that is: if usually equivalent widths and line widths, let us say half-maximum, are given, one very useful piece of data that could be given with no more effort, is the central intensity above the continuum of the lines that are observed. It would help a great deal, I think, in the theoretical interpretation.

Sahade: Anne Underhill was talking about ejection velocities when referring to the velocities derived from different atoms or ions. I think the expression is misleading; one should talk about the velocity of a certain layer, rather than the ejection velocity.

Underhill: No, I think one is justified in speaking of ejection velocities, in the sense that those velocities we measure are all greater than any estimated velocity of escape from the star.

Thomas: No, the point that Sahade is making is this. I have the photosphere layer out here, somewhere away from the star. That I observe C IV, with a velocity being produced here, C III with a velocity here and helium out here, does not mean this is an ejection velocity. This means that that is the velocity of the material at this point, and I may have no helium here, because it is all ionized, I can just have C IV. So, the final ejection velocity out here, is maybe 1000 kilometers a second. But the fluid motion down here is maybe only 200 kilometers a second for everything.

Underhill: Well, it is an outward-directed motion. Perhaps, I misled people; instead of saying the outward-directed component of motion, I just used one word, ejection. It appears to have led to a misunderstanding.

Sahade: They may not be increasing all the way out, you see.

Underhill: They may not be, no, but I really meant the outward-directed component of velocity.

Thomas: Bappu, do you want to make the remarks that you made to me in private about this point now?

Bappu: I thought Anne Underhill had covered it fairly sufficiently, but I think is is rather important to realize that if these violet-absorption edges can be measured for velocities, there are two possibilities that come through: it is obvious that the O VI ions have much lower violet-edge velocities than the He I atoms, and one infers from it, that perhaps it is indicative of a variation of velocity with the excitation conditions. On the other hand, parallel to this variation of excitation possibilities, there is the possibility of a variation in atomic mass, and you have the heavier ions, slower-moving and the lighter ions, faster-moving, a result of mass transfer, kinetic energy being converted into velocities. But I do think that such a possibility has to be kept in mind, rather than to make a rush for the ionization potential-velocity correlations.

Thomas: It is just interesting that the ionization and mass go the same way in this sense.

Kuhi: If indeed, that is what you are going to do, how do you explain the behaviour of the C III 5696 line? That was the one that I talked a little bit about with regard to the way it changes from a round-topped line in WC9 to a completely flat top line in WC5. How do you account for that?

Bappu: My feeling is that, depending upon the spectral sub-class, you have certain characteristic overall widths. Now, if you assume an extremely simple procedure, which I shall develop from core-expanding shells, that yield a set of individual profiles (Bappu, M. K. V. and Menzel, D. H., *Astrophys. J.* **119**, 503, 1954), the net profile which is the summation of the individual contributions gives you the observed profile. In other words, I visualize that you have a series of concentric shells of different velocities. The picture that you get finally is defined by the extent of the limits of the final velocity and the initial velocity. Once you bring this initial velocity close enough to zero your extent of the flat-top gets close enough to zero, and, therefore, you can build up almost any profile which looks like a Gaussian by having a lower limit of velocity which is close to zero. And in a higher excitation star like HD 165763, your velocities are much larger.

Kuhi: I do not understand that.

Underhill: I think that part of the problem is C III 5696. In the case of the WC8 Campbell's hydrogen envelope star, it is sort of at maximum excitation. We get from that star what we think is a low excitation spectrum on the whole and, therefore, the major part, which I call the dense part, probably is forming most of that C III. There would be some drifting out. That can exist over certain temperatures; it depends on the rate of cooling outwards. So, whether it is quite flat-topped or not, I am not sure. I have a feeling that it has a small flat-top. What is the difference between that and a slightly round one? Because in the early WC's, you get in the very broad lines the flat-top, but then the only time that you have C ions around, so that you can get C III 5696, would be in the outer regions, where the temperature-drop inside would be such that the ionization ratio did not favour production of C III lines. So, although your line-of-sight, in principle, integrates through the whole atmosphere, it would pick up nothing from the inner part. The only part it picks up is from the exosphere. The exosphere, by definition of being an exosphere, has this thin shell of outward-directed velocity, which is the geometrical construction one requires to get a flat-top line. I think what you saw, what you demonstrated, was a nice demonstration of facts, and perfectly straight forward.

Paczyński: It was mentioned here a number of times that perhaps the observed different velocity of outflow of different elements might be due to different flow velocities. One can verify such a hypothesis by means of very simple calculations. If we are going to have the different velocities of outflow for different elements then the density of the medium must be small, so that the flow with different velocities is possible. We have to produce the emission lines in the same region. As the density is low and the observed emission lines are strong we need a very large volume and mass of the line-emitting region. It is very difficult to understand how, under so low density conditions the absorption edges can be formed. I am sure that we have a continuous flow, and the different emission lines show different expansion velocities because they are formed in different regions.

Underhill: That is very likely, so that they are formed in points above where the absorption lines are, and you can only see a few and those few are the sort that come from levels that would be strongly populated, in a low density, low radiation, standard dilution effective. You do not see all of the lines, even if they have large *f*-values.

Van Blerkom: If the molecular weight determines the velocity, then I do not see how you can get rapidly moving He I and slowly moving He II.

Underhill: I do not think you do. I think it is a question you can only observe He II when the temperature and density are such that you have enough atoms around sitting in the level with $n = 4$ to give you the absorption line. If you do not have enough, you do not see it.

Kuhi: A point that I think is really important, concerning this last discussion, namely Anne Underhill's and Bappu's approximate correlation with the square root of one over the molecular weight, and that is this: there is no doubt that there is a good correlation between the line widths, say, of N V, N IV and N III, in the sense that N V is narrower than N III. Now, the velocity that you assign also includes a temperature term; it goes as the square root of the temperature over the mean molecular weight. Therefore, if you are looking at a region of high temperature, the velocity should be high; therefore, the velocity that you observe for N V should be large and the velocity for N III should be small, i.e. the correlation goes the wrong way from what is observed. Therefore, the hypothesis warrants no further discussion!

Smith: And the absorption lines go the same way as the line widths, so, the N V absorption lines have lower velocities than the N IV, and this is the best and longest observed correlation in the history of Wolf-Rayet stars.

Thomas: I am glad for the details, but the main thing is somehow we must make sense out of this point Paczynski raised. How do you get it out?

Underhill: That point may come under discussion at the IAU Colloquium which is going to be held at Goddard in February. Stellar Chromospheres is the name, purposely chosen to cover all sorts of highly excited outer atmospheres.

Thomas: I, personally, have gotten an enormous amount out of this Symposium; I have learned very much from it. The biggest thing I have learned is that when I came here I was very worried that this was only a couple of years since we had the Wolf-Rayet Symposium in Boulder, and I worried that if we concentrated mainly on the Wolf-Rayet stars, with only side aspects of other things, it would be a re-hash and repetition. I had thought that maybe what one should do is extend it to the hot stars with just casual, passing references to the Wolf-Rayet stars. I am delighted to have been so wrong, both in terms of lots of the things that I thought I knew about what the Wolf-Rayet stars were, and in terms of the organization of the Symposium. It is very clear that what we got from Boulder, was a look at a subject which had been lying dormant, but was slightly stirring in recent years. Problems were posed, uncertainties were raised, and lots of interesting questions were thrown open. Then, in the years since then, people have worked actively on these problems, and have come up with lots of ideas which they brought here. In the course of this week's discussion, a lot of these ideas which have jelled to a very considerable extent since Boulder, jelled even more. I keep hearing from all of you as you stood up: "Well, I had a thought and now, in the last few days, it has changed", even Anne Underhill, who told me when she came in, she had to work very hard to write her summary before the meeting started, in terms of what people should better say, or, if they had not, she was going to fix it. But, what she said today, had no semblance with what she told me she was going to say.

So, it seems to me that this Symposium has taught us two things: one, how to think in terms of what you ought to have, in terms of what the results were, and, second, how to organize the thing. One, the specific developments on the Wolf-Rayet and the hot stars, and second, the lesson on how to organize such a symposium as this. On the Wolf-Rayet atmosphere, I break it into three parts, in my usual attempt to over-simplify, the bottom, the middle, the top. And you will forgive me if this is highly abstracted. At the bottom of that atmosphere, we worry about T_e, and that is why I was trying to push Paczynski and Lindsey Smith so much in the first few days, because it seems to me, that what you have done here is to talk in terms of stellar models and stellar mass ejection and the like, as a basis for specifying what the effective temperature is. And then, in the middle, what we really had from all the things that Kuhi, and the very good summary that Van Blerkom gave about profiles, is a real resolution of this stratification program. I am delighted to have been so wrong in what I had thought before, not only in details, but in terms of general philosophy. We have the ionization direction and the velocity direction all fixed now, finally and unambiguously. If you remember I keep saying one should use the Sun and the Wolf-Rayet stars as extreme examples of stellar chromospheres; now, of course, the point from all of this is that you should use the Wolf-Rayet stars as an extreme example of what I call corona and exosphere, rather than a chromosphere. So, for how you use the Wolf-Rayet stars, in addition to what they are, that is very interesting.

And then the lower middle atmosphere is something which we have not debated, a region where we must have a temperature rise from the photosphere, if any of these conclusions are correct, from something like $T_e = 10^4$ K in the photosphere to $T_e = 10^5$ K in the corona. So, here, we have, literally just zero empirical data on the details of the temperature rise. It is interesting here, if one does indeed have such a rapid rise it may well be that the reason that I do not see it, is that I have such a small atmospheric extent for the region of the rise. So again one stresses the rocket ultraviolet where maybe I have enough optical depths to probe this region. Now, then, at the top of the atmosphere occurs the mass-loss problem, and we have just had a debate on that, so there is no point in going into it again. The big point here is: can I produce it by radiation pressure? Can I produce it by the boiling off processes. What is it? And I just think that Paczyński summarized it so well the other day that there is no point in belaboring it: namely, we do not know.

And so, it is a problem to do much thinking on. I do think that my old idea about the Sun and the Wolf-Rayet stars representing the extreme range in the non-classical atmospheres, is reinforced. So that one again looks at the Wolf-Rayet stars and the Sun as extreme examples, aiming always at getting a model atmosphere which applies to all stars. I make you a wager, now, that that kind of a model behaves like the following. I have a photosphere, then I have a temperature drop, the decrease in the temperature coming in the standard LTE model, then there is the rise, coming not from the mechanical energy we put in, but the change in what fixes the temperature from a collision-dominated quantitative radiation field to a photo-ionization dominated qualitative radiation field. And again I emphasize, go look at what John Liebecker, Bob Stein and others are doing now, because they conclude you do not really get mechanical heating, you do not get the filter action coming, until you get the temperature upturn. I am personally, trying to establish the thesis, that this upturn comes independently of any heating, the heating only comes higher after the upturn, and you then have a very rapid rise of heating, as is empirically shown in the Sun and seems to be empirically shown, by default, here in the Wolf-Rayet stars.

Smith: If you have any temperature drop above the photosphere, would you predict that we would see some absorption lines?

Thomas: What you see depends very much on the optical depth. And that is why I like to use the Sun, the Wolf-Rayet stars, and all things in between, to map a range of the kind of things you ought to see, but not really need an empirical guide to develop the theory.

Smith: I understood that the only way you could get the observed spectra, with no absorption lines, is to have a situation where, in the continuum you see only to the temperature minimum, not beyond, being different from the situation in the Sun, where we do see beyond the temperature minimum and, therefore, we do see absorption lines.

Thomas: I can get absorption lines in an atmosphere, but in a standard non-LTE way; it depends on the kind of line. So, the presence or absence of absorption lines, by itself, does not prove anything. Your point is correct in principle, even if I quibble about the details a little bit. What one has to do, is to have indicators which map all this behaviour out. For example, you can show, by computing on a pure hydrogen atmosphere, that you can get a steeper initial temperature fall than in the LTE case before the other non-LTE things take over; then you get the T_e rise. But your point is absolutely right in principle, you just have to map it. That is why I like the idea of using all the ranges of conditions given by considering all stars in order to guide the theory. Well, I do not want to dwell on it; these are just pure guesses. If you miss on one thing, start guessing again, that is the important thing, And so, I am delighted to have been wrong in the direction of the stratification, but this leads to the suggestion that most of the observed atmosphere is a corona rather than a chromosphere. I would like to push the corona-exosphere as focussing on the thinking that Paczyński and Lindsey Smith did. If we can resolve this mass loss, it gives us again a good picture of the stellar atmosphere. Now, so much for the specific details.

Let me come to this organization business, on which I think I learned much from. We had a couple of Symposia on stellar atmospheres in a row, Varena and, then, Nice. The idea was that this was the time to go into aerodynamic phenomena in the stellar atmosphere, after having considered aerodynamic phenomena in the interstellar medium, because atmospheres exhibited the aerodynamic effects at least as well as did the interstellar medium, and at least as mysteriously. But the way we did it was to say, let us look at the whole variety of aspects in two succeeding symposia; at the first one, look at three or four aspects, on the second one, look at another three or four aspects. You have a little bit of overlap, but not the whole two symposia being devoted to the same things. What you taught me here was, let us have two colloquia in succession, and reexamine the same problems.

Pick a subject, get some people together, summarize it, and look at it from all aspects. Go home and work, two or three years, or, better, do not fix it in terms of time scale, but in terms of productivity. Come back, do exactly the same thing as you did before, invite as many of the same people as you can get plus those other people whom you have drawn in by your frenzied activity in the field. Have exactly the same symposium again, and ask your progress. But look at the possibilities of getting other people who do not have resources, of whom nobody has heard of yet; those are the people you want to attract to these meetings. And, then, third, the idea of having large-scale summer institutes (or winter, I do not know wihch is winter or summer these days) but where you have lots of people from within your own locality, and 20 % from outside. We ran a symposium in Yalta a couple of years ago. We had 125 people there, bigger than normal, of them, about 35 were from the outside, 100 were from within Russia, who had never been any place, for one reason or another. It was very useful for them, simply from the standpoint of finding people whom they had seen before to argue with, to discuss concepts and argue.

I am sorry to hammer on these points, I realize it is a cracked phonograph record, but I do think it pays off, as is exemplified by the meeting here. It is small and it would have been nice to have had some more money to bring some more people in from outside, to talk especially, to the people of Buenos Aires. So, Sahade and Bappu, I am delighted to have been wrong on so many things, and so grateful to you for having had this thing for us here.